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PAPERS AND PROCEEDINGS

OF THE

Sixty-seventh Annual Meeting

OF THE

AMERICAN ECONOMIC ASSOCIATION

Detroit, Michigan, December 28-30, 1954

Edited by James Washington Bell, Secretary of the Association

and

Gertrude Tait, Executive Assistant

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PROGRAM OF THE SIXTY-SEVENTH ANNUAL MEETING OF THE AMERICAN ECONOMIC ASSOCIATION

Detroit, Michigan, December 28-30, 1954

No central theme has been sought for in the program this year. The attempt has been rather to provide opportunity for presenting results of current research, with preference in selection of papers given to the younger scholars who have not participated actively in the program in recent years.

Tuesday, December 28, 1954

10:00 A.M.

Meeting of the Executive Committee

12:30 P.M.

Luncheon Meeting of the Executive Committee

2:30 P.M.

International Flow of Economic Ideas

Chairman: V. W. BLADEN, University of Toronto

Papers: T. W. HUTCHISON, University of London; JOSEPH DORFMAN, Columbia University

Discussion: J. M. LETICHE, University of California; G. H. HILDEBRAND, University of California at Los Angeles

Long-term Trends in Capital Formation and Financing (joint session with the American Finance Associaiton)[1]

Chairman: J. J. O'LEARY, Life Insurance Association of America

Papers: A. S. TOSTLEBE, Wooster College; SERGEI DOBROVOLSKY, Wayne University; M. J. ULMER, American University

Discussion: G. T. CONKLIN, Guardian Life Insurance Company; ELI SHAPIRO, Massachusetts Institute of Technology

Regional Economics

Chairman: WALTER ISARD, Massachusetts Institute of Technology

Papers: J. V. KRUTILLA, Tennessee Valley Authority; F. T. MOORE, RAND Corporation

Discussion: PHILIP NEFF, University of California at Los Angeles; LEON MOSES, Harvard University; F. A. HANNA, Duke University

8:30 P.M.

Concepts of Competition and Monopoly

Chairman: CLAIR WILCOX, Swarthmore College

Papers: J. M. CLARK, Columbia University; SIDNEY WEINTRAUB, University of Pennsylvania

Discussion: FRITZ MACHLUP, Johns Hopkins University; KERMIT GORDON, Williams College; GARDNER ACKLEY, University of Michigan

Current Problems in Agricultural Economics

Chairman T. W. SCHULTZ, University of Chicago

Papers: E. O. HEADY, Iowa State College; MARTIN BRONFENBRENNER, University of Wisconsin

Discussion: D. G. JOHNSON, University of Chicago; D. R. KALDOR, Iowa State College

Models of Economic Growth (joint session with the Econometric Society)

Chairman: R. W. GOLDSMITH, National Bureau of Economic Research

Papers: STEFAN VALAVANIS-VAIL, University of Michigan; JOHN POWERS, Stanford University

Discussion: E. D. DOMAR, Johns Hopkins University; T. C. SCHELLING, Yale University

Pricing in Transportation and Public Utilities

Chairman: M. L. FAIR, Tulane University

Papers: W. S. VICKREY, Columbia University; F. K. EDWARDS, National Coal Association

Discussion: E. W. CLEMENS, University of Maryland; J. C. NELSON, Washington State College

[1] To be published in the *Journal* of the American Finance Association.

Wednesday, December 29, 1954

9:30 A.M.

Development Policy in Underdeveloped Countries
Chairman: MAX MILLIKAN, Massachusetts Institute of Technology
Papers: H. B. CHENERY, Stanford University; W. H. NICHOLLS, Vanderbilt University
Discussion: E. E. HAGEN, Massachusetts Institute of Technology; WILFRED MALENBAUM, Massachusetts Institute of Technology

Economic Stabilization, Forecasting, and the Political Process
Chairman: ROY BLOUGH, United Nations
Papers: R. C. TURNER, Indiana University; S. K. BAILEY, Princeton University
Discussion: SOLOMON BARKIN, Textile Workers Union of America; H. R. BOWEN, Williams College; R. A. LESTER, Princeton University; T. G. MACGOWAN,[2] Firestone Tire and Rubber Company

The Impact of Antitrust Laws
Chairman: R. B. HEFLEBOWER, Northwestern University
Papers: S. N. WHITNEY, Twentieth Century Fund; J. W. MCKIE, Vanderbilt University; LOUIS MARENGO, Washington, D.C.
Discussion: WALTER ADAMS, Michigan State College; S. M. LOESCHER, Indiana University; J. H. STAUSS, Grinnell College

12:30 P.M.

Luncheon Meeting (joint session with the American Finance Association)[1]
Speaker: T. O. YNTEMA, Ford Motor Company

2:30 P.M.

Economic Development: Case Studies
Chairman: ARTHUR SMITHIES, Harvard University
Papers: J. H. YOUNG, Yale University; MARVIN FRANKEL, University of Illinois
Discussion: D. H. STEINTHORSON, Bank of Canada; DUANE EVANS, Bureau of Labor Statistics

Economic Research and Public Policy
Chairman: GERHARD COLM, National Planning Association
Papers: J. A. LIVINGSTON, *Philadelphia Bulletin*
Discussion: EWAN CLAGUE, Bureau of Labor Statistics; G. W. ENSLEY, Joint Committee on the Economic Report; STANLEY LEBERGOTT, Bureau of the Budget; BERT SEIDMAN, American Federation of Labor; M. A. WATSON, National Shoe Manufacturers Association

Taxation and Income Distribution
Chairman: H. M. GROVES, University of Wisconsin
Papers: D. M. HOLLAND, National Bureau of Economic Research; P. J. STRAYER, Princeton University
Discussion: W. W. HELLER, University of Minnesota; R. E. SLITOR, Washington, D.C.

8:30 P.M.

Presidential Address
Chairman: I. L. SHARFMAN, University of Michigan
Paper: SIMON KUZNETS,[3] Johns Hopkins University

Thursday, December 30, 1954

9:30 A.M.

Urbanization and Industrialization of the Labor Force in a Developing Economy (joint session with the Industrial Relations Research Association)
Chairman: L. G. REYNOLDS, Yale University
Papers: W. E. MOORE, Princeton University; B. F. HOSELITZ, University of Chicago
Discussion: G. B. BALDWIN, Massachusetts Institute of Technology; W. H. KNOWLES, Michigan State College; SIMON ROTTENBERG, University of Chicago

Cyclical Experience in the Postwar Period
Chairman: GARDNER ACKLEY, University of Michigan
Papers: RENDIGS FELS, Vanderbilt University; K. D. ROOSE, Oberlin College
Discussion: JOHN LINTNER, Harvard University; DANIEL HAMBERG, University of Maryland; E. C. HALD, University of Washington

Research on the Business Firm
Chairman: H. R. BOWEN, Williams College
Papers: EDITH PENROSE, Johns Hopkins University; C. A. HICKMAN, North Carolina State College
Discussion: ALBERT LAUTERBACH, Sarah Lawrence College; W. W. COOPER, Carnegie Institute of Technology

[2] No manuscript received.
[3] Published in the March, 1955, issue of the *American Economic Review*.

2:30 P.M.

Long-term Trends in International Trade

Chairman: FRITZ MACHLUP, Johns Hopkins University

Papers: R. E. BALDWIN, Harvard University; H. G. AUBREY, Federal Reserve Bank of New York

Discussion: C. P. KINDLEBERGER, Massachusetts Institute of Technology; THEODORE MORGAN, University of Wisconsin; A. C. HARBERGER, University of Chicago

Price and Wage Flexibility (joint session with the Industrial Relations Research Association)

Chairman: R. T. BOWMAN, University of Pennsylvania

Papers: J. M. BLAIR, Federal Trade Commission; K. E. POOLE, Northwestern University

Discussion: GIDEON ROSENBLUTH, Queen's University; F. C. PIERSON, Swarthmore College

Debt Management and Monetary Policy

Chairman: L. H. SELTZER, Wayne University

Papers: M. A. ROBINSON, Dartmouth College; C. R. YOUNGDAHL, Federal Reserve Board

Discussion: A. G. HART, Columbia University; E. R. ROLPH, University of California

5:00 P.M.

Business Meeting

6:00 P.M.

Dinner Meeting of the Executive Committee

THE purpose of the American Economic Association, according to its charter, is the encouragement of economic research, the issue of publications on economic subjects, and the encouragement of perfect freedom of economic discussion. The Association as such takes no partisan attitude, nor does it commit its members to any position on practical economic questions. It is the organ of no party, sect, or institution. Persons of all shades of economic opinion are found among its members, and widely different issues are given a hearing in its annual meetings and through its publications. The Association, therefore, assumes no responsibility for the opinions expressed by those who participate in its meetings. Needless to say, the papers presented are the personal opinions of the authors and do not commit the organizations or institutions with which they are associated.

JAMES WASHINGTON BELL
Secretary

INTERNATIONAL FLOW OF ECONOMIC IDEAS

INSULARITY AND COSMOPOLITANISM IN ECONOMIC IDEAS, 1870-1914

By T. W. HUTCHISON[1]
University of London

I

The activities of economists—what they do and how and why they do it—are of at least as much interest and importance as the activities of the people economists themselves study: businessmen, housewives, bankers, finance ministers, and trade-union leaders. Although economists may—for better or for worse—not always have the effects on the politico-economic world around them which they intend or desire to have, they do directly and indirectly, often after long lags and much "friction," exercise some extremely important effects on various kinds of economic processes. A complete explanation of many types of economic processes cannot be given unless this explanation, or the model being used, takes account of the tides, gusts, streams, and flows of doctrines, opinions, and prejudices emanating from economists. So the study of economic processes must include the study of economists, or of the origin, flow, and development of their ideas—for one can hardly separate the study of the origin of ideas from that of the change and flow of ideas. Such a study includes as well as all the processes of changes in ideas, and how ideas originate, also the processes by which they succeed, catch on, and dominate their time; it includes also the lags and impediments in the flow of ideas, both across and within national and linguistic frontiers, and the questions of whether and in what sense there can be said to be progress in the flow of economic ideas, or whether there is a circular or pendulum movement, and how far this movement is the result to some extent of fashion —since the adjective "fashionable," in a faintly pejorative sense, is one of the most frequently encountered adjectives in our economic journals. All such questions lie in a vast field covered as yet only, on the one hand, by fairly casual unsystematized individual observations and, on the other hand, by a few wildly oversimplified and overcomprehensive generalizations (for example, the cruder sorts of Marxist generali-

[1] I am much indebted to Professor G. J. Stigler for valuable help in the conception and execution of this paper, though, of course, he does not bear any responsibility for its contents.

zation). It is in a minute subsection of this vast, amorphous, uncategorised field that this paper and this section of the Association's program, "The International Flow of Economic Ideas," belongs. It is very tempting to continue this general stage setting: indeed it would be easy to go on with the stage setting so long that there would be no time for the little play I am supposed to be putting on. But I must turn now to my allotted task and to the state of economic ideas and the international flow thereof, round about 1870.

II

In reviewing the international flow of economic ideas between 1870 and 1914, I must, from limitations of space and knowledge, take a rather Anglo-centric line, and consider primarily the flow into and propagation in Britain of economic ideas from abroad, leaving out movements and conflicts between other European centers: for example, between Austria and Lausanne and between Austria and Germany. The story seems to fall fairly naturally into two or three main phases. The first phase, in which Jevons is the outstanding figure, is that of the seventies when we have the discovery, for British economists especially, of the great French and German predecessors of marginal analysis—Cournot, Dupuit, von Thünen, and Gossen—some twenty, thirty, or forty years after the first publication of their works. At the same time we have also the contemporary interchange between Jevons and Walras. After something of a lull in the eighties, towards the end of which the ideas of the leading Austrian economists first became widely known in England, we reach in the nineties the high point in our period of the cosmopolitan interchange and development of economic ideas, notably concerning the theory of the consumer and the theory of marginal productivity. In this international interchange of the nineties English economists play a certain part, but not relatively a very large or leading part. Then in the final phase, from the nineties down to 1914, contemporary with the predominance of Marshall and his school, the flow of economic ideas into Britain from abroad, and their acceptance and appreciation, dries up very considerably, although there were still, all the time, many new ideas and refinements being advanced in Lausanne, Italy, Austria, and Germany, notably on the subjects of the consumer, welfare, interest, money, and business cycles.

The marginal utility theory of value was not, of course, discovered in 1871-74. Both the component ideas (especially that of the relation between utility and value but also that of the marginal concept, as well as the combination of the two ideas in the marginal utility theory of value) had had a number of previous expounders. The years 1871

to 1874 are simply the time from which hindsightedly and *ex post* we can recognize one of the major fundamental turning points in economic ideas: first, there follows the widespread application of the marginal concept especially to microeconomic maximization formulae; secondly—and this applies to Britain only—we can date in 1871 the end of roughly a century of that perhaps slightly overconfident, doctrinaire, and distorted Anglo-classical overemphasis on the role of labor and cost of production in value; and we have something of a return back to the mainstream of balanced European thinking about value since Aristotle, in which a full and explicit role for the demand side had never been long or seriously left out of account.

The three men of 1871-74 were not reacting to, or stimulated by, any common cosmopolitan intellectual situation brought about by an active international interchange of ideas about the problem of value (or any other economic problem). In fact, for several decades there had been hardly a trickle, much less a flow, of economic ideas into Britain from elsewhere, and of course the internal flow of ideas in Britain itself had almost completely dried up. In the judgment of the most orthodox and overwhelmingly influential authorities, the subject of value had been settled once and for all. I think it can be said that the more narrowly economic theories in Mill's *Principles of Political Economy* show hardly any positive traces of any idea from any French, Italian, German, or Austrian source, except in regard to J. B. Say's theory of markets (and certainly *not* in regard to Say's quite sensible treatment of value and distribution). Though J. E. Cairnes wrote a critical essay on Bastiat, from his *Leading Principles* (1874) it appears that pretty well no one outside Britain had ever had an idea on the central doctrines of economic theory that was worth mentioning (apart, again, from Say's theory of markets). Senior had been to some extent a notable exception. He had studied Hermann's work and had derived from it at least a certain confirmation or encouragement in his departure from some of the main doctrines of Ricardian orthodoxy. If, of course, J. S. Mill had had time in the rather brief periods he later devoted to political economy to study what Taussig described as "the incisive and original" work of Hermann, with its "high intellectual quality," he might have been moved to rethink his ideas about the wages-fund doctrine some thirty-seven years before he eventually did so. Of course some of the "underground" English utility theorists such as Banfield, and perhaps Jennings, had learnt from European economists. In fact, to call attention to the work of Hermann and Rossi (J. B. Say's successor in Paris) had been the proclaimed primary intention of Banfield's lectures at Cambridge in 1844. But as regards the orthodox and overwhelmingly dominant treat-

ment of the fundamentals of economic theory, the position for a quarter of a century or more before 1871 could have been summed up in the celebrated London newspaper headline: "Fog in the English Channel, Continent Isolated." As Taussig put it: "The insular condition of social and political speculation in Great Britain in the middle of the century, and the stagnation of economic thought in particular, prevented a breath of influence from reaching English thinkers." (*Wages and Capital,* page 266.) Though this generalization may or may not be questionable as regards social and political ideas, it is surely well founded with regard to economic theories. We might, however, mention in passing one peculiar but vastly consequential case of the international flow of economic ideas that was taking place in mid-century in the British Museum library; that is, the discovery by Karl Marx of a framework for his economic doctrines in the works of Smith and Ricardo.

Thus with the international flow of economic ideas virtually at a standstill, the three men of 1871-74—and this applies to Menger and Walras as well as to Jevons—each reached his discovery by his own original and mainly national route. There is, therefore, quite a measure of coincidence in the almost simultaneous publication of their ideas by Jevons, Menger, and Walras, though the increasing occurrence of the isolated publications of the marginal utility idea, or of ideas pretty near it, made such a double or treble coincidence in dates almost inevitable sooner or later. And, of course, there is in any case nothing of a coincidence in the fact that this threefold, nearly simultaneous, publication, by three university professors, became recognizable in retrospect as that major turning point when enough force was finally generated gradually to get started a cumulative process sufficient to mark what can be seen now as the opening of a new period in economic thought. For such a major shift to get under way, at first very gradually, some such threefold spark was necessary. None of the numerous previous isolated individual flashes had been enough to get an ideological chain reaction or cumulative process started, or to get the torch burning sufficiently steadily so that it could not be neglected or quenched. As Walras proudly reported to his aged master Cournot in 1874: "M. Jevons et moi avons amené la question à un point tel qu'elle ne pourra bientôt plus être écartée par les économistes."

Let me develop rather further and qualify slightly this generalization about the separate national settings in which Jevons, Menger, and Walras developed their ideas. Jevons' marginal utility theory was the fruit of his own originality stimulated by his study of Lardner, Bentham, and perhaps Jennings. Through Lardner there came from France possibly some extremely diluted and selective traces of Cour-

not's monopoly analysis. Menger's theory was the product of his own originality building mainly on the works of Hermann, Schaeffle, and other Germans, including perhaps Mangoldt. And Walras was indebted to his father, to his father's classmate Cournot, and, of course, beyond that to the traditional current of French thought on value and distribution going back through J. B. Say to Turgot, Condillac, and Galiani. These extremely brief and familiar statements do really seem to summarize all that is worth summarizing, and it would be pointless, and perhaps even quite misleading for our purpose, to explore much further. Though there may well have been other particular institutional influences relatable to the ideas of Jevons or Menger or Walras, *separately,* we can only think of one *institutional* influence which may have been common to the background both of Jevons and Walras, and which was present in the first economic problem Jevons wrote about, that of the railways of New South Wales, at the same time as he was studying Lardner's book on *Railway Economy*. This institutional influence may at a later stage perhaps have to some extent acted on Walras through Dupuit, though Walras does not seem to have studied Dupuit till well on in the seventies and then not, at first at any rate, to have appreciated the significance of his work. This common institutional background influence was that of the growing problems, in the second half of the century, of the pricing policies of public utilities especially in transport, with their large fixed and comparatively small variable costs, where the divergencies between marginal and average cost and marginal and average revenue were so important and inescapable. For those who hanker after some "exogenous" explanation of the development of marginal analysis (that is, an explanation in terms of the problems and institutions of the contemporary economic world), there is this consolation prize, such as it is. But the discovery, and above all the subsequent elaboration, of marginal analysis is surely an example of a predominantly "endogenous," logical, intellectual development out of the internal theoretical problems of economic analysis itself, at a considerable level of abstraction. It has also been argued that the marginal utility theory was developed as a bourgeois counterblast to Marxian economic theory. Though subsequently Wicksteed, Böhm-Bawerk, and Pareto made some use of the subjective theory in this way, there does not seem to be even a grain of truth in this argument applicable to the original triumvirate of 1871-74, or to their motives, conscious or unconscious.

The increase in the international flow of economic ideas in the seventies—slight and qualified as compared with what came later but very marked as compared with what had gone before—may be traceable to two interconnected and mutually reinforcing factors. First,

there is the relatively much more abstract mathematical nature of the new ideas on value, and, secondly, there is the gradually increasing importance of the academic scholar in the world of economists and the increasingly professional intellectual status and standards of the pioneers of marginal analysis and of the whole neoclassical period. The marginal formulae were very easily susceptible to mathematical modes of expression; that is, to formulation in an international language. Obviously today *Econometrica* (though in some ways the least intelligible of our magazines) is clearly the most cosmopolitan and internationally or interlinguistically the most intelligible. Jevons' enthusiasm for the mathematical method may perhaps seem today a little quaint considering his comparatively very elementary use of it. But Marshall, of course, knew just what he meant and was making a significant comparison with previous economic theories when he wrote of the work of Carl Menger—ironically a virulent opponent of the mathematical method—that it, "though not making use of the mathematical language is distinctly mathematical in tone" (*Principles*, first edition, page 149). Secondly, with economics becoming increasingly a subject for university scholars, it became more widely regarded as a duty, if not universally followed as a practice, to explore and refer to work in other languages by way of a professional scholarly obligation not felt or fulfilled to the same extent by the often less academic political economists of preceding generations. The example of German scholarship was of some importance here.

We may take as marking the opening of this new international flow of economic ideas the moment in May, 1874, when Walras and Jevons exchanged letters, though Jevons had read Walras' work a short time before they corresponded. I cannot fix with precise certainty when it was that the works of Menger or of other Austrians were first noticed in England or outside Austria and Germany. In any case, Jevons never saw Menger's work, and certainly no considerable or wide attention was paid to any Austrian ideas in Britain till the second half of the eighties. But it was in 1879 in the Preface to the second edition of his *Theory of Political Economy* that Jevons set new standards of cosmopolitan scholarship and intellectual receptiveness, especially for British economists, by searching out with unprecedented conscientiousness all the works of predecessors and contemporaries which had anticipated or shared in his own new ideas. This Preface is, in its new standards of scholarly presentation, almost as outstanding and novel an achievement, in its way, as his ideas on marginal utility had been in the first edition. Of course there had some time before been McCulloch's historical and bibliographical work. But the bibliographical listing of all known writings on political economy and its various

branches is something quite different from the international documentation of a particular new idea, not only in its origins but in its contemporary treatments, such as Jevons achieved in his Preface. The former is largely an *ex post* record; the latter serves much more to create a contemporary international market in ideas and to make advances in the subject to some extent a process of international cooperation.

Cournot, Dupuit, Gossen, Léon and August Walras, von Thünen (and incidentally William Whewell, of Cambridge) are here all given, in some cases absolutely their first recognition by an English economist, in others certainly their first sympathetic and illuminating reference in English. Jevons threw open the windows on a whole vast new landscape of economic ideas, including the writings of one or two of those now recognized as among the most fundamental seminal pioneers in the history of the subject. Naturally Jevons himself did not, and could not reasonably have been expected to, take in anything like the full significance of the vast range of work of the half-dozen great economists he presents.

There is one detail here over which we should like to linger for a moment, and that is the slight mystery which seems to hang about Jevons' reference to the work which some economists might take today to be the greatest and most original of all pioneer achievements; that is, Cournot's *Recherches*. The question arises as to who was the first economist in Britain to appreciate something of the vast significance of Cournot's *Recherches*. Jevons says in his Preface (page xxix): "This work must occupy a remarkable position in the history of the subject. It is strange that it should have remained for me among Englishmen to discover its value." This statement seems as it stands to be inaccurate, and rather curiously inaccurate. Twenty to thirty years later, Marshall was to write to J. B. Clark that "I fancy I read Cournot in 1868," and Keynes says that the *Recherches* "must have come into his hands some time between 1867 and 1870." This of course gives Marshall a priority Jevons could not have known about, except—and it is curious that Jevons did not seem to have noticed this—that Marshall did make a single fleeting mention of the *Recherches* in his article, prominently published in the *Fortnightly Review* in 1876, on "Mr. Mill's Theory of Value." But it is certainly true that no one could have appreciated from Marshall's passing reference the vast significance Cournot's book was having both for Marshall's own work and for all subsequent economic thought. Another rather curious question is posed by Jevons' mention that the man who had put him on to the possible significance of Cournot was the mathematician Todhunter, who was, or had recently been, a fel-

low of St. Johns—Marshall's own college. Todhunter himself had not seen the *Recherches,* but in 1875 suggested its significance to Jevons, who had obtained a copy in 1872 without studying it carefully till some years later. Now it is ruled out that it was Marshall who put his fellow Johnian mathematician Todhunter on to Cournot, because Todhunter is quoted by Jevons as writing that "when I have mentioned the title, I never found any person who had read the book." Were these two fellow Johnians, both interested in the application of mathematics to economics, not on speaking terms? It is indeed an oddity for students of the flow of ideas, international or national, that the width of the high table at St. Johns College was, apparently, for a few years at any rate, a sufficient impediment to the propagation in Britain of the ideas of one of the outstandingly original and consequential writers in the history of economic thought. So here with regard to the discovery of Cournot in Britain is a further point of contention for protagonists—if there are any left—of the relative priorities and achievements of Jevons and Marshall. Anyhow, it is certain that Jevons first fully brought Cournot's work (as well as Dupuit's, Gossen's, von Thünen's, and Walras') prominently and inescapably to the attention of British economists. Marshall on the other hand had been quietly studying Cournot some years before, but before 1890 made one single fleeting reference in 1876 to the work which had, as Marshall himself put it, "directed the forms of my thought." We might add that it seems possible that the full significance of Cournot's work was first appreciated in America before it was in Britain. Anyhow, as Professor Dorfman has pointed out to me, Simon Newcomb in his review of Jevons' *Theory of Political Economy* in the *North American Review* for 1872, while warmly welcoming Jevons' work (in this respect being almost alone among Jevons' reviewers in English) nevertheless places it well below the supreme class which he considered Cournot's *Recherches* to occupy—which work was obviously thoroughly well known to and appreciated by him. Jevons gives no sign, as far as I know, of ever having seen this remarkable review of Newcomb's, though Newcomb is mentioned in Jevons' Preface to his second edition of the *Theory.*

The international flow of economic ideas set going by Jevons and Walras was followed by something of a lull in the eighties, which, at any rate in Britain, seems on the surface to have been rather a lean period, though it was, of course, in this decade that Marshall's *Principles* was being written and though Edgeworth's *Mathematical Psychics* of 1881 was soon to have an important international influence. The eighties are, however, the great decade of the original Austrian triumvirate, and towards the end of it their works—thanks

to Smart, Bonar, and others—became widely known in Britain and elsewhere. In the early nineties, Böhm-Bawerk's two volumes and Wieser's *Natural Value* appeared in English translations, as had Pantaleoni's *Pure Economics* in 1889, a work in which the Austrian influence was strong. So far as I have ascertained, these, with the possible addition of Cassel's *Nature and Necessity of Interest,* are the only major neoclassical translations into English in our period. (The Fisher edition of Cournot in 1898 is in rather a different category, as are the English editions of Marx.)

Towards the end of the eighties and through the nineties, we come to the peak decade in the flow and interchange of ideas in the neoclassical period. We have the development of the marginal productivity theory of distribution, which had of course its English and German predecessors but which was now perfected, refined, and widely accepted. We have also a new analysis of consumers' equilibrium and new debates over the theory of capital; and all these advances are carried through by a wide international exchange of ideas. In their writings of that decade or of a few years previously, Edgeworth, Wicksteed, Auspitz and Lieben, Wieser, Böhm-Bawerk, Wicksell, Walras, Pareto, Barone, and Fisher all drew on a broad, internationally known literature, made up to a large extent of their own writings. Works like Wicksell's *Value, Capital and Rent* and Pareto's *Cours,* though they contain their own particular original ideas, are constructed essentially on the basis of a wide, eclectic, cosmopolitan reading of their contemporaries and immediate predecessors. The marginal productivity theory was, therefore, developed in a very different setting to that in which the marginal utility pioneers of 1871 had worked out their ideas. We must omit the details of this decade, but we must call attention to one fact. Although Edgeworth and Wicksteed made very important contributions to the international development in the nineties of the theory of distribution and though to a considerable extent they followed Jevons in the international breadth of their reading and comprehension of contemporary European writings, patches of fog were again noticeable over the English Channel and the Continent was again partially isolated. There were important ideas, first broached in Europe in the nineties or soon after the turn of the century, on the subject of the theory of the consumer, of welfare or "ophelimity" economics, of interest, money, and business cycles, which were not to find a reception in Britain for the best part of two, three, or even four decades. I refer of course to what are now recognized as some of the most celebrated contributions to economics of Walras, Pareto, Auspitz and Lieben, Fisher, Wicksell, Spiethoff, Mises, and Schumpeter.

III

How did this reaction come about? After Jevons had, shortly before, opened up such great vistas in respect of French and German economic ideas, how and why was it that a so much more stay-at-home regime so soon supervened in Britain? We must first get an idea of the restricted extent of the flow of economic ideas from abroad after the early nineties. After 1893 there was, in the period we are concerned with, no further translation into English of any major neoclassical book of economic theory (except for Cassel's on *Interest*, already mentioned). One might have imagined that the establishment of the specialist *Economic Journal* in 1891 might have marked a renewed attempt to expand and maintain, in the tradition of Jevons' Preface, the flow of ideas from the Continent which, as we have seen, was then almost teeming with new developments in one branch or another of economic theory. Edgeworth, the editor, was certainly a greater master of languages, including the international language of mathematics, than Jevons had been, and kept in close, though not always very sympathetic, touch with Austrian, French, and Italian economists. But either for Edgeworth's own reasons or because he deferred to the judgment of his senior colleague, the *Economic Journal* in what it included and what it excluded (especially in what it excluded) cannot be said in our period to have contributed notably to the international flow of economic ideas. Looking through the volumes of the first twenty-five years, we find a single article a piece by each of the three leading Austrians, Menger's being on the subject of money, with certainly quite extensive reviews of the new works of Wieser and Böhm-Bawerk. But we find no article by Walras nor a single review of any work by him, though—as Professor Jaffé has shown me that his correspondence reveals—Walras was most keen that all or some of his work should appear in English and actually pressed it upon Edgeworth. No contribution from Barone appears, though according to a letter of his to Walras, which Professor Jaffé has kindly informed me of and copied out for me, he submitted an article on marginal productivity at Edgeworth's request only to have it rejected. "Ma note n'a pas le bonheur d'être agreé pas le pontiffe des économistes anglais, M. Marshall, et le pauvre M. Edgeworth s'est trouvé dans l'embarras!" Pareto contributes no article and he appears only once briefly as a reviewer in 1912 and once as a "reviewee"—as regards his *Cours* in 1896. There is a single famous article by Wicksell in 1907, which seems to have roused very little attention or understanding at the time, and when his *Lectures* were published in German in 1913 they were dealt with in a single twenty-line paragraph. It is true that when in the years just before the first

World War the business cycle became increasingly the subject of systematic study in Britain, some—often rather critical—notice was taken of the writings in French of such predecessors as Tugan-Baranovsky and Aftalion, though the ideas of Spiethoff and also of Schumpeter hardly received the attention which in subsequent decades they have been recognized as deserving.

How are we to explain these considerable limitations to the flow of economic ideas into Britain, this lack of interest in and receptiveness to many of the best ideas of Walras, Pareto, Wicksell and others—many of them ideas which in the decades since seem to have won out as among the best of their time? We can only try to indicate some of the broad considerations and factors.

By 1900, the superstructure of neoclassical marginal analysis was complete in its main outlines, and a considerable measure of unification and agreement had been fairly easily reached as to this superstructure of formulae. That part of the main outline where nothing like agreement or unification was attained and where these were, of course, far more difficult of attainment concerned basic philosophical or methodological questions: for example, regarding the mathematical method, and regarding the utility and social utility concepts. These differences regarding the utility concept still persisted, and they led to diverging analyses of the consumer and to some extent of welfare economics, which if not very different in their conclusions—they were in any case pretty well void of empirical content—were based on very different concepts and techniques. Separate analyses thus came into being, divided by a quagmire of philosophical-methodological conundrums and terminological dogmas over which no agreed bridge could be constructed. Here were issues certainly not capable of any easy, generally acceptable resolution by the precise international language and criteria of mathematical or quasi-mathematical analysis, such as had served to produce the more or less common agreed superstructure. Moreover, the spectacle of the recent Austro-German *Methodenstreit* was certainly not one which could arouse much hope of enlightenment through an international debate of the methodological issues at stake.

Moreover, at the opening of this century that curious intellectual-psychological-sociological phenomenon, the Academic School, was at the height of its influence, cohesion, and prestige, with its two outstanding examples, the Austrian and Marshallian Schools, which were also, of course, largely national schools. Though much of the superstructure of their systems could be and was presented in easily reconcilable terms, these "school" systems rested on tenaciously maintained, if not always energetically explored, terminological dogmas, con-

cepts, and philosophical presuppositions, which turned them somewhat into impermeable, monolithic, intellectual structures in the nature of ideologies, insusceptible to modification or adjustment by any piecemeal give-and-take of ideas, national or international. The Austrian School, though by no means monolithic in all its doctrines, was agreed as to its original leaders in its vigorous rejection of all forms of mathematical analysis and expression. Menger and Böhm-Bawerk even rejected such a fundamental mathematical notion as that of the mutual determination and interdependence of economic quantities, and held dogmatically to a sort of monocausal theorizing which prevented the intellectual bridges being built over which a mutual permeation of their system of thought with the systems of Marshall or Lausanne could have come about. Those Austrians who were sympathetic to mathematical methods or receptive to non-Austrian ideas, such as Auspitz and Lieben and Schumpeter, soon became more or less outcasts from the Austrian School proper.

On the other hand, the Marshallian School held both to a number of well-known but specifically Marshallian concepts and assumptions, as well as to a sort of attenuated Anglican utilitarianism, or "utility-arianism," which it was not considered worth while to explore beyond, argue over, or re-examine. In his recent review, Professor Sir Dennis Robertson recognized this topic "on the face of it as of some importance," but as one, he goes on, which "when I started to read economics at Cambridge in 1910, it was not, I think, fashionable among us to think much about. . . . To us, I think, it seemed a topic more suitable for discussion by Germans than by Englishmen. We thought we knew pretty well what sort of things we wanted to know about, and were glad enough to take the counsel given by Marshall himself near the beginning of the *Principles*, 'the less we concern ourselves with scholastic enquiries as to whether a certain consideration comes within the scope of economics the better.'" (*Utility and All That* [1951], page 14.) British economists under the leadership of Marshall who followed out this line of approach were naturally cut off from much interest in economic ideas and analysis, whether emanating from Germans or from others, which proceeded from different conceptions of "what sort of things we wanted to know about," or, for example, from different conceptions of the significance and legitimacy of the utility and welfare concepts.

After 1890, Marshall's *Principles* and his school of thought soon rose to a position of almost complete and exclusive domination in Britain, and the considerable limitations in the international flow of economic ideas as far as Britain is concerned are more or less closely

bound up with what Professor Pigou has called "the Marshallian dictatorship." I certainly do not wish to suggest anything in the nature of a black-and-white contrast between the generous cosmopolitanism of Jevons' Preface and the tendency to insularity or exclusiveness of the Marshallian school of thought. In fact, a wide and cosmopolitan exploration of French and German writings in his first few formative years as an economist, during which he had found his way to the works of Cournot and von Thünen, then scarcely known in Britain, had of course contributed centrally and essentially to Marshall's *Principles*. In fact, it was mainly through the *Principles* that Cournot's and Thünen's ideas became a generally recognized part of economic doctrine. But the Marshallian school of thought thereafter proceeded in a somewhat insular independence of the work of Walras, Pareto, Wicksell, and the Austrians, treating Cournot and Thünen as something of a once-and-for-all import which is was hardly necessary to follow up once the *Principles* had been launched.

The explanation of this attitude would seem to be somewhat as follows. The economic ideas which will readily flow internationally are the sort of precise abstract mathematical or quasi-mathematical formulae which Marshall considered to be a very small part of economics proper and in isolation apt to be misleading. As Keynes put it, "Marshall arrived very early at the point of view that the bare bones of economic theory are not worth much in themselves and do not carry one far in the direction of useful, practical, conclusions." The theoretical skeleton had to be covered, clothed, and brought to life by a complement of historical and descriptive economics which was inevitably subject to a considerable national relativism, particularly if the ultimate objective—the guidance of policy—was kept in view. Moreover, Marshall considered that a more or less adequate abstract analytical "organon," as he called it, had been created and, as Mr. Harrod has said, "his programme of work for them [his pupils] was to study the workings of the economic system in all their rich and varied detail with the aid of principle; it was a programme for the development of applied economics." (*The New Economics,* edited by S. Harris, page 65.) Theoretical innovations, imported or otherwise, were rather a waste of time; it was all in Marshall.

Moreover, Marshall wanted a system of economic principles which would obtain and hold the interest of the statesmen, civil servant, banker, and industrialist, expressed in a language they could understand and invoking familiar British names or authorities. His emphasis on the continuity of the national tradition in economics obviously had much to do with this ambition. An architect might in fact be basing his

plans on the latest revolutionary continental principles of construction, but in selling an architectural design to the Bank of England, at least till fairly recently, he would have had a much better chance of success if the novel constructional principles employed were concealed behind a homely, familiar façade in the style we know in Britain "as Bankers' Georgian" (in America, "Bankers' Colonial") than if exposed in some stark unfamiliar un-British construction in the style of the Bauhaus, or Corbusier. Walrasian and Paretian importations, with their mathematics and their foreign terminology of "ophelimities" and "technical coefficients," would simply have frightened off those who Marshall rightly or wrongly regarded as the prospective readers of his *Principles* and of the new *Economic Journal*.

It seems that it is on some such lines as these that the case for the insularity of the Marshallian school of thought would have to be made out. I would be delighted if someone with more understanding of that case than I have were to make it out more effectively.

IV

It can hardly be expected that a summary of conclusions will yield any very firm, precise, or novel points. However, we can see from our survey that just as, by and large, there will be more international exchange the more active national economies are, so the flow of ideas internationally became more active when in each of the national centers more new ideas were forthcoming. Again, just as graded or precisely standardized wheat will usually be traded internationally rather than loaves of bread, so the international exchange of ideas came about mainly in formulae of pure analysis easily susceptible to precisely standardized mathematical expression. We can see also that the development of economics as an academic discipline, carried on increasingly and mainly by university teachers, led, on the whole and with considerable and sometimes glaring exceptions, to the recognition of standards of scholarly presentation requiring a more extensive and systematic acquaintance with serious new work, whatever its language or country of origin. On the other hand, the emergence of powerful local academic schools, representing monolithic doctrinal systems, with their tenaciously held philosophical or methodological presuppositions, terminological dogmas and war cries, held up the flow of ideas between different centers, or at any rate prevented their mutual permeation, modification, and agreed unification. Finally, much depended on the personal intellectual tastes and psychology of the main leaders.

The problem today is very different. With the vastly increased number of translations and of widely circulating specialist journals,

including international journals, and with the increasingly mathematical character of advanced economic analysis, it seems, on the whole, very unlikely that good new ideas, whenever or wherever they do arise, will not have a reasonably fair chance of being heard and of making their way. I am speaking, of course, simply of the world this side of the Iron and Bamboo Curtains. The problem today is probably not whether there has just been published a *Recherches* of Cournot or an *Interest and Prices* of Wicksell, the vast significance of which will eventually come to be generally recognized and followed up by about the time of the 1990 meeting of this Association. The problem is probably not that there are Cournots among us whom we are failing to recognize but rather that Cournots are just not coming into existence at all. The question is whether by one of those typical paradoxes of improved organization and larger scale, which in the moral and social cosmos so often empty out babies with bath water, the very expansions and refinements of intellectual machinery which have removed former lags and impediments in the flow of ideas have not at the same time removed one of the former sources of original new ideas. In the period we reviewed, much that was new and vital came first from those whose main interests, or from those some of whose main interests, were outside economics (like Cournot and Dupuit and even to a different and lesser extent like Jevons and J. S. Mill), from intellectual frontier types, or from slightly crankish outsiders (like Hobson and Johannsen). Vastly increased specialization and the disappearance of independent incomes has to a considerable extent removed this kind of thinker, who was so fertile in the period we have been considering. Economists are now part, even often from their undergraduate years, of large, organized, internationally-linked academic machines, with their subjects closely organized and defined and their questions and categories ready formulated, and the problem that arises is whether at the same time a main source of stimulus to intellectual advance, even though it was one which almost inevitably operated often only after long time lags, may have disappeared. It is quite impossible to assess the answer to this sort of question of hypothetical opportunity cost, but perhaps the question is worth asking all the same. Adam Smith, though extolling the productivity of the division of labor in manufacturing industry, was very apprehensive as to some of its applications and results in other spheres.

Much, inevitably, has been said about Marshall in this paper; so let us conclude with a typical quotation from him, which bears on our point and on much else besides and which radiates wisdom like the expanding ripples from a stone dropped in a still pool: "The widening range of standardised methods [in science] tends generally to increase

the dependence of the creative mind on large capitalistic aid in obtaining scope for its activities. . . . The fact that Aristotle, Newton and Cuvier would have much to learn, if they should meet a mediocre student of modern science on his arrival in Hades, does not tell entirely on the side of the present age." (*Industry and Trade*, pages 242-243.)

THE ROLE OF THE GERMAN HISTORICAL SCHOOL IN AMERICAN ECONOMIC THOUGHT

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Just as American economic thought has affected developments abroad, so have foreign streams left an impress on the American scene. The British stream of ideas has been by far the dominant one. But then the American cultural heritage is predominantly British. This is especially true of the political and economic institutions—the basic conditioning forces. To unfold the highly complicated story of the naturalization of British economic thought on American soil would require much more than one session. Let me restrict my address to a more manageable foreign stream. Let me take the German Historical School which was especially influential in the seventies and eighties. In the broad flow of history, this movement was a contributory force to the renaissance in American intellectual life that accompanied America's great industrial revolution in the post-Civil War era.

As they envisaged the promise of America's industrialization, enlightened men were anxious to avoid the ills that disfigured England's factory and machine age and threatened the Anglo-American tradition of liberty. In an effort to cope with the pressing economic issues of the new era, progressive-minded economists began to question the adequacy of traditional, classical economic theory which dominated America in a vulgarized form. They sought ways not only to explain but also to fructify the economy and build up social wealth. They were concerned, not only with analysis, but—in an incipient way of course—with social engineering.

Technically the origins of the German Historical School go back at least to the middle of the nineteenth century, but it received a strong impetus from the intellectual revolution symbolized by the name of Darwin and the effort in all fields to assimilate the doctrine of evolution. So far as the Anglo-American world is concerned, it was a movement to do for economics what Sir Henry Maine did for English jurisprudence in correcting the overanalytic emphasis of the dominant Austinian School by an infusion of historical and comparative methods. This was not the old-fashioned history, hardly distinguishable from belles-lettres but the new history that was concerned with detailed, objective inquiry. It was in this atmosphere that the German Historical School makes its appearance in America.

We may begin with a formal event. In 1877, the Trustees of Columbia appointed the German-trained Richmond Mayo-Smith to develop economics on an inductive basis. They hoped he would follow in the footsteps of Continental European pioneers who "brought together . . . the . . . information . . . gathered by the statistical bureaus of the several governments, and have sought to infer . . . the system most favorable to industrial development, to growth in national wealth and to the fairest and most equal distribution of the rewards of labor."[1] Here was the formal acceptance of the German Historical School in the academic world.

The school—or more properly, members of the school—had come to the attention of Americans in various ways. There were, of course, the American students who had studied in Germany. Even before that movement took on large proportions, there was knowledge of the school through English and French periodicals which were widely read in the United States. There was also the deep interest of some American economists in statistics and therefore in the techniques developed by the Germans. For example, as early as 1863, Samuel B. Ruggles, as the official American delegate to the International Statistical Congress at Berlin, expressed his conviction that "abstract theories and historical traditions doubtless have their use and their proper place, but statistics are the very eyes of the statesman, enabling him to survey and scan with clear but comprehensive vision the whole structure and economy of the body politic." A few years later, at the next Congress, Ruggles met and was deeply impressed by Ernst Engel, head of the Royal Statistical Bureau of Prussia and one of the most famous economists of Europe. His Bureau was unique in that it included a "seminary" to train university graduates who qualified for admission to the higher branches of the civil service. In this way, governmental offices, like the Statistical Bureau itself, would become laboratories of political science. Engel was deeply interested in social problems. He visited England to study labor conditions in the most advanced industrial nation. He was one of the promoters of the organization of German economists in 1872—the Union for Social Politics—that apparently first gave prominence abroad to the Historical School. The organization espoused social reform along English precedents and was soon confronted by hostile critics, who nicknamed the members *Katheder-Sozialisten* (Socialists of the Chair). Rumors soon circulated abroad that the organization advocated socialism, supported breaking of labor contracts, and favored strikes.

The picture was obscured abroad by the general lack of knowledge of German economics. To overcome this difficulty in England, the

[1] Cited in Joseph Dorfman, "The Department of Economics," in *A History of the Faculty of Political Science of Columbia University* (New York, 1955), pp. 170-171.

Fortnightly Review published an article in 1873, written by Professor Gustav Cohn, on the state of German economics. Cohn pointed out that the Union was influenced by the widespread revolt in the learned community against the highly abstract reasoning of the eighteenth century with its emphasis on natural rights, state of nature, laws of nature. They insisted on the need to understand "the facts which preceded the modern state of society" before any reasonable conclusions concerning the future could be drawn. These economists were, he continued, affected by the success of the inductive method in the natural sciences and the progress of a philosophy which asserted the authority of society and the organic character of the state against the workings of extreme laissez faire.

Two years later, the same magazine published a review article that set off a controversy in the United States over the German Historical School. Wilhelm Roscher, the patriarch of the school, had just published *The History of German Political Economy*. Professor T. E. Cliffe Leslie of Queen's College, Belfast, in the review defended the school against the charge of being socialistic. In this essay he said:

> Man, in the eyes of the historical or realistic school, is not merely an "exchanging animal" . . . with a single unvarying interest, removed from all the real conditions of time and place—a personification of an abstraction; he is the actual . . . human being . . . history and surrounding circumstances have made him, with all his wants, passions, and infirmities.

He pointed out the wide range of the publications by members of the German Historical School and insisted that:

> A great diversity of opinion is to be found among the economists of this school . . . ; some being conservative and others liberal in their politics; but no revolutionary or socialist schemes have emanated from its most advanced Liberal rank. Their principal practical aims would excite little terror in England. Some legislation after the model of the English Factory Laws, some system of arbitration for the adjustment of disputes about wages, and the legalization of trade unions under certain conditions, are the main points in their practical program.

Leslie's essay attracted the attention of the *Commercial & Financial Chronicle* of New York. In a eulogistic editorial it declared that the new German school treated the forces that caused a nation to grow in wealth. Interest in the school was stimulated by another eulogy which appeared at the same time in a French organ, *Revue des Deux Mondes*. The article was written by the eminent Belgian economist, Émile de Laveleye. He fanned the flames of controversy by contending that the erroneous belief in natural laws strengthened the opposition to bimetallism and protectionism, both of which, he argued, were essential to business prosperity.

This brought the *Nation* into the arena, a journal which exercised the greatest influence on respectable opinion. The editor, E. L. Godkin, was a firm adherent to the creed of free trade and the gold standard. He wrote an editorial asserting that the new group of German economists

were erudite university teachers with little public experience and long accustomed to accepting militarism and bureaucracy. In fact, they were socialists. Immediately the *Nation* received protests from several of its prominent readers.

One defender of the school was W. F. Allen, of the University of Wisconsin, who had studied in Germany. He was an orthodox Ricardian in his foundations. He strongly opposed, however, the extreme laissez faire of the dominant economics. It is all very well, he said, to point out the socialistic tendencies and the disastrous effect of public poor relief, for example, but after all was it proper for the state to "suffer its citizens to die of starvation in open day"? ("Fawcett's Essays," in the *Nation*, September 25, 1872, page 204.) He was, therefore, in sympathy with a school which he felt accepted the foundation laid by Adam Smith but built a new structure on it.

The *Nation* conceded that the German economists were not socialists but insisted that they had been blinded by resentment against the incidental, wild speculation of the prosperous era, which unification had brought to Germany.

The following year the controversy was intensified by the attack of the French economist, Maurice Block, which appeared in translation in the *Penn Monthly*. In the name of the "scientific school," he denounced the German Historical School and Leslie as "empirics" seeking to substitute sentiment for principle and holding that "the state . . . should conduct everything, direct everything, decide everything." Leslie asked the *Penn Monthly* to reprint a small extract from his article, "On the Philosophical Method of Political Economy," which would show that far from being an opponent of the philosophical spirit, as Block had claimed, his object had been to explain what the philosophical method ought to be. The journal decided to reprint the essay in full, because "it . . . represents a real advance in the development of economic science."

In 1878, with the aid of Allen, John J. Lalor, a sound money and free trade journalist, brought out the English translation of Roscher's famous *Principles*. This was immediately attacked by William Graham Sumner, who presented the view of dominant orthodoxy. Sumner stated that the work was distinguished by its pervasive faith in the state. History and statistics could not be merged with "dogmatic economics" but were separate disciplines.

Nevertheless, interest in the school continued to grow. John Kells Ingram, of Trinity College, Dublin, gave an important address before the British Association for the Advancement of Science in 1878. Ingram condemned the extremely abstract method of current English economics and declared that the study of the economic phenomena of society

should, in accordance with the historical method, be systematically combined with that of the other aspects of social existence. He pointed out that the essentials of the method were presented by the Secretary of the Union for Social Politics. Adolf Held, a student of Engel, had published elaborate inductive studies in taxation. He warned against too abstract theorizing on tax incidence and against overestimating the income tax as the sole means of reaching the entire taxable capacity. At the moment, Held was attempting to meet the challenge of socialism by supporting an advanced program of social security that culminated in Bismarck's legislation of sickness, accident, invalidity, and old age insurance. Ingram presented Held's standpoint in his own words:

> 1. The new school . . . opposes . . . the view . . . that the production and acquisition of wealth by individuals is the single . . . object of human life; wealth, it regards as a means used by Humanity in its struggle towards moral ideas of life, and for the furtherance of universal culture
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> 2. It . . . seeks to understand present economic phenomena through the study of their historical development, and to ascertain them as accurately as possible through statistical investigations. It uses the knowledge of the nature of man's intellect and will for the rational explanation of economic facts, but does not construct those facts themselves out of one-sided assumptions respecting the nature of man.
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> 3. It . . . recognizes the right of the state to positive intervention in the economic relations of the community, for the support of the weak and the strengthening of public spirit. As the Political Economy of the last century applied itself chiefly to the liberation of the economic forces from antiquated and useless restrictions, so the new school specially meets the acknowledged need of new social arrangements, the need of social reform in opposition to social revolution on the one hand and to rigid *laissez-faire* on the other.
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> 4. It takes up, therefore, a less isolated position in relation to the other Moral and Political Sciences.

In a quiet way the school had already struck root. Already in 1875, Colonel Carroll D. Wright, chief of the Massachusetts Bureau of Statistics of Labor, had introduced Engel's law of consumption. He drew upon Engel's studies to make a comparison of the condition of the workmen in Massachusetts with that in other states and foreign countries. Wright went a step beyond Engel and concluded also "that the higher the income . . . the greater the saving, absolutely and proportionately." Thanks to Wright, the study of family budgets was developed which, whether in the form of "Engel tables" or "Engel curves" has become so pivotal in economic analysis in our day. Engel also influenced Wright's fundamental work on the structure for a price index which has provided the statistical substance for many far-reaching decisions, such as adjustment of wages to cost of living. Inspired by Engel's success in training statisticians, Wright successfully promoted among American colleges the establishment of courses in statistics.

The German influences were summed up by Wright himself. The new school, he exclaimed, "bids fair to include on its roll of pupils the men in all civilized lands who seek by legitimate means, and without revolution, the amelioration of unfavorable industrial and social relations." (*The Relation of Political Economy to the Labor Question*

[1882], page 13.) The rigidly orthodox promptly castigated Wright as being infected by the socialists of the chair who stress moral forces and ignore the primitive and more elementary force of self-interest. Wright, however, continued to plead for social reform, in terms of the idiom of the Historical School. Thus, while the United States Commissioner of Labor in 1893, he described the German social legislation as a brave attempt to reduce economic insecurity.

Equally receptive to the school was a scholar who was to become the country's foremost economist and statistician, General Francis A. Walker. He insisted that the classical economics, as formulated by John Elliot Cairnes, furnished the skeleton foundation for sound economics, but he questioned the wage-fund doctrine and supported international bimetallism. Walker castigated the reigning orthodox economics for its extreme conservatism. He observed their neglect of historical methods, and their overemphasis upon an a priori method which achieved a simplicity in classification to which the subject matter was not susceptible. These factors, he said, in 1879 had cost the science of economics public regard, especially among the laboring classes. As a remedy for this, the Historical School, it seemed to him, offered the greatest promise. "The economists of Germany, Italy, Belgium, and France," he wrote, "are doing the work which Adam Smith began, in his spirit, but with larger opportunities and a wider and ever widening view."[2]

The movement reached a high pitch in the eighties, especially under the impact of English writings, that opened up the field of economic history. Indicative of the temper of the times was the fact that the article on "Political Economy," in the ninth edition of the *Encyclopaedia Brittanica* (1885), was prepared by Ingram; and the *Encyclopaedia*, of course, was an authority here as well as in England. The essay was quickly reprinted here for use in college classes, largely through the efforts of F. W. Taussig.

The Historical School was scoring, again because an ever increasing number of college graduates were going to Germany for advanced work in the seventies and eighties. Germany had long been known as the land of scholarship, a land where, it was said, professors achieved a "degree of perfection . . . that astonishes the world." (F. W. Taussig, "College Graduates in Germany," in the *Nation*, April 2, 1885, page 275.) And it was pointed out in 1876 by Charles F. Dunbar, of Harvard, that the lead in economics had now passed from England and France to Germany.

The young men who went to Germany were imbued with a lively

[2] Walker, "The Present Standing of Political Economy," 1879; reprinted in Walker, *Discussions in Economics and Statistics*, Vol. I, edited by D. R. Dewey (New York, 1899), p. 318.

interest in statistics. A number who studied with Engel and his assistant, August Meitzen, at the Royal Statistical Bureau, achieved considerable prominence. Henry Carter Adams became the first statistician of the Interstate Commerce Commission and devised its pivotal accounting system which served as a model for the regulation of public utilities here and throughout the world. Roland P. Falkner was the first man to hold a leading American university post devoted exclusively to statistics, at the Wharton School of the University of Pennsylvania. He prepared the translation of Meitzen's *History, Theory, and Technique of Statistics*. His work on the famous Aldrich Report of 1893, *Wholesale Prices, Wages and Transportation,* was one of the landmarks in statistical investigation. Richmond Mayo-Smith became the outstanding teacher in statistics, and turned out such students as Walter F. Willcox and the inventor of the electrical tabulating machine, Herman Hollerith.

Another area which left a definite impress on American students was the systematic treatment of public finance or, as the Germans called it, the "Science of Finance," a subject which the English had neglected as comparatively unimportant. Interestingly, in line with this emphasis, the American pioneers in the field followed a German precedent in substituting the term economics for political economy, as the more comprehensive term. Political economy was restricted to private finance, or the domain of voluntary association. The term public finance covered the "wants of the state and the means by which they are supplied." Political economy and the science of finance became branches of economics. (See Joseph Dorfman, "Henry Carter Adams: Harmonizer of Liberty and Reform," introductory essay in Adams, *Relation of the State to Industrial Action and Economics and Jurisprudence* [New York, 1954], page 13.)

Students were also impressed by the scope of national economy covered in the massive works of such leaders as Roscher and Adolph Wagner. For example, comprehensive instruction was given, not only in principles, but also in agriculture (including forestry), transportation, commerce, manufactures, and finance. It was under this impact that Adams urged the need for a course on "American Technics" which would comprise the contributions of agriculture, manufacturing, and transportation.

To clarify the nature of the influence of the German Historical School, let me discuss specifically five German-trained economists who were the leading promoters and the first officers (along with Walker) of that landmark in the development of economic thought in America, the American Economic Association.

J. B. Clark (in "Unrecognized Forces in Political Economy," the

New Englander, October, 1877, page 712) warned that the assumed man of orthodoxy "is too mechanical and too selfish to correspond with the reality; he is actuated altogether too little by higher psychological forces."

E. J. James was more positive. He attracted attention through his contributions to the authoritative *Cyclopaedia of Political Science, Political Economy, and of the Political History of the United States* (1883-84). His articles ranged from the "History of Political Economy" and "Finance" to "Banks of Issue" and "Factory Laws." In the spirit of his German training, he declared that factory legislation could be justified, not only as protection for the helpless, but as an essential movement in the interest of society. To quote: "A state has other and nobler ends to follow than the accumulation of mere material wealth. . . . Moderate wealth and happy homes are better than a degraded proletary and ability to underbid all competitors in the industrial world."

Richard T. Ely was the most provocative of the group. He had not only studied in Germany but found it desirable to spend there a good part of each of the three years, 1911-13, in order to complete his most substantial book, *Property and Contract*. On his return in 1880 from his first trip to Germany he found academic openings scarce. Forced "to wander about the streets of New York . . . in a most wretched desperate state," he vowed to "use every opportunity to benefit those who suffer."[3] After a year and a half he obtained a foothold at Johns Hopkins. To aid in devising the best methods of carrying out proposed reforms and executing the laws, he lectured on the "Principles and Practices of Administration with Special Reference to Civil Service Problems and Municipal Reform." City planning, the mother of modern planning, likewise owes much to Ely. He was deeply impressed with the efficient administration of German cities by a permanent civil service which included the highest officials. A considerable advantage of a civil service, he wrote in 1880, was the permanence and steadiness of policy. Plans could be laid for a number of years and carried out gradually as a city could afford to execute them.

While still fresh at Johns Hopkins, he published widely read studies on the "new political economy." He granted that the older political economy had considerable merit. It separated wealth from the other social phenomena for special study. It showed the impossibility of understanding society without investigating the processes of the production and distribution of goods. In serving to pull down outworn institutions, it answered satisfactorily the needs of the latter part of the eighteenth century and the early part of the nineteenth century, but

[3] Ely to Labadie, August 14, 1885, in "The Ely-Labadie Letters," edited by Sidney Fine, *Michigan History*, March, 1952, p. 17.

like the French Revolution it was negative. Not least among the merits of the new view, Ely said, was that it gave a more concrete interpretation of economic history, by attempting to understand past doctrines in the light of their environment. He especially commended Wagner for his statement of the three ethical principles underlying economic policy. The first was the principle of individualism. This was modified by the social principle which acted through the state. Finally, there was the "caritative" principle—the principle of brotherly love expressed in voluntary action on behalf of others. Charity was only one form. The principle softened the rigors of life in ways that the social principle could not, for it was not obliged to operate according to fixed rules. Ely also noted Wagner's program for comprehensive social security that would eventually include protection against unemployment during hard times. Ely's emphasis, as early as 1882, on the acceptance of German social legislation, was prophetic. "It behooves . . . Americans to follow diligently the course of these experiments," he stated, "for we may be sure the same social problems which now vex Germany, will one day confront us." ("Bismarck's Plan for Insuring German Laborers," in the *International Review,* May, 1882, page 526.) To this end, he, along with Henry Farnam, later promoted the American Association for Labor Legislation.

Ely also pointed out that government regulation of industry provided the means of effectively applying the inductive method, for the legislation necessitated the systematic gathering and classification of data. He got John R. Commons and other students to prepare the famous *History of Labour in the United States.* In his widely used *Introduction to Political Economy,* Ely not only presented Engel's law, but called attention to Engel's basic objective "that it might be possible by a careful study of a sufficient number of family budgets for a period of years to construct a sort of *social signal service";* in other words, "that changes in total expenditures and in expenditures for various items in a sufficient number of typical families could enable one to predict the coming of industrial storms."

Henry Carter Adams, though he found the Germany of Bismarck's day an example of efficiency and enlightened reform, warned against the dangers to liberty implicit in indiscriminate state intervention. He admired the German methods of study and their skill at systematization, but he was disturbed by their worship of the state. This lay at the heart of his warning, in speaking of German university training. "It is not possible," said Adams, "for an instructor whose lectures are worth the hearing to separate himself from the peculiar influence of his time and immediate environment, and in Germany especially do the lectures one hears upon political sciences reflect the bias of German local and

national policy." ("Political Science in German Universities," in the *Michigan Alumnus,* January, 1899, pages 137-138.) On the other hand, Adams was also disturbed by the workings of unrestrained private enterprise in America, which in another century, he felt, would contradict the theory of freedom and destroy the government. "From this dilemma must arise," he said, "an American Political Economy—an Economy which is to be legal rather than industrial in its character." ("The Position of Socialism in the Historical Development of Political Economy," in *Penn Monthly,* April, 1879, page 294.) In this he was deeply impressed, as he wrote in 1878, with Wagner's view that the tendency in economic study was towards jurisprudence.

Adams noted the need for forest conservation, and he pointed out that corporations could not undertake such a task because the fruits of the investment were too remote. In the famous monograph, *Relation of the State to Industrial Action,* he declared that there were two important functions that the government could perform in the industrial area. First, the state could raise the ethical plane of competition. For example, factory legislation need not curtail competition, but it could remove serious abuses in the factory without eliminating the benefits of individual action. Second, the state may realize for the public good the benefits of monopoly. For this purpose, Adams developed the principle of "increasing returns" to cover industries which lend themselves to public control; for example, the railroads.

Government control, Adams argued, would not necessarily lead to corruption. Corruption was due to the lack of correlation between the public and private functions. The inducements offered in the two varied widely. Extension of the state's functions, manned by a well-paid civil service, would restore the harmony between state and private service, for it would bring social distinction, the chance to exercise one's talents, and the pleasure of filling well a responsible position.

Factory legislation and monopoly regulation, however, did not touch the problem of the rights and duties under which work is done. This brought Adams, in 1886, to the need of building up a common law of labor relations, through collective bargaining.

Explicitly building on Held, Adams contended that in the scheme of petty industry, the regime of tools, the ordinary rights of personal freedom, secured to men an enjoyment of the fruits of their labor. In the great industries of today, however, the laborer was dependent upon the owner of machines, of materials, and of places for the opportunity to work. It followed that laborers must unite or they would surely get the worst of any bargain. Underlying labor's demands was the "idea" that the laborers had some right of proprietorship in the industry to which they gave their skill and time. Thus collective bargaining and the labor

contract envisaged a crystallization of a common law of labor rights which was in full harmony with the development of Anglo-Saxon liberty.

Finally, E. R. A. Seligman did much to clarify the objectives of the Historical School. He said, in 1883, that the difference between the orthodox school and the modern German school was not between deductive and historical methods. The exclusive use of either led to absurdities, producing an unreal science or a body of archeological facts. A judicious combination of both was the only permissible procedure. The chief point of difference was that the orthodox had an atomistic, individualistic point of view; the other had a social standpoint. The orthodox posited the universal spirit of self-interest; the other stressed the "multiplicity of motives which cannot be jumbled together in the phrase, 'desire for wealth.'" It emphasized, also, the importance of legal systems and historical causes and the close connection between ethics and economics as sister moral sciences. Seligman felt that only extremists would deny any value to the views of those on the other side. He called attention, however, to the "practical use to which the bald, unqualified, and therefore untrue theories" have been put in late years. For example, the so-called "scientific socialism" of Lassalle and Marx was the "logical conclusion from the premise that labor is the cause of all value, and many of Mr. Henry George's wild fallacies are traceable to an almost superstitious acceptance of the Ricardian law of rent." The state, concluded Seligman, had a duty to interfere where free competition ends disastrously, and "where the powers of state themselves are threatened . . . by corporate monopolies." ("Sidgwick on Political Economy," the *Index*, August 16, 1883, pages 75-76.)

Despite the numerous attacks on it, the "new school" had made its way. In the seventies and eighties, it furnished a rationale for combating the arrogant individualism of the time. Its emphasis on history and statistics was a powerful force in developing these mighty instruments for the expansion of economics. Its impact is epitomized in the statement of principles of the American Economic Association on its formation in 1885:

1. We regard the state as an agency whose positive assistance is one of the indispensable conditions of human progress.
2. We believe that political economy as a science is still in an early stage of its development. While we appreciate the work of former economists, we look not as much to speculation as to the historical and statistical study of actual conditions of economic life, for the satisfactory accomplishment of that development.
3. We hold that the conflict of labor and capital has brought into prominence a vast number of social problems, whose solution requires the united effort, each in its own sphere, of the church, of the state, and of science.
4. In the study of the industrial and commercial policy of governments we take no partisan attitude. We believe in a progressive development of economic conditions, which must be met by a corresponding development of legislative policy.

Having given so much, let us take something away. The statement of principles was in a few years dropped. For a while there occurred an overwhelming emphasis on the doctrine of marginal utility as the key to all economic analysis. Interestingly, the German-trained contingent was the first to welcome Jevons' theory as a part of the new economics (Clark attributed his own version of marginal utility to the inspiration of his German teacher, Karl Knies), but they had hardly intended that economics should be restricted to this doctrine, a development which was accelerated by the popularity of the translations of the nonmathematical, "Austrian" works. The creation of chairs of economic history, sociology, and social ethics tended to remove from economics the issues that had made the Historical School a vital force, especially the concern with the moral problems presented by industrial changes.

We are ready to sum up. From the beginning, the Americans followed the German Historical School in a discriminating fashion. They distinguished between its methods and its political philosophy. The first they adopted with enthusiasm, and made it an enduring feature of American economic thought. They diversified their scientific study. They developed new realms of interest, such as public finance, railroads, agriculture, labor, and the history and significance of technological and legal relations. They kept alive a serious interest in the systematic treatment of "commercial crises." They enriched economics with historical and sociological material. It became difficult henceforth to discuss even the most abstract doctrines without reference to statistics, history, and environment. Yet Americans adapted the attitudes of the German Historical School to their own traditions. For the Germans, the organic character of society was reduced to a question of action by a centralized and almost dictatorial state authority. This is not what organism meant to the Anglo-American mind. The national state was not the only embodiment of society. In the United States, particularly, there was a multitude of state and local authorities. There were, in addition, entities of a social or economic sort that exercised organic functions. In the new doctrine, therefore, as transformed by the Anglo-American mentality, doctrinaire individualism was modified or corrected by the encouragement of the powers and functions of a great many aggregates. This spelled the fundamental distinction in political outlook between the Historical School in its native land and its American heirs.

DISCUSSION

J. M. LETICHE: The papers presented by Professors Hutchison and Dorfman constitute an important event in the history of our discipline, for to my knowledge this is the first appearance of research which has been conducted expressly and systematically on the international flow of economic ideas. It is an appropriate occasion to raise general questions about a great deal rather than particular questions about a little. Why is the international flow of economic ideas a problem? What are the contributions and hypotheses which emerge from this new research? How are they relevant to the economist in the twentieth century? These are the three questions to which I shall direct my attention.

To what Professors Hutchison and Dorfman have said as to why it is a problem, I wish to add here only the following comments. The study of the international flow of economic ideas deserves particular attention for its possible contribution to the future rate of development of our science, both within and across national and linguistic frontiers. Among nations of the noncommunist world, the state of economics in the mid-twentieth century appears to be distressingly fragmentized particularly along national lines. In the communist dominated world, this field adds no support to Soviet claims of discovery. Since the second World War, the United States has become an important exporter of economic ideas, but as yet we know little of the relation between this exportable product and the altered form which it takes in new intellectual and institutional environments. We face the problem of adjustment from the role of a net importer to that of an expanding exporter of basic research. The impact of this adjustment upon our own scientific advance is of great importance. Recent appraisals of our science by Professors Schumpeter and Robbins reveal no strong ground for complacency. Schumpeter asserts that the theory of our time is greatly superior to the theory of 1900 as regards technique. He writes in his recently published *History of Economic Analysis:* "Results are more reliable, proofs are more rigorous. This in itself also means *more* results and more *specialized* results that fit better the endless variety of the configurations of economic reality. At the same time it must be admitted that fundamentally new ideas have been almost wholly absent. We make much more of the ideas which we have inherited from the preceding period and often present them in a new light, but we have added little to them." (Page 1145.)

Robbins in his book, *The Economist in the Twentieth Century* (pages 1-17), notes that in the last fifty years important contributions have been made in the general theory of money and output. He agrees with Schumpeter that development in the applied fields has been remarkable. I think we all recognize, however, that, while our profession has witnessed much advance in the techniques of economic analysis (i.e., in economic history, economic theory, statistics, and economic sociology) and that within limits they have proved their practical usefulness, the deficiencies of our knowledge are

lamentable and must continue to be a source of embarrassment. Research on the nature of the development of economic ideas and their international impact is, therefore, long overdue.

Professor Hutchison has shown that between 1871-74, Jevons, Menger, and Walras reached similar conclusions with respect to marginal utility analysis without, for the most part, reacting to, or being stimulated by, any common cosmopolitan intellectual situation brought about by an active international interchange of ideas about the problem of value or any other economic problem. In effect, they reached their discovery by their own original and mainly nationally independent routes. This discovery represents yet another important case study of advance in analytical economics which revealed inadequate former technique. Many of our intellectual resources would be freed if it were recognized once and for all that the development of our science cannot be explained in deterministic terms or on grounds of the author's "politics" or "what he stands for."

There has been a continuous, though somewhat zigzagged, development in techniques per se. I shall argue in a moment, however, that another branch of growth had different roots, has been more complex and probably more important for the international flow of economic ideas.

Regarding the increase in the international flow of economic ideas which occurred in the 1870's, Professor Hutchison says that it may be traceable to the abstract or mathematical nature of the new ideas on value and to the gradually increasing importance of the academic scholar. These factors were probably important, but I doubt whether economic ideas expressed in nonmathematical terms are less likely to obtain an international hearing than those expressed in mathematical terms. The opposite experience has probably been the case as illustrated by the fact that Adam Smith used not a single mathematical or highly abstract term. Yet English editions and translations of the *Wealth of Nations* appeared in Ireland, the United States, Denmark, Holland, France, Germany, Italy, Spain, and Russia within approximately twenty-five years of its publication.

Professor Hutchison observes that the intellectual ferment, considerable cosmopolitan interchange, and significant development of economic ideas that occurred generally in the early 1890's are to be matched only by the insignificant inflow and development of ideas in England during Marshall's predominance. Hutchison suggests that England's insular independence of the work of Walras, Pareto, Wicksell, and the Austrians was primarily the result of Marshall's emphasis upon "historical and descriptive economics which was inevitably subject to a considerable national relativism," particularly if guidance of policy was kept in view. Hence Marshall emphasized the continuity of the national tradition in economics and relied on familiar British authorities and modes of expression. Certainly these national elements played their role, but I submit for your consideration a more general, complementary hypothesis. Whenever a great synthesis is achieved in economics it results in a reduction of intellectual ferment, of controversy, and of national as well as international flow of economic ideas. When one regards Marshall's superb analytical insight, his wide range of information, and, above all, his strong

sense of proportion and relevance, is it at all surprising that he overpowered the economics profession of his generation? Adam Smith also appears to have either overwhelmed or chloroformed his critics. From the publication of the *Wealth of Nations* in 1776 to the founding of the *Edinburgh Review* in 1803 there was a barren period in the development of our science.

While the importance of a free international flow of economic ideas cannot be overemphasized, the general conclusion that I draw from Professor Hutchison's and Professor Dorfman's research is that during the periods analyzed parallel development of economic thought occurred independently in Britain and the United States with relatively little foreign influence. Professor Hutchison tells us in his book, *A Review of Economic Doctrines—1870-1927* (1953, page 67), that "Ricardo, Mill, Cournot, Thünen, Jones and Roscher, representing the English classical system, mathematical analysis, and the historical approach, gave the background from which Marshall started." Walter Bagehot also had a strong influence. Marshall's insistence upon retaining the proposition that the quantities of the factors of production are not given, but are functions of their rates of remuneration is an important difference between the Marshallian and the Austrian system, and obviously follows the British classical tradition.

In his third volume on *The Economic Mind in American Civilization, 1865-1918,* Professor Dorfman cites ample evidence for the contention that in each new generation the economists of the United States "remained fresh in their approach" and carried the ideas of their predecessors a step forward, "bearing witness to the need of change and adaptation in a dynamic economic order." (Page xii.)

I agree with Professor Dorfman's statement about the influence of the German Historical School on American thought, but, as he has written, men such as Simon Newcomb, a professor of mathematics in the United States Navy and Johns Hopkins University, were greatly influenced by Jevons and even more so by Cournot as well as by the German Historical School. Moreover, Jevons was a man who had won a reputation by the excellence of his work in applied economics. His *Serious Fall in the Value of Gold* and his brilliant *Coal Question* met the most rigorous standards of the antitheoretical school. His statistical methods were superior to those devised by the German Historical School.

Professor Dorfman's citations of the German authors concerning the position of the English classical school on such matters as "Induction vs. Deduction," "man not being merely an exchangeable animal," excessive beliefs as to "natural laws" and harmonies, "universal spirit of self-interest," etc., may have dispelled silly notions held by some American economists. But they are either irrelevant or erroneous as an interpretation of Adam Smith, David Ricardo, Nassau Senior, and John Stuart Mill.

It is questionable whether there was any economics as a professional science in the United States prior to the 1880's and 1890's. Previous to that time even in the oldest American universities economics was taught in departments of philosophy or social science. The basis of scientific development cannot come from abroad. Domestic economists can save years of work by

utilizing foreign developments if they are prepared to understand the foreign contributions and if the environment is sufficiently conducive to carry the science forward. Even within a country, economic theories become obsolete, perhaps more often and more rapidly, because the questions which they were designed to answer are no longer of interest rather than because new techniques are developed or new insights lead to detection of error in their inherent logic. Given the different national objectives and various institutions of different countries, this phenomenon appears to be even more important in the development of economics through the international flow of economic ideas. For example, the reception of neoclassical thought in the United States was dominated by the work of Marshall. Yet Taussig, though influenced by the German Historical School, followed a strong American tradition, emphasizing facts and verification. This admittance of theory with an empirical orientation was an important American contribution to economic thought. I believe that the emphasis upon empirical research was to a great extent an endogenous phenomenon stemming from the historical and institutional needs of the American environment. The early work of W. C. Mitchell and W. I. King and of the National Bureau of Economic Research represented the real foundation of empirical research on business cycles and national income in the United States. Empirical market research is another illustration.

In the twenties the saving-investment formulations came primarily from abroad, as did the output employment analysis in the thirties. Yet in each case some parallel work was going on in virtually all the important national centers of economic research.

Since the later thirties and particularly since the second World War, original contributions have been stemming to an ever greater extent from America. By this time great American economists were working at home, and a large number of younger economists had been trained in an environment where controversy and new ideas were welcome. Research is long overdue on the role that migration of people as compared to ideas and techniques has played in the development of scientific knowledge, as well as in economic progress. In his theory of class circulation, Pareto spoke of the role of abstract reason and stratified thought in the development of science; he maintained that the lack of mobility among classes results in stratification of thought, and conversely (see Vilfredo Pareto, *The Mind and Society*, edited by Arthur Livingston, Vol. III, pages 1419-1432, and Vol. IV, pages 1740-1915). In the thirties we witnessed the migration of probably more than half of the best trained Austrian and German economists to the United States. Others came from Hungary, Poland, Czechoslovakia, etc. They served as a great catalytic agent and added greatly to the American scientific output and to the international flow of economic ideas. Recent work in the theory of games, activity analysis, input-output analysis, and oligopoly is associated with the names of some of our most distinguished economists who came from abroad. There is danger that this factor may be exaggerated, since being foreign, their work stands out; but the fact is that they would not have been able to make their great contributions had the American environment not been receptive to their ideas. Their research has to a large extent

been affected by the experiences and problems of the American profession as well. The result of this interchange can be seen by comparing the quality of their output with that of the few good economists who remained in Germany.

The international flow of economic ideas that deal with general theory or technique per se is for the most part neutral to political boundaries. Those ideas pertaining to more specific "models," based on more particular assumptions, recognize national frames of reference. The latter may be utilized to great advantage if wisely applied to the national environment, but pitfalls will be encountered if they are transplanted indiscriminately. The menace of the "ideological approach" to the international flow of economic ideas (as typified by communism and fascism) rests in the fact that it is largely a pathological phenomenon, based neither on economic technique per se nor on national frames of reference. These three categories require clarification by research with respect to the form and effect of the contemporary international flow of economic ideas.

There is important precedent as well as current relevance for the correspondence of the reception of new ideas and the freedom of the environment into which they flow. To the economist in the twentieth century, the international flow of economic ideas is pertinent for at least the following reasons.

The greater the difference between the national objectives of one country and another the less general is the application of economic principles. This fact raises the problem of developing economic ideas which are or can be made more relevant to the various objectives of other countries in the noncommunist world so that they will contribute to the integration rather than the further diversification of those objectives.

American economic thought does not appear to have much relevance to economists in some European countries; for example, to those of France and Italy, not to mention those of underdeveloped areas. I submit that American economists will have to deal to a greater extent with problems of political economy if they are to obtain a more productive hearing abroad; for otherwise there is danger that our friends will look elsewhere. The premises and objectives of a good deal of American economic thought do not appear to be applicable to the problems of many nations of the free world.

The most significant obstacle to the development and the international flow of economic ideas does not seem to be the problem of the disappearance of "frontier types" coming from other disciplines, as argued by Professors Hutchison and Dorfman. I believe that the past half century has produced economists who in analytical power and originality compare well with any other period in the history of our science. The trouble today is that our science is badly compartmentalized. Our writing has become more aesthetic than communicative. As economic theorists, economic historians, econometricians, etc., we appear to be somewhat insulted if anyone understands us. The difficulty appears to be that collectively we know too much for any one of us to know very much. The progress and the international flow of economic ideas is in the future much more likely to depend upon greater synthesis

and upon the application of new general tools of knowledge within the economics profession rather than upon additions coming from other disciplines.

The United Nations, the exchange of persons program, and our educational foundations have greatly assisted in the international flow of economic ideas. Fundamentally, however, basic research is an individualistic phenomenon. It will continue to progress most rapidly in those national environments where genuine scholarship, with its characteristics of spontaneous curiosity, dispersed accident, and free controversy, can best endure.

GEORGE H. HILDEBRAND: As these two excellent papers both show, the history of economics can be interesting both as history and as economics.

From the standpoint of history, both authors are concerned with the dissemination of certain key ideas in economic thought. They are aware that this problem requires more than the establishment of dated sequences, important as these happen to be. Actually, the task belongs to the realm of cultural anthropology. Essentially it involves the processes of geographic diffusion of strategic conceptions, though not the simple type of diffusion originally suggested by Mr. Grafton Elliot Smith.

There is first the question of determining which economists initially proposed significant new departures in thought, at what times and in what places. This leads naturally to the second step: to what extent were the innovators really innovating and to what extent were they drawing upon earlier and more fragmentary suggestions? As Mr. Hutchison says, for example, Jevons and Menger both advanced their main ideas of marginal utility in 1871, each working in isolation. Walras followed, also independently, in 1873 and 1874. Yet all three had a substantial, if mixed, intellectual inheritance at their disposal. Major items in the bequest were the psychological principle of satiety and the notion that demand was an inverse function of price. The donors were Bentham, Jennings, Cournot, A. Walras, Dupuit, Hermann, and Gossen. Drawing upon Usher's theory of invention, I suggest that Jevons, Menger, and Walras performed the work of creative synthesis, generalizing, unifying, and extending formerly isolated and largely neglected ideas. No doubt they were in some degree inspired by a crisis in technical thought, in which they were driven to reject rather than to defend outworn ideas.

This leads to the next main question: the diffusion and circulation of the innovators' major intellectual productions. Partly this is a matter of the geographic distribution of ideas. Obviously distribution is fostered by travel and study abroad, familiarity with languages, and international dissemination of published materials. Professor Dorfman has aptly illustrated these processes, in his interesting account of the influence of the ideas of the German Historical School upon American economic thought after 1870. However, there is another side to the question, well indicated by Mr. Hutchison. It concerns the barriers rather than the channels to effective distribution of new modes of thought. Partly these barriers have been linguistic. Indeed, the obstacle to the circulation of the main contributions to marginal analysis, with the notable exception of Wicksteed, was unfamiliarity with

mathematics. As Hutchison suggests, the situation has improved somewhat in these respects in more recent times.

Partly also there is a formidable barrier in the tendency of established systems of thought to persist, asserting themselves through such very practical media as dominant schools led by strong personalities in control of academic opportunities. We have heard of self-perpetuating groups in politics and business; this phenomenon is not absent from universities. Schumpeter says that French chairs were long barred to the followers of Walras and German ones to those of Menger. Historicism exerted similar dominance in Germany. Mr. Hutchison refers to the eminence of Marshall's leadership in precluding much inflow of Continental teachings into England after the early nineties. By contrast, a vigorous brand of utility analysis, mainly mathematical, was easily transplanted and readily took hold in Italy, fostered by strong intellects such as Barone and Pantaleoni and relatively unopposed by established academic interests, because there were none. Rather ironically, Italy in recent times has lagged rather markedly in the domain of applied economics, primarily because its valuable tradition of pure theory has long dominated academic research and instruction.

To take still another example, consider the relatively slow penetration of English economics by marginal analysis, despite Jevons' pioneer work. Jevons himself felt in 1879 that the only hope was "to fling aside, once and for ever, the mazy and preposterous assumptions of the Ricardian School. Our English economists," he complained, "have been living in a fool's paradise." Yet it remained for Marshall to carry through this intellectual reformation. He did so in typical English style, preserving what he thought was usable from the past, keeping the form while changing much of the content. Thus the classical real cost doctrine of factor supply survived within the newer context of marginalism, in Marshall's system.

Turning now from history to economics proper, I wish first to suggest a few points raised by Mr. Hutchison's paper. I would agree with him that so far as English economics is concerned, the story after 1870 centers on the slow triumph of marginal analysis, first in value theory and later in factor service pricing and distribution. The conquest of Richardianism by the marginalists involved the ultimate seizure of intellectual territory long isolated from Continental contacts and fresh ideas. Credit for the victory goes to Jevons, Marshall, and Wicksteed. Partly the triumph was a revolution from within, wrought by Jevons' own originality of thought. Partly also the victory was gained by the infusion of new ideas from abroad—ideas that Marshall and Wicksteed absorbed and reworked in their respective fashions.

However, I think there was also another influence at work, indigenous to the British Isles. I refer to the theory of wages, which by the end of the sixties represented a genuine crisis in thought. As a result of the onslaughts of Longe and Thornton, the wages-fund theory was now in disrepute, though the odium was somewhat undeserved. The subsistence theory had patently failed to fit the facts of falling urban birth rates and rising real wages. Trade-unionism was growing stronger, winning intellectual support for its claims

to be able to raise general living standards and to redistribute income. To these claims the older theories had no obvious answer.

The reason was that the older doctrines lacked the tools of partial equilibrium analysis; hence an adequate conception of a derived demand function for labor. Though in its crude way, as Schumpeter says, the wages-fund theory was correct in pointing to over-all, short-run limits to raising living standards by simple redistribution of the social product, its reasoning carried no conviction for those partial cases in which union wage raising actually went on and still largely goes on.

The renovation of wage theory began with the work of Fleeming Jenkin, an engineer by profession. In 1870, Jenkin introduced the idea of a negatively sloped demand function for labor services—a concept redolent with skeptical implications regarding the ameliorative claims of unionism. The theoretical possibilities were not lost by Marshall, who went on to work out the principle of elasticity of derived demand. However, Jenkin himself suggested a kind of indeterminacy for the wage bargain, an idea that was taken up and developed in the eighties by Jevons and Edgeworth. Edgeworth in particular was thus led to his famous theory of bilateral exchange. By the middle of the nineties, the principle of marginal productivity had been worked out, in various formulations, by Wicksteed, Wicksell, and Barone. Clearly this history is complex. Yet it seems a safe inference that the crisis of wage theory in England in the sixties had much to do with the revolution in distributive theory that occurred in that country in the three decades that followed.

Turning to a radically different kind of theory—that of the German Historical School—Professor Dorfman has given us a most interesting account of its influence upon American thinking. As he shows, that influence had its constructive side. It was responsible for the introduction of statistics into academic training, and it fostered interest in the accumulation of quantitative data. So were laid the foundations for modern theoretical and applied work of the highest importance. Moreover, the Germans did much to develop the art of public administration. They were, of course, keenly interested in state intervention and control, where again they influenced American thought. There are legitimate differences of view as to how beneficial *in toto* these doctrines of state intervention have actually turned out to be. But there is no doubt that they did help indirectly to temper the harsh and extreme conception of individualism that was hitherto prevalent in American life and thought.

Yet, so far as the German influence is concerned, there is another side to the coin. Inspired by the Hegelian tradition, the earlier Germans such as Roscher, Knies, and Hildebrand attacked the generalizing qualities of pure theory and sought to substitute in its stead various philosophies of history, apparently wholly unaware that the philosophy of history is another type of generalization from natural law premises. The result was intellectual confusion and little else. But the reaction in Germany was not even a return to classical theory, save for the Marxian sect; far less was it a reformulation of classical ideas along more modern lines. Rather, there emerged the pragmatic relativism and antitheoretical bias that typified the methodological futilities of Schmoller and his circle.

Certainly there was nothing either wrong or even unique in that group's emphasis upon the collection of facts. But to paraphrase Marshall, facts themselves teach nothing. They must be related to theory. It was here that the German school failed. It had seen the possibilities of state intervention. Through its quasi-official standing it greatly developed the arts of public finance and administration. Yet its methodological narrowness seems to have prevented it from ever regarding state undertakings other than as problems of organization. The school showed no awareness of the elementary distinction between economy and waste, whatever the purposes of state action. Having rejected traditional theory as obsolete natural law doctrine, with allegedly hedonistic foundations and implications for policy that supposedly were uniquely laisser faire, the group could offer no rational economic criteria for guiding the choice between the things government could efficiently do and those it could not. In its view the state was omnicompetent. Perhaps there is, then, some connection between this kind of teaching and the popularity of crude ideas of physical planning in more recent times.

One last point: I wish that Professor Dorfman had decided to include Thorstein Veblen in his discussion, for I would have been interested in his assessment of Veblen's relation to German historicism. Concededly, Veblen was not a direct product of German teaching. Moreover, as Mr. A. W. Coats points out in the December, 1954, issue of the *Journal of Political Economy*, Veblen severely criticized the Germans for their preoccupation with facts and historical narratives; hence their lack of "a theory of anything" or of "a consistent body of knowledge." Yet I think Veblen shared much in common with the Germans, in the character of his destructive attack upon pure theory, old and new, and in the relativism of his theory of knowledge. That he was the most influential American exponent of these views seems to me beyond doubt. If so, he could be considered part of the German tradition as it developed in American economics.

Regarding the history of economic thought, an encouraging renaissance is under way. These two papers are valuable contributions to its advancement.

WILLIAM JAFFÉ: The character of an economy, to which Professor Bladen has alluded, depends in a certain measure upon the character of economists. Not that economists can make any serious claim to shaping the economy in any significant degree, but simply because what is regarded as the character of an economy consists of attributes selected and delineated by economists. Hence our general conception of any economy is bound to be influenced by the character of the economists describing or analyzing it. When economists are swayed by chauvinistic and nationalistic passions, the progress of our science is inevitably affected.

This makes it all the more interesting to reflect upon the personal attitudes revealed in the private correspondence of the economists of the period covered by Professor Hutchison's paper. From the Walras correspondence preserved mainly at Lausanne and Lyons we learn much about the character not only of Léon Walras but also of his most distinguished contemporaries. Their per-

sonal passions and prejudices had much to do with the sluggishness with which economic ideas moved across international and linguistic frontiers.

Did anyone, for example, discourage Edgeworth from presenting Walras' theories before the English public? The evidence is none too clear. Walras wrote to Foxwell on March 13, 1889, saying that he hoped that his theorem of maximum utility of new capital goods, as it then appeared in the corrected proofs of the second edition of the *Eléments*, would be found worthy of being offered to competent English readers. Foxwell informed Walras on March 15 that he had sent these proofs to Edgeworth and that Edgeworth was editing and translating the theorem for insertion in the next number of the *Journal of the Royal Statistical Society*. Edgeworth, however, soon abandoned the project, for as he explained it to Walras in a letter dated March 19, he had encountered technical difficulties in understanding the theorem (see my article in the *Journal of Political Economy*, April, 1935, page 203, and my Translator's Note [3] of Lesson 27 of Walras' *Elements of Pure Economics*). He added, and this is the significant point for our present purpose: "***with whom I have spoken and myself wish you to feel that we have spared no pains in this matter. I have, in addition to the regret which he feels at not being able to put the paper before the British, the feeling of remorse. . . ." Where I have put ***, the original letter shows a piece of illegible overwriting, the only example of this sort of illegibility I can find in Edgeworth's letters to Walras. It looks a little like "Prof.," and it was translated in Aline Walras' copies as "Le professeur." The "professor" might have been Foxwell, who was then a member, along with Edgeworth, of the Council of the Royal Statistical Society; but perhaps it was Marshall. Such a guess is probably not farfetched, particularly in the light of a later event in which Enrico Barone was involved and to which Professor Hutchison calls attention in his paper.

If the English did not display any great alacrity in admitting the contributions of Continental economists to their journals, it must be conceded that these discriminatory sentiments were reciprocated on the other side of the Channel. In fact, such sentiments were quite general in Europe. Did not Walras write to Barone in November, 1895: "One does not get anywhere with the English unless one puts them in their place and refuses to be pushed around by them. . . . I know from experience that having the English against us brings to our side the Americans who are generally well disposed in our favor." Pantaleoni, writing on August 12, 1889, asked Walras confidentially whether he did not agree that Menger was a plagiarist and Böhm-Bawerk a charlatan! Walras replied cautiously on August 17: "I do not think that Menger was altogether fair to Gossen. . . . As for Böhm-Bawerk, it seems to me that in the preface of his last work, he stated explicitly enough that he had borrowed his theory of capital from Jevons [sic! See Preface to *The Positive Theory of Capital*, page xxiii]; but since this theory isn't worth much, I am quite willing to let that pass."

The Iron and Bamboo Curtains of today were preceded three-quarters of a century ago by nationalistic, linguistic, and other curtains—all impeding the

free flow of ideas in the learned world. What, I wonder, would have been the fate of economic theory in this country had Enrico Barone succeeded in his attempt to obtain a chair here? On November 26, 1895, he wrote to Walras and pleaded with him to use his influence with J. B. Clark on his behalf. Had either Columbia or Harvard found a place for Barone at that time, his theoretical work, which, according to Schumpeter, was never properly appreciated in Italy, might have found new scope; and economic theory in America might earlier have been enriched from abroad, as it has been more recently by the teaching of economists who have come to us from many lands.

DEVELOPMENT POLICY IN UNDERDEVELOPED COUNTRIES

THE ROLE OF INDUSTRIALIZATION IN DEVELOPMENT PROGRAMS[1]

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Industrialization is the main hope of most poor countries trying to increase their levels of income. It is also the most controversial aspect of the problem of economic development. Attempts to apply general economic principles to this field have usually proven inconclusive. This result has been due to an incomplete theoretical formulation of the problem as well as to inadequate data. In most cases there remains a wide margin of disagreement between the advocates of international specialization and investment in primary production on the one hand and the proponents of "balanced growth" and industrialization on the other.

Most of the participants in this controversy would probably accept the proposition that the primary aim of a developing country is to secure the greatest increase in its income (total or per capita) from its available resources in the long run. The main problem lies in evaluating the amount of scarce resources which will actually be required by alternative types of production. For a number of reasons, the price system, which makes this evaluation fairly accurate in more highly developed economies, is often a rather unreliable guide to the desirability of investment in an underdeveloped economy. Even when corrections have been made for the more obvious defects in prices, such as tariffs and subsidies, there remain two sets of factors which make a partial equilibrium analysis based on existing prices very difficult: (1) the existence of structural disequilibrium in the use of factors of production, with labor commonly underemployed and capital and foreign exchange rationed; and (2) the interconnectedness of productive sectors, as a result of which investment in one may make investment in others more profitable—often called "external economies."[2]

[1] This paper is based on a study of Resource Allocation in Development Programs which has been made possible by a grant from the Stanford Committee for Research in the Social Sciences.

[2] The distinction between these "pecuniary" external economies and the more traditional "technological" external economies has been pointed out by Scitovsky in "Two Concepts of External Economies," *Jour. Pol. Econ.*, April, 1954. Rosenstein-Rodan was among the first to analyze this factor in "Problems of Industrialization of Eastern and South-Eastern Europe," *Econ. Jour.*, 1943, pp. 202-211.

Both of these factors are particularly important in evaluating industrial investments. Less than half of the total use of primary factors takes place directly in the average sector producing an industrial commodity; the remainder is scattered throughout the economy in the sectors producing raw materials, producer services, and other industrial commodities. In contrast, most of the requirements of capital, labor, raw materials, and foreign exchange for agricultural and mineral production and services occur directly in the producing sector. It is therefore possible to take account of the existence of structural disequilibrium fairly well in evaluating nonindustrial investments, but both the existing disequilibrium and the effects of future investment complicate judgments on industrial investment.

The present paper will investigate the influence of these two factors —structural disequilibrium and external economies—on the optimum amount and composition of industrial investment in a development program. Such an analysis can only be made in the context of a general equilibrium model because intersectoral relations are the essence of the problem. The empirical background for the model is taken from an over-all study of the Italian economy[3] and of the development programs for Southern Italy.[4]

The paper consists of three parts. The first summarizes the essential features of the model and the method of application. The second part shows the effect on industrial investment of variation in three key factors reflecting the factor endowments of a given area: the labor supply, the availability of foreign exchange, and the rate of growth. The third part shows the difference between an optimum program which takes into account external economies and one which does not. It also tries to shed some light on the general problem of investment priorities.

I. *A Model of Economic Development*

General Characteristics. In order to throw light on the problems I have just outlined, the analytical model must include the following elements: (1) the use of capital, labor, and purchased materials and services in each type of production; (2) the foreign exchange cost of possible imports; (3) the foreign demand for possible exports; (4) restrictions on the commodity composition of any increase in national income; (5) availability of labor, foreign exchange, investment funds, and other scarce resources.

[3] Preliminary results have been published in H. B. Chenery, P. G. Clark, and V. Cao Pinna, *The Structure and Growth of the Italian Economy* (Rome: U. S. Mutual Security Agency, 1953).

[4] Empirical data for the analysis of development programs within the framework of a regional input-output model is being collected by the Input-Output Unit of the Italian government and the Association for the Industrialization of Southern Italy, to which the author is a consultant.

The statistical basis for such an analysis is provided by an input-output table, augmented by studies of consumer demand, export demand, etc. The solution to the problem involves choice between domestic production and imports, between production for the home market and exports, and maximization of output within the given restraints. The analytical technique, therefore, must be the more general one of linear programing, which in this case is a straightforward extension of input-output methods.

The purpose of the investment program is to secure a specified increase in national income with a minimum use of investment funds, subject to limitations on the composition of national income, export demand, labor supply, and foreign exchange availability. In the case of Southern Italy, investment funds are variable through subsidies from the central government, and total investment is therefore variable within limits. In other cases, the maximum growth achievable from a given rate of saving could be estimated by varying the time period of the analysis.

The several restrictions in the model are incorporated in the solution in various ways:

1. The division of national income between consumption and investment is taken as fixed. Consumption is assumed to be a function of per capita income only, as described below. All elements in final demand are therefore determined by the assumed increase in national income.

2. Total investment must equal savings plus the excess of imports over exports, summed over the time period involved (ten years). We are not concerned with the time path of production during the period, however, but only with the flow of goods and services at its end. If necessary, the amount of investment included in final demand may be modified in a successive approximation.

3. The excess of imports over exports must correspond to the required amount of external investment funds determined by the solution. Solutions must be determined for several possible levels of the trade deficit to meet this condition.

4. The price of labor must be such that total demand does not exceed the total supply.

The size of the model used represents a compromise between the need for sufficient detail to illustrate the interdependence actually existing among productive sectors and the necessity to hold down costs of computation. Since the interconnections among the industrial sectors are the most significant, the test model contains nine industrial sectors, two sectors of primary production, and three service sectors. Five export sectors have been distinguished, while imports are possible in ten of the fourteen sectors. This model represents a considerable aggrega-

tion of the original Italian input-output study, which contained sixty sectors, and of the matrix for Southern Italy, which contained twenty-eight sectors.

Elements in the Model. The following paragraphs will indicate the conceptual nature of the various elements in the model. For present purposes, it is sufficient to consider the numerical data as fairly realistic estimates of the parameters involved. The input coefficients and consumption estimates result from extensive statistical investigations, while most of the other relations are based on scantier evidence or (where indicated) on pure hypothesis.

1. *Total Requirements.* As the model is formulated, a specific set of final consumption levels for the several commodity groups is taken as the goal of the investment program. These requirements can in most cases be satisfied from either production or imports. The GNP in the target year will equal the total of these final demands less the import surplus. To simplify the analysis, I assume that capacity in the base year is fully utilized and that in no case will it be efficient to shut down existing production. Present production (net of interindustry use) can then be subtracted from the given final demands to determine the total requirements by sector to be supplied from either increased production (requiring new investment) or imports. These two sets of estimates are shown in Table 3 on page 47.

Final demand consists of government consumption, private consumption, investment, and exports. (The estimates of final demand were made in Italy by the collaborating groups noted above. They were taken as data for the present test, with the exception of some exports, which were allowed to vary as explained below.) The magnitude of investment was based on the estimated sum of domestic savings and the import surplus (as indicated above) while government consumption was projected from past trends; the composition of both was kept unchanged. A more detailed study was made of private consumption. Consumption estimates for some seventy commodity groups were based on population growth and income elasticities related to prospective increases in per capita incomes. They were then aggregated to the present number of sectors.

2. *Production Possibilities.* Each of the commodity groups in the model represents an aggregation of many subgroups. For this test, I have secured average input coefficients through aggregating the intersectoral flows into fourteen groups. I have retained the variation in capital/output ratios within each sector, however, by arranging the subsectors in the order of their capital intensity and assuming that their output will be required in fixed proportions. On this assumption, it will be economical to produce those commodities within each sector

which have low direct capital requirements and import those having higher capital requirements, since the indirect capital use within each major sector is assumed to be the same for all subsectors. (If this assumption is not made, the number of sectors must be increased.)

For the linear programing model, the alternative ways of satisfying the given requirements in sectors where imports are possible consist of mixtures of domestic production and imports. The input requirements for domestic production are given in Table 1, and the capital and labor requirements in Table 2A. Some allowance must be made in using minimum investment as a test of efficiency for the fact that capital goods have different degrees of durability. This was done here in the case of land reclamation and other agricultural construction through reducing the capital requirement by the estimated present value of the capital stock twenty-five years hence. An alternative would be to treat capital consumption as a separate input. The assumed variation in capital coefficients within each sector is largely hypothetical, as are the labor coefficients. The capital coefficients in industry were based on a sample of some seventy-five investment projects which were actually undertaken in the past few years. Imports are assumed to require one unit of foreign exchange in each sector except agriculture (assumed to require 1.25).

The choice between domestic production and imports is made by comparing the marginal direct and indirect use of capital in domestic production with the domestic price of imports. This procedure results in a decrease in the proportion of requirements in each sector which is imported as the price of foreign exchange rises.

3. *Exports.* The assumed demand functions for exports are given in Table 2B. The optimum level of exports in each sector will be that at which the marginal (direct and indirect) cost of producing exports is equal to the domestic value of the marginal revenue from increased exports. This optimum level will increase for higher prices of foreign exchange.

Solution. The techniques of solving linear programing problems need not concern us here, but the concepts are useful. In the present case it was possible to take advantage of the fact that the matrix of input coefficients can be made almost triangular—i.e., the interdependence is almost all in one direction, as shown in Table 1. This characteristic makes it possible to solve the problem without inverting the matrix of coefficients, which saves on solution time and also makes it easy to handle the demand functions for exports which have been assumed. The concept of "shadow price" plays a key role in the solution. It measures the total use of scarce resources in the economy to produce a given commodity. In the present case shadow prices are expressed in

TABLE 1

INPUT COEFFICIENTS*

Sector	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	XIV
I. Clothing	+.917	—	—	.001	.001	—	—	—	—	—	—	—	—	—
II. Textiles	.254	+.615	.001	.003	.016	—	—	.001	—	—	.008	—	.001	—
III. Construction	—	—	+.819	.013	.011	—	.010	—	—	—	.004	—	.001	—
IV. Mechanical	.003	—	.063	+.897	.012	—	.001	—	—	—	.001	—	—	—
V. Other industries	.005	.001	.033	.037	+.857	.001	.003	—	.037	—	.008	—	.001	—
VI. Food	.027	.001	—	—	.001	+.873	.001	.021	—	.006	.023	.001	.002	—
VII. Metallurgy	.001	—	.081	.221	.018	.004	+.515	—	—	—	.011	—	—	—
VIII. Agriculture	.003	.138	.002	—	.149	.430	.002	+.899	.011	—	.023	.005	.001	.001
IX. Transport	.001	.005	.011	.011	.049	.021	.017	.022	+.966	.032	.012	.151	—	—
X. Petroleum	.002	.008	.027	.017	.010	.005	.019	.004	.253	+.987	.015	.011	.003	.018
XI. Chemicals	.018	.059	.042	.017	.089	.006	.050	.023	.002	.013	+.691	.032	—	.001
XII. Mining	.001	.005	.058	.003	.002	.004	.055	.001	.048	.211	.071	+.995	—	.035
XIII. Services	.357	.168	.043	.141	.193	.192	.007	.169	.027	.084	.132	.067	+.957	.010
XIV. Power	.001	.007	.005	.009	.004	.003	.049	.002	.038	—	.035	.013	.005	+.994

* Per unit of output (in value terms).

TABLE 2A

CAPITAL AND LABOR COEFFICIENTS

Sector	Labor/Output Coefficient*	Average Capital/Output† Coefficient
I. *E* Clothing Export	1.00	.5
I. Clothing	1.00	$.2+.1d$
II. E Textile Export	.50	.65
II. Textile	.50	$.5+.275d$
III. Construction	1.80	.45
IV. *E* Mechanical Export	.65	.87
IV. Mechanical	.65	$.5+.75d$
V. Other Industry	.90	$.2+.9d$
VI. *E* Food Export	.15	.58
VI. Food	.15	$.33+.25d$
VII. Metallurgy	.50	$.6+.9d$
VIII. Agriculture	0	$2.0+.833d$
IX. Transportation	.50	3.86
X. Petroleum	.15	.72
XI. *E* Chemical Export	.35	.75
XI. Chemical	.35	$.38+d$
XII. Mining	.45	$-6.796+10d+1.578/d$
XIII. Services	3.00	.58
XIV. Power	.20	3.0

* In 1,000 employees per billion lira of output.
† Where d is the ratio of domestic production to total requirements in each sector.

terms of investment cost. The shadow prices of other basic factors whose supply is limited—foreign exchange and labor—are measured by their opportunity costs. Given the prices of these resources, the shadow price of each commodity consists of its direct capital cost plus the sum of all its inputs multiplied by their shadow prices. The optimum solution is reached by comparing cost of securing each commodity by alternative methods with the value given by its shadow price. This method is well suited to handle alternative production techniques when such data are known.

The usual method of solving a linear programing problem involves fixing the resource availabilities and output requirements and determining the optimum combination of activities which corresponds to these

TABLE 2B

ASSUMED DEMAND FOR EXPORTS

Sector	Export Level*
I. *E* Clothing	$200\ (1-P)$
II. *E* Textiles	$300\ (1-P)$
IV. *E* Mechanical	$600\ (1-P)$
VI. *E* Food	$2000\ (1.1-P)$
XI. *E* Chemical	$200\ (1-P)$

* Where P is the price in foreign currency. All functions are purely hypothetical.

limitations. In the present case it has proved useful to examine the implications of varying levels of availability of foreign exchange and labor. I have therefore assumed several combinations of the prices of labor and foreign exchange and determined the use of these resources corresponding to the assumed prices. The results can be plotted in a graph, as in Figure 2 on page 51, from which it is possible to determine the optimum prices corresponding to any levels of foreign exchange and labor. These prices in turn determine the activities used and investment in each sector.

TABLE 3

TOTAL REQUIREMENTS BY SECTOR

Sector	Final Demand*	Total Requirements†
I. Clothing	278	162
II. Textiles	195	191
III. Construction	447	280
IV. Mechanical	427	383
V. Other industries	163	144
VI. Food	1,308	443
VII. Metallurgy	36	31
VIII. Agriculture	639	98
IX. Transport	70	24
X. Petroleum	225	168
XI. Chemicals	132	149
XII. Mining	63	68
XIII. Services	148	40
XIV. Power	59	22
Total	4,190	2,203

* Including 416 billion of exports for which the demand is taken as given.

† Final demand less domestic production in excess of interindustry demand in the base year; i.e., the excess of projected total demand over net domestic availabilities in the base year. Total requirements are equal to the increase in GNP plus the excess of imports over exports.

For each set of parameters, we have made several trials with varying prices of foreign exchange, holding the other parameters constant.[5] The results of these trials are shown in Table 4. Each one represents the solution requiring the minimum investment under the stated assumptions as to the values of the parameters. In the first three trials, an interpolation has been made at a constant deficit of 284, which was estimated to be the average amount of external resources necessary to achieve the desired increase in GNP within a ten-year period.

II. *Key Factors in Industrial Development*

I will now investigate the effects of differences in resource endowments, factor proportions, savings levels, and other basic factors on

[5] All the calculations on the aggregated model were made by Kenneth Kretschmer.

TABLE 4

RESULTS OF TRIAL SOLUTIONS

	A1	A2	A3	A4	A5	B1	B2	B3	C1	C2	C3	D1	D2	D3	E
Price of foreign exchange	2.7	2.8	2.9	3.0	2.84*	3.0	3.3	3.5	3.5	3.8	3.719*	2.7	3.0		3.60
Price of labor	0	0	0	0	0	.3	.3	.3	.6	.6	.6	0	0		0
Requirements‡	1.0	1.0	1.0	1.0	1.0	1.0	1.0	1.0	1.0	1.0	1.0	.5	.5		1.0
Exports	686	714	739	766	725	597	680	737	582	666	643	686	766		651
Imports	1163	1062	944	832	1009	1278	964	739	1208	823	927	861	629		926
Trade deficit	477	348	205	66	284	681	284	2	626	158	284	175	−137		276
Increase in GNP	1391	1524	1672	1812	1590	1150	1553	1834	1187	1652	1527	953	1051		1598
Investment in:															
Industry	1436	1600	1789	1959	1685	1063	1494	1824	1023	1468	1348	1033	1448		1629
Agriculture	389	519	676	838	589	376	840	1231	625	1289	1110	293	637		789
Services	669	717	772	818	742	579	720	818	592	746	704	436	555		740
Total	2494	2836	3238	3615	3016	2019	3054	3874	2240	3503	3162	1736	2640		3158
Total labor†	2839	3042	3270	3473	3145	2346	2929	3347	2298	2923	2755	1914	2423		3001
Ratio of investment to increase in GNP	1.793	1.861	1.937	1.994	1.895	1.755	1.966	2.112	1.887	2.120	2.057	1.850	2.513		1.976

* Interpolated.
† Excluding labor in agriculture.
‡ Fraction of requirements vector given in Table 3.

the optimum patterns of economic development. The hypothesis underlying this study is that industrialization may play a very diverse role in different types of country. Where structural disequilibrium exists, it is almost impossible to determine from the price system the true weight which should be given to these factors in investment decisions.

Variation in the underlying structural factors is reflected in a formal model through differing values of some of the parameters. Here I will

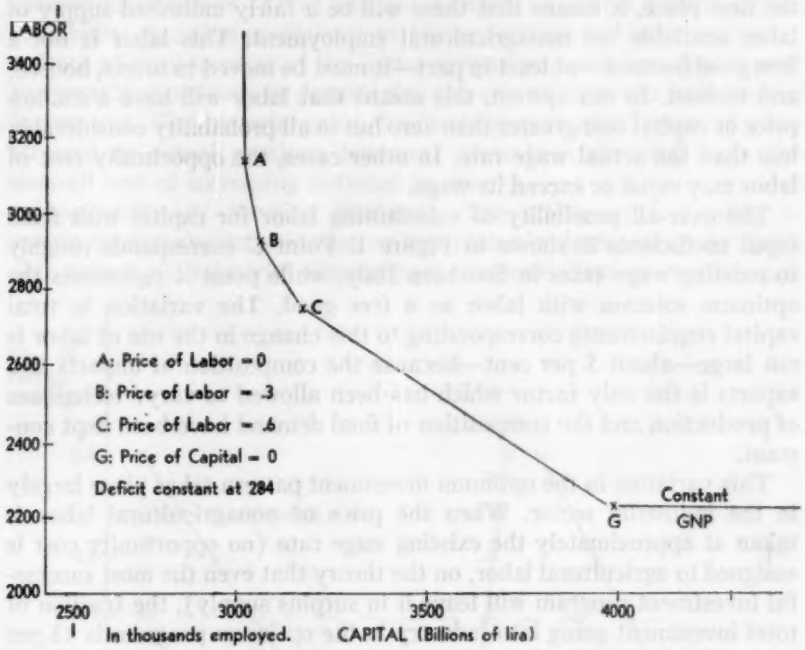

FIGURE 1. SUBSTITUTION BETWEEN CAPITAL AND LABOR

analyze effects of variation in the parameters related to three basic characteristics of developing economies: (1) factor proportions; (2) availability of foreign exchange; (3) the rate of growth.

Factor Proportions. The optimum pattern of economic development obviously varies widely with the endowment of available economic resources. Individual mineral resources, specific types of agricultural land, etc., are reflected in the capital requirements of individual sectors and determine the desirability of specific types of primary production. They require little comment. The relative abundance of larger categories of economic factors—aggregate natural resources, labor, and investment funds—is subject to more general analysis. The most sig-

nificant relationship is that between population or labor force as a whole and natural resources plus capital stock. It is often suggested that in the "overpopulated" areas, where pressure on the developed resources—primarily agricultural—is high, industrialization is more desirable than in countries where a shift of the labor force from agriculture to industry (without investment or technological improvement) would cause a significant fall in agricultural output.

Population pressure may affect several parameters in the model. In the first place, it means that there will be a fairly unlimited supply of labor available for nonagricultural employment. This labor is not a free good because—at least in part—it must be moved to towns, housed, and trained. In our system, this means that labor will have a shadow price or capital cost greater than zero but in all probability considerably less than the actual wage rate. In other cases, the opportunity cost of labor may equal or exceed its wage.

The over-all possibility of substituting labor for capital with fixed input coefficients is shown in Figure 1. Point C corresponds roughly to existing wage rates in Southern Italy, while point A represents the optimum solution with labor as a free good. The variation in total capital requirements corresponding to this change in the use of labor is not large—about 5 per cent—because the composition of imports and exports is the only factor which has been allowed to vary; techniques of production and the composition of final demand have been kept constant.

This variation in the optimum investment pattern takes place largely in the industrial sector. When the price of nonagricultural labor is taken at approximately the existing wage rate (no opportunity cost is assigned to agricultural labor, on the theory that even the most successful investment program will leave it in surplus supply), the fraction of total investment going into industry in the optimum program is 43 per cent. ("Industry" includes mining and manufacturing and excludes power and other services. See Table 4 above.) When both types of labor are "free," the optimum share of industry is increased to 55 per cent. (These proportions refer to "productive" investment, which is perhaps 70 per cent of the total investment taking place.)

Population pressure is also likely to be reflected in the relative levels of the marginal capital coefficients in agriculture and industry. In an overpopulated area, land is utilized more intensively in agriculture, and the capital required to extend the land under cultivation through reclamation, irrigation, etc., is greater. Densely populated areas are also likely to have fragmented land holdings which make mechanization less productive. To take two rather extreme examples, the marginal capital requirements per unit of output in agriculture in Southern Italy

are perhaps three times as high as they are in Turkey. There is little difference in the capital requirement in industry, however.

A variation in the capital/output ratio in agriculture will be reflected in changes in the desirable levels of investment in all the industrial sectors if a given increase in GNP is to be achieved. In the present instance, a lowering of the agricultural capital coefficient by about 15 per cent produced a decrease in the share of investment going to industry of about 20 per cent. Only the food processing industries increased their output. These particular values of course depend on the variability of capital coefficients within agriculture and industry.

Both of these aspects of the factor proportions problem thus appear to be quite significant in determining the role to be given to industrial investment. The importance of the "employment ratio" may be overstressed in actual practice, however, when it is divorced from the over-all test of increasing national income.

Availability of Foreign Exchange. The ability of a country to secure foreign exchange is another key factor in determining the desirable extent of industrial development. Industrialization consists pri-

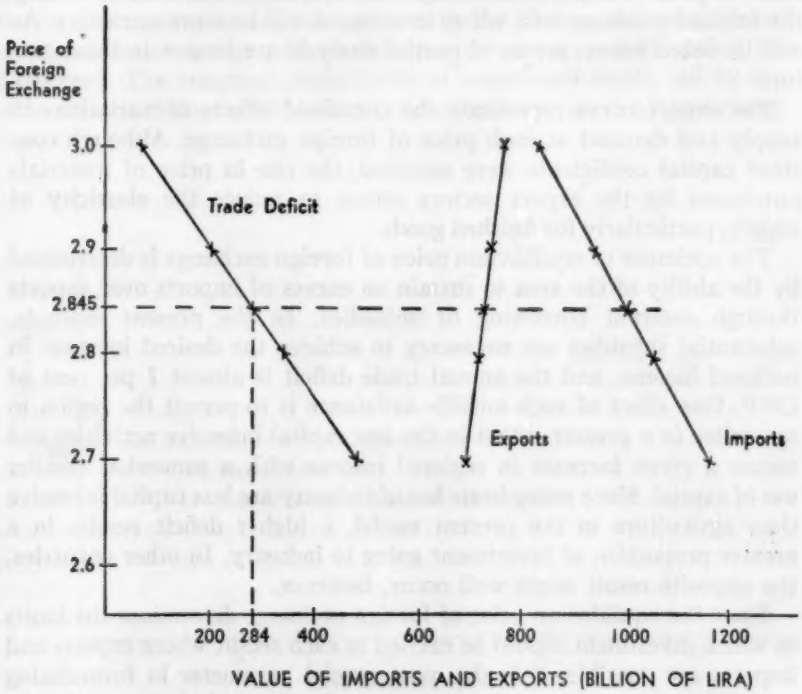

FIGURE 2. EFFECT OF THE PRICE OF FOREIGN EXCHANGE ON THE TRADE DEFICIT

marily in the substitution of domestic production of manufactured goods for imports. The choice between domestic production and imports should be based on the scarce resources required by each alternative. In a linear programing model, this decision is based on the shadow price of foreign exchange, which is determined by the opportunity cost of domestic production in the marginal importing and exporting sectors. In equilibrium, the price of foreign exchange becomes the reciprocal of the marginal productivity of investment in these sectors.

The effect of changes in the price of foreign exchange on imports and exports is shown in Figure 2. The import curve represents the total demand of the economy for imports, taking account of optimum readjustments in domestic production at each price of foreign exchange.

The effect on the investment decision in any sector of a rise in the price of foreign exchange will be determined by three factors: (1) the rise in the price of imports of the commodity produced; (2) the variation in direct capital and labor costs in the sector; (3) the rise in the cost of materials purchased from other sectors. Investment in other sectors will cause the price of purchased materials to rise less than the price of imports. These external economies will affect primarily the finished goods sectors, where investment will be more attractive. As will be noted below, errors of partial analysis are largest in those sectors.

The export curve represents the combined effects of variations in supply and demand at each price of foreign exchange. Although constant capital coefficients were assumed, the rise in price of materials purchased by the export sectors serves to reduce the elasticity of supply, particularly for finished goods.

The optimum or equilibrium price of foreign exchange is determined by the ability of the area to sustain an excess of imports over exports through external borrowing or subsidies. In the present example, substantial subsidies are necessary to achieve the desired increase in national income, and the annual trade deficit is almost 7 per cent of GNP. One effect of such outside assistance is to permit the region to specialize to a greater extent in the less capital-intensive activities and secure a given increase in regional income with a somewhat smaller use of capital. Since many branches of industry are less capital intensive than agriculture in the present model, a higher deficit results in a greater proportion of investment going to industry. In other countries, the opposite result might well occur, however.

Since the equilibrium price of foreign exchange determines the limits to which investment should be carried in each sector where exports and imports are possible, it is the most crucial parameter in formulating a development program. If too low a value is used, for example, the

trade deficit will be larger than can be financed and emergency measures to stimulate exports or reduce imports will have to be taken. These will lead to investment at lower rates of productivity than would be the case if the proper exchange rate had been used throughout the program. The resulting increase in total cost of the program is illustrated in Section III below.

The Rate of Growth. The rate of growth affects the pattern of investment in two different ways. In the first place, foreign exchange earnings are fairly independent of the rate of growth. Except for improvements in technology or possible economies of scale in export production arising from a larger domestic market, foreign exchange earnings depend on the exchange rate and the elasticity of foreign demand for such exports. Imports, however, will increase with the rate of growth unless the exchange rate is changed, as shown in Figure 3. Even if foreign borrowing depends to some extent on the growth in national income, there will be a tendency for the net deficit to be greater at higher rates of growth.

In order to limit the trade deficit of foreign exchange availabilities a higher price must be given to foreign exchange. In the present example, an increase in the rate of growth by 10 per cent would have increased the exchange rate needed to maintain a constant deficit by 2 per cent. The marginal productivity of investment would fall by the same amount. Although this effect is not significant for small variations,

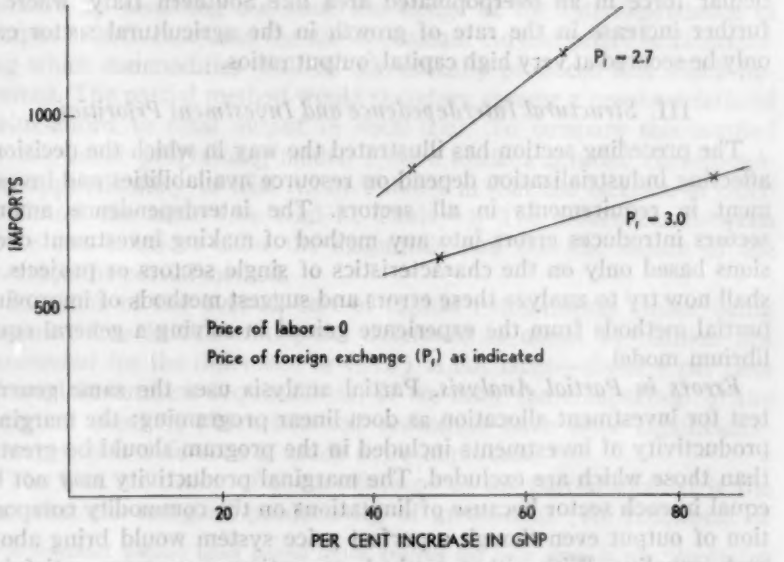

FIGURE 3. EFFECT OF GROWTH ON IMPORT REQUIREMENTS

when the growth rate is greatly accelerated it may become quite important. Higher growth rates lead to a greater need for self-sufficiency unless export demand is highly elastic.

The rate of growth will also act on the composition of investment through its effects on final demand. As per capita incomes increase, the proportion of income spent on manufactured consumer goods of all kinds may be expected to increase. The study of consumption in Southern Italy on which the final demand in the present model is based showed an income elasticity of demand of 1.35 for all manufactured goods (except food). The proportion of national income spent on investment goods is also likely to increase. Both of these factors will lead to an increasing proportion of investment in industry.

Both the increased demand for imports and the shift in the composition of final demand to more manufactured goods tend to give an increased importance to industrial investment at higher rates of growth. The increased pressure on foreign exchange may affect the desirability of investment in agriculture as much as in industry in some countries, but there are more likely to be technological limits to a rapid increase in agricultural output through increased investment. Throughout this discussion I have left out of account technological improvement in agriculture, which may be one of the most important elements in increased output. The rate of technological progress in agriculture is limited by other factors, however. This conclusion appears to have particular force in an overpopulated area like Southern Italy, where a further increase in the rate of growth in the agricultural sector can only be secured at very high capital/output ratios.

III. *Structural Interdepedence and Investment Priorities*

The preceding section has illustrated the way in which the decisions affecting industrialization depend on resource availabilities and investment in requirements in all sectors. The interdependence among sectors introduces errors into any method of making investment decisions based only on the characteristics of single sectors or projects. I shall now try to analyze these errors and suggest methods of improving partial methods from the experience gained in solving a general equilibrium model.

Errors in Partial Analysis. Partial analysis uses the same general test for investment allocation as does linear programing: the marginal productivity of investments included in the program should be greater than those which are excluded. The marginal productivity may not be equal in each sector because of limitations on the commodity composition of output even though a perfect price system would bring about such equality. With either method, an optimum program satisfying

the various resource limitations can only be arrived at by trial and error. There are various routes by which the final solution can be reached, but a convenient method in both cases is to assume a marginal productivity of investment, to exclude projects falling below this figure, and then to see whether investment funds have been exhausted and other limitations satisfied. In the partial analysis it is possible to take account of some errors in the price system, such as the existence of a tariff on imports of the product, but not of the indirect effects of such errors on the price of purchased materials.[6]

As has been pointed out earlier, the partial analysis is subject to two general types of error arising out of interconnectedness: inability to determine how much of a given commodity will be required and the omission of external economies due to investment in other sectors. The first type of error is probably of greater quantitative significance. To measure it would require a comparison of the output forecasts which would be made in ignorance of the input-output pattern with the results of the present model. These errors would be small for finished goods and larger for intermediate products and producer services. Since the latter sectors have higher capital coefficients, the errors in estimating investment requirements would be greater than those in forecasting output. They cannot be measured here for lack of a basis of comparison, however.

The second type of error—omission of external economies—can be isolated by assuming the same knowledge of the interdependence of output in both cases and the use of constant current prices in determining which commodities shall be domestically produced and which imported. The partial method would therefore assume a constant ratio of value-added to total output in each trial. To compare this method to the linear programing model, I have made a solution corresponding approximately to trial A5 in Table 4 in that the increase in GNP is almost the same (see trial E, Table 4). The differences between these two solutions show the magnitude and also the nature of the errors in the partial method.

In terms of the over-all test of capital requirements (labor being assumed free in this trial), the difference of about 115 billion lira (corrected for the difference in GNP) is not large—about 4 per cent of total investment requirements. If we limit the comparison to the marginal cases in which external economies have some significance, however, this difference is much more important.

The best indication of the inefficiency of the partial method is the fact that the marginal productivity of investment (the reciprocal of

[6] A suggested method using partial sector analysis has been given in my "Application of Investment Criteria," *Quart. Jour. Econ.*, February, 1953, pp. 76-96.

the price of foreign exchange) with the partial method is much lower—28 per cent as compared to 35 per cent for linear programing. This difference results from the fact that external economies are not taken into account and underinvestment in some sectors results. In other sectors, investment must be pushed to lower levels of productivity to secure the same increase in GNP. There will be underinvestment in the sectors which purchase a large amount from other sectors, as in the production of finished goods. Textile production in the present case was 40 per cent lower with the partial method than with linear programing. Conversely, there will be overinvestment in primary production and basic industries to compensate for the first error; agriculture is 30 per cent higher with the partial method.

Regardless of the values of the various parameters, there will be this systematic bias in favor of primary production and basic industries and against finished goods when external economies are ignored. These biases also extend to the export sectors.

Planning Industrial Development. To improve on partial analysis, some of the effects of structural interdependence must be taken into account. The methodology which has been used here may be helpful even when all the elements are not known. This method takes advantage of the fact that interdependence in the economy is not general, but is largely "one way." The demand for basic materials depends on the demand for finished goods; the demand for minerals and agricultural products depends on the first two categories; and, finally, the demand for fuels, power, transport, and producer services depends on all of the others. The extent of this one-way interdependence in the Italian economy is shown in Table 1; less than 5 per cent of the interindustry flows occur to the right of the diagonal when the sectors are arranged in such a way as to minimize such circular interdependence. To compute output requirements we should start with the sectors at the top of the list, while to trace out the effects of external economies on prices we should start at the bottom.

A procedure which will introduce some of the elements of a general equilibrium analysis into the usual sector approach is the following:

1. Divide the economy into as many sectors as can be managed and arrange them in the order of maximum one-way interdependence as indicated in Table 1.

2. Within each sector, rank projects according to their marginal return on investment, using expected prices and correcting for tariffs, subsidies, etc., where possible.

3. Estimate the final demands in the economy which would result from the increase in income expected from the investment program.

4. Using an assumed marginal productivity of investment, exclude

projects in each sector which have lower returns, wherever there is the alternative of importing. Include all export projects which exceed this rate of return. Start at the bottom of the list in making this estimate (with services and primary production) and revise the cost of purchased materials where possible on the basis of previous decisions as to imports or domestic production.

5. Starting at the top of the list of sectors, compute the required output in each from the final demand and the estimated use in preceding sectors. Compute imports, exports, and the payments deficit.

6. Revise the size of the program downward if the available investment funds have been exceeded. Revise the productivity of investment downward if the trade deficit is excessive. Repeat the calculation until limits on investment, foreign exchange, and other resources are satisfied.

It would require a paper much longer than the present one to explore the practical problems involved in each of these steps. They will vary greatly depending on the type of information available. One great advantage of using such a framework for investment analysis, however, is that it can incorporate whatever fragmentary information is available from a variety of sources—engineering studies, demand analyses, import statistics, etc. This approach merely makes the judgments which will be made in any case more consistent.

IV. *Conclusion*

The role of industrialization in economic development cannot be evaluated by looking only at individual sectors. This paper has explored the ways in which our judgments may be modified by looking at the total effects of an investment program. To make this investigation, I have had to abandon the *ceteris paribus* assumptions of partial equilibrium analysis and work with a general equilibrium system. Although some of the parameters are estimated from very inadequate evidence, the most important elements—the input coefficients—are the most reliable. Few of the above conclusions would have been changed by large deviations in the other parameters.

Knowledge of structural interdependence is particularly important in the industrial sectors of the economy. The external economies which occur in these sectors provide the justification for investment in "social overhead" facilities and in much of primary production. The desired level of output in each industrial sector depends on developments in the others. We have seen that factor proportions, comparative advantage, and the rate of growth all affect the amount and type of industrial investment which is desirable. My main result, therefore, has been to stress the need for over-all analysis in planning industrial development.

INVESTMENT IN AGRICULTURE IN UNDERDEVELOPED COUNTRIES

By WILLIAM H. NICHOLLS[1]
Vanderbilt University

"Ill fares the land, to hastening ills a prey,
Where wealth accumulates, and men decay."
—GOLDSMITH, *The Deserted Village*

In this paper I shall assume that development policy in underdeveloped countries has, as its minimum objective, an increase in per capita real national income. However, in considering the place of agricultural investment in general economic development policy, I prefer to use a more rigorous standard of economic progress which also takes the distribution of the (larger) national income into account. More specifically, I shall define economic progress as a rise in per capita real income which is in some degree shared by every sector of the economy. This means that I am unwilling to accept as economic progress the alleged gains (essentially a statistical mirage) which may result from subsidizing a parasitic type of development in one sector by means of direct or indirect levies on other sectors, which are made worse off in the process. It also means that, at least in a long-run context, I insist that agriculture and its major subsectors must share in any real gains in per capita national income if we are to consider the claims of successful economic development valid. At the same time, I shall argue that—quite apart from such equity considerations—sustained economic development is impossible if it is sought on the basis of agricultural policies which promote the interests of a few of the larger commercial farmers without concomitant attention to progress within the broad base of essentially subsistence agriculture so characteristic of most underdeveloped countries.

Due to the limitations of space and my own foreign experience, I have decided to confine my treatment of an extremely broad subject to a case study of one prominent underdeveloped country, the Republic of Turkey. I shall review Turkey's remarkable expansion in agricultural output in recent years, shall seek to appraise the extent to which its ambitious agricultural development program has contributed to this expansion, and shall conclude with a discussion of the lessons to be learned from Turkey's recent experience.

[1] I am indebted to G. E. Brandow, Murray Ross, T. W. Schultz, Frank T. Bachmura, Anthony M. Tang, and Roger Weiss for their helpful criticisms of an earlier draft of this paper.

I. *Recent Agricultural Development Policies in Turkey*[2]

When I was in Turkey in 1950 as a member of the International Bank mission, even Turkey's limited industrial output was piling up in inventories because of lack of purchasing power on the part of its dominant peasant population. The 1949 cereal harvest had been extremely poor, resulting in a sharp reduction in national income, and several hundred thousand tons of wheat had to be imported. Partly under the prodding of the ECA mission, accompanied by considerable financial and technical assistance from abroad, the Turkish government had already embarked upon a substantial agricultural development program. This program put major emphasis upon farm mechanization and high cereal price supports. Mechanization was justified on the grounds that it would put Turkish wheat and cotton on a substantial export basis by which the foreign exchange for farm machinery (including spare parts and fuel) might be more than recouped. The two agricultural specialists of the Bank mission, of which I was one, were extremely skeptical of the prospects that mechanization and price supports would prove to be very effective means of expanding agricultural production.

Our skepticism resulted from the fact that the typical Turkish peasant had only ten to fifteen acres of land, which appeared to be far too small to make mechanization economically feasible or high price supports a very effective stimulus to increased output. Even on larger holdings, fragmentation often offered a further deterrent to mechanization. Hence, our Bank mission report emphasized the importance of improving rural transportation, communication, and marketing facilities so that Turkey's several million peasant families might be more fully integrated into a single national market economy. We also strongly recommended expanded and improved agricultural research and extension services, public farm credit, and direct subsidies for the adoption of recommended farming practices, directed at helping the peasant increase his output from his few acres of land.

In making these recommendations, we assumed that, given its prevailing primitive production and marketing techniques and small landholdings, Turkish agriculture had a very low short-run elasticity of supply. Hence, we supported longer run public policies which we believed would lower its whole supply schedule. However, we emphasized

[2] This section draws heavily upon the following source materials: G. E. Brandow, *Agricultural Development in Turkey*, Foreign Operations Administration, Ankara, Dec., 1953, mimeo., pp. 2, 7, 9, 11-21, 26, 36-37, 40-42, and 55; *The Economist*, May 15, 1954, Special Insert on "Turkey," pp. 1, 4-5, and 11; International Bank for Reconstruction and Development, *The Economy of Turkey*, Report of a Mission, Washington, 1951, especially Chap. V; International Monetary Fund, *International Financial Statistics;* United Nations, *Statistical Papers*, Series H; and U. S. Dept. of Agric., *Foreign Crops and Markets* and *The Wheat Situation.*

the importance of finding improved techniques which were not laborsaving and which, being adapted to small-scale operations, did not need to await farm enlargement in order to raise agricultural productivity. On the other hand, we seriously questioned the use of high price supports, either with existing primitive techniques or with government promotion of new techniques which would substitute much scarce capital for an already redundant labor supply while benefiting most a relatively few medium to large landholders.

Despite our recommendations, the Turkish government continued to put major emphasis upon farm mechanization and high price supports. The number of tractors, which had already grown from 2,500 to 10,000 during 1948-50, continued to increase to 35,000 (well in excess of Brazil's 22,000-30,000) by 1953. Furthermore, following the upsurge in world wheat prices resulting from the Korean war, the Turkish government continued to advance its very high price supports on cereals as export prices weakened substantially after 1951.

Of course, Turkey's agricultural development program was not limited to mechanization and price supports. Between 1948 and 1952, the Agricultural Bank of Turkey increased its outstanding public farm credit (albeit by inflationary means) more than fourfold, with about one-third of the increase in direct loans to farmers earmarked for machinery and equipment purchases. The excellently managed road-building program helped to bring more Turkish farmers and idle land into the market economy. The government distributed about 1.8 million acres of previously uncropped land to families and villages, and the amount of land under irrigation increased by about 15 per cent. The public seed-improvement program made considerable progress and the supply of fertilizers (two-thirds imported) increased more than fivefold. Agricultural research and extension activities also were gradually expanding but, with the major appropriations going to the independent price support and agricultural credit agencies, the 1953 budget of the Ministry of Agriculture was still only about 25 million dollars.

Whatever the combination of reasons, it is certain that Turkey did enjoy a remarkable expansion in agricultural output during 1948-53. Cereal production increased from 9.0 to 14.3 million metric tons and cotton lint production from 58 to 140 thousand metric tons. Wheat exports rose from nil to 587,000 tons, Turkey (thanks to Argentina's default) achieving at least temporarily fourth place among the world's wheat-exporting nations. Cotton exports rose from 16,000 to 100,000 metric tons. In 1952, the index of Turkish agricultural production was 47 per cent above its 1948 level, and the index of industrial production was 33 per cent higher (a year later 49 per cent higher). According

to the *Economist*, Turkish national income (in current prices) increased from 8.1 to nearly 12 million Turkish liras during 1948-53 while per capita national real income increased 21 per cent during 1950-53. If these data are correct, they indicate that per capita real income—which approximated its 1938 level in 1948 and 1950 and stood at only 84 per cent of 1938 in the bad harvest year 1949—rose spectacularly during 1950-53.

Frankly, I suspect that the actual rise in Turkey's per capita income was considerably less. At least it is quite possible that faulty price deflators and the dominant importance of agriculture (with its heavy dependence upon natural conditions which were extraordinarily favorable during 1951-53) have served to exaggerate the extent and permanence of Turkey's recent economic progress. Furthermore, as we shall see later, nature and economic events have taken a less favorable turn during the past year. Nonetheless, having seen and sensed the great strength of Turkey's will to progress, I feel confident that—however uncertain the quality of Turkish national statistics—the recent pace of its economic development has in it much of real substance. Hence, an appraisal of the virtues and shortcomings of Turkey's recent agricultural development policies would appear to be in order. Before doing so, however, let us look briefly at the recent expansion of two major Turkish crops—cereals and cotton—and attempt to assess the factors responsible for the increased production of each during 1948-53.

Cereals, produced under semiarid conditions, account for around 85 per cent of Turkey's cropland harvested and for perhaps 40 per cent of its net agricultural income. In his recent study, Professor Brandow found that a significant part of the sharp increase in Turkish cereal production during 1948-53 was due to weather, which was unusually favorable during 1951-53. Thus, he estimated that 1953 cereal yields were 22 per cent higher than normal weather yields with improved practices then in use; that weather alone caused 1953 yields to be about 10 per cent above 1948, when growing conditions were also above average; and that, in conjunction with increased area, this higher yield accounted for nearly one-fourth of the increase in cereal output during 1948-53. Supporting Brandow's judgment about the importance of weather, the 1954 cereal crop (wheat and barley), with much less favorable weather, yielded one-third less per acre than in 1953. In light of previous experience, one would expect this decline in cereal production to be reflected in some decline in Turkey's national income for 1954. Nevertheless, Turkish cereal production in 1954 was more than double its 1949 low and, despite a 10 per cent lower yield, was still 26 per cent above its level in the good-weather year 1948. Thus, quite independent of the weather factor, cereal production has shown a marked increase since

1948—primarily because of the sharp increase in the area planted to cereals. During 1948-53, cereal acreage increased by 7.4 million acres (nearly 40 per cent), largely at the expense of poor-quality grazing lands and unused fields.

Undoubtedly, the most important factor in bringing about this increased area in cereals has been the Turkish government's high price support program. For the crops of 1952-54 (and, prospectively, for the 1955 crop), the government's *Toprak* office has been supporting wheat prices at levels about a third higher (at the official exchange rate) than present high US price supports. The effects of these supports on increased acreage have been further enhanced by improvements in transportation and a doubling of the number of government grain-buying stations. Although only about 12 per cent of the 1952 cereal acreage was cultivated with tractors, mechanization also contributed to the increased area by making possible the tillage of a larger area of cropland. Mechanization also helped to raise yields per acre, although only a strikingly small part of the increase in total cereal output was attributable to yield-raising practices (improved seed, fertilization, etc.) other than those associated with mechanization. According to Brandow's estimates (*op. cit.*, pages 40-41), with normal weather throughout 1948-53, expected cereal output in 1953 would have been 43 per cent higher than 1948 expected output. Brandow estimates that each of the following factors in isolation would (with normal weather) by 1953 have added the indicated percentage to 1948 normal output: favorable prices and markets (increased area), 12 per cent; mechanization (effects about equally divided between "increased area" and "improved practices"), 10 per cent; improved transportation (increased area), 5 per cent; seed improvement, 2 per cent; fertilization negligible; and area-increasing intangibles 14 per cent. Of the estimated 43 per cent increase in cereal output with normal weather, five-sixths was credited to increased area and only one-sixth to improved practices.

Cotton lint production in Turkey increased nearly threefold between 1948 and 1952, its acreage more than doubling and its yield per acre 25 per cent higher in the latter year. In 1953, with some cutback in acreage and yields, production was about 15 per cent lower (616,000 bales) but still about 2.4 times its 1948 level. Undoubtedly, a major part of this increased output was due to the tremendous boom in cotton prices which the Korea outbreak created. During 1950-51, Turkish cotton prices more than doubled and acreage expanded sharply as the United States put a temporary embargo on its own cotton exports. While subsequent declines in prices brought some contraction in acreage in 1953 and 1954, Turkish cotton acreage and production remained

well above their 1950 level and far above 1948. Meanwhile, mechanization of cotton proceeded at a rapid pace, 56 per cent of the cotton in the Mediterranean and Aegean regions being cultivated with tractors by 1952. Brandow has estimated that the normal-weather yield of seed cotton increased about 7 per cent during 1948-53 as a result of the use of improved practices on a greater proportion of the land in cotton. Meanwhile, the area in cotton doubled between the two years. Of the factors (other than intangibles) accounting for the increased area in cotton, he gives about equal credit to favorable prices and mechanization. Of the factors representing improved practices, he gives major weight to mechanization, with irrigation, seed improvement, and fertilization also making significant contributions to the increased output. On the assumption of normal weather throughout 1948-53, Brandow estimates (page 42) that each of the following factors in isolation would by 1953 have added the indicated percentage to 1948 normal output: mechanization, 53 per cent (via increased area, 24 per cent; via improved practices, 29 per cent); improved seeds, 10 per cent; fertilization, 6 per cent; and area-increasing intangibles, 8 per cent. Of the estimated 115 per cent increase in cotton output with normal weather, half was attributed to increased area and half to improved practices. However, Brandow emphasized that he assigned to increased area only the production that would be obtained from it by primitive methods. Hence, as he explicitly recognized, his methodology proved awkward for cotton, appearing to understate the importance of area.

So much said by way of summary and background, let us turn to an appraisal of Turkey's recent agricultural policies.

II. *An Appraisal of Recent Turkish Agricultural Policies*[3]

Turkey's farm mechanization program has probably received the most popular credit for the nation's recent agricultural progress. To what extent has this farm mechanization program been sound development policy? On the credit side, mechanization has made a significant contribution to both the area under cultivation and to higher yields. Brandow has estimated that about 17 per cent of the new land added to cultivation of field crops (17 per cent for cereals; 32 per cent for cotton) during 1948-52 was developed by tractors. A recent University of Ankara survey of mechanized Turkish farms found that—because of more effective, better timed, or more frequent tillage and seeding

[3] Most of the data in this section are derived from Brandow, *op. cit.*, pp. 9-10, 16, 23-24, 26, 41-42, 45, 53-54, and 56; and from *Economic and Social Aspects of Farm Mechanization in Turkey*, A Study by a Committee of the Faculty of Political Science, University of Ankara, Sponsored by the MSA (FOA) Mission to Turkey, Ankara, 1953, mimeo., pp. 6-8, 14-18, 20, 25, 30-36, 48-50, 62, 64-69, 70-76, 73-79, 83, 86-89, 91, 94, and 96-98. The latter survey, made in mid-1952, included a sample of 3,015 mechanized farm families, constituting about 4 per cent of all families in 448 of Turkey's most mechanized villages.

operations—mechanization produced substantially higher yields than did primitive techniques. Brandow's estimates also suggest that, exclusive of the weather factor, 23-25 per cent of the 1948-53 increase in "normal" output of cereals and cotton was attributable to mechanization. Furthermore, through 1952, Turkey's net investment in farm machinery had probably more than paid for itself in terms of increased foreign exchange earnings, net of costs of importing replacements, spare parts, fuel, and oil.

However, with export prices falling, with the existence of export surpluses of cereals highly dependent upon the vagaries of weather, and with the larger farms now substantially mechanized, these factors hardly justify a comparable rate of additional mechanization in the future. In fact, it is quite possible that mechanization of Turkish agriculture has already been carried too far. In 1952, the annual amount of cropland (owned, rented, or worked on a custom basis) cultivated per tractor on Turkey's mechanized farms was probably about 160 acres and the undepreciated machinery investment per acre of cultivated land was about $27. But 43 per cent of Turkey's tractors were in regions whose mechanized farms averaged only 49-86 acres per tractor. To be sure, the remainder of the tractors were in three regions with 165-496 acres per tractor. In the regions having the greatest numbers of relatively large farms, machinery investments per acre approximated that of medium to large commercial farms in the United States. Undoubtedly, many of the larger Turkish farms (if not too much fragmentized) can utilize existing farm machinery rather fully and might invest profitably in still more machinery, particularly complementary tractor-drawn equipment which has lagged behind the expansion in tractor numbers in Turkey. Nonetheless, it is clear that many of Turkey's mechanized farms are already well below the size considered necessary for reasonably efficient utilization of machinery in the United States, where labor is relatively much scarcer and where machinery and fuel are much less expensive and require no foreign exchange. Hence with only about 3 per cent of Turkey's 2.5 million farm families holding over 123.5 acres (a third or more in fallow), the number of tractors in Turkey would appear to be close to the saturation point pending substantial farm enlargement.

Through 1953, the Turkish farm mechanization program had probably directly benefited only 25,000-27,500 (scarcely over 1 per cent) of Turkey's farm families. These few families probably enjoyed average annual gross cash incomes in excess of $15,000 each and were the recipients of at least 25 per cent of the public farm credit outstanding to all Turkish farmers at the end of 1952. Such an initial concentration of agricultural credit upon the medium to large farms, whose produc-

tion can most readily respond to improved conditions, was probably wise. The extent to which such intermediate-term farm credit should have been almost wholly confined to machinery and equipment loans is less certain. Insofar as mechanization brought additional land into cultivation which would otherwise (at constant prices) have remained virtually unused, the value of additional output in excess of machinery investment and operating costs was a clear gain to the nation, since no agricultural workers needed to be displaced by the machinery. During 1948-52, the average tractor-owning family in Turkey increased its average acreage operated, exclusive of custom work, by 31 per cent. Brandow has estimated that two-thirds of this increase in cultivated area was previously uncultivated. To the extent that this was true, Turkey's mechanization program would appear to be largely warranted, assuming that the new land would not otherwise have been brought under cultivation.

On the other hand, to the extent that mechanization resulted in tractor-owning families acquiring additional land previously cultivated by others, the answer is much more complex. In this case, the increase in gross value of product per acre—whether by an increased yield of the same crop or by the substitution of a more intensive crop—attributable to mechanization is less likely to exceed the net costs of the mechanized techniques. Furthermore, the social problem of displacing agricultural workers by machinery may well arise. Of course, insofar as tractor-owning farmers acquire by rental or purchase the use of previously cultivated land from owner-operators, the displacement is presumably voluntary, with the former operators somewhat better off than before. However, the same is not true if the additional land acquired by mechanized farmers was previously cultivated, for these or other landlords, by tenants and sharecroppers. Under the latter circumstances, the mechanized farmers might achieve large economic gains while displacing former tenants or sharecroppers who lack alternative employment opportunities.

The Turkish mechanization program has apparently already unduly encouraged such deleterious social effects. With the increasing pressure for additional land in Turkey's mechanized villages, land disputes have become more frequent, while marked changes in land tenure have followed mechanization. Sharecropping has decreased while cash rental of land has increased, with a larger proportion of landowners working their own land since mechanization. At the same time, the percentage of mechanized farmers who hire farm workers has increased but the number of full-time hired workers has tended to decline in favor of temporary hired labor during the busy seasons only. With mechanization increasing the seasonality of labor requirements, temporary migra-

tory labor has become more important, and cash wages have largely displaced wages in kind. Thus, insofar as Turkish mechanized farmers have taken over land previously cultivated by tenants or sharecroppers, the latter appear to have suffered a steady reduction in their share of the crop, usually ending either in a shift to the status of seasonally hired day labor or in complete displacement.

Upon the basis of the University of Ankara survey, I have estimated that each new tractor during 1948-52 resulted in the gross displacement of about 3.4 sharecroppers. Projected to all Turkey, these estimates suggest that around 79,000 sharecroppers (71 per cent of them in the two major cotton regions) were so displaced, about 64,000 of them remaining in their villages. By regions, the displacement ratio was 8.9 sharecroppers per tractor in the Mediterranean, 5.9 in Southeastern Anatolia, 0.8 in Central Anatolia, and 1.8-2.4 in the other three survey regions. Of course, some of the sharecroppers—especially those who were able to obtain well-paid and *nouvelle élite* jobs as tractor drivers or mechanics—improved their well-being as hired workers. Particularly, the doubling of cotton acreage was an important offset to otherwise declining labor requirements, and it is significant that the survey villages apparently experienced considerable net in-migration during 1948-52. Nonetheless, against the gains through increased yields per acre to the mechanized farmers, some of their hired workers, and the nation must be weighed the probable substantial net increase in numbers of underemployed villagers who, lacking alternative farm or nonfarm employment opportunities, have had considerable reductions in income. This income-distribution problem has been further aggravated by the fact that even the wealthiest Turkish farmers pay no income taxes on soil products and only negligible property taxes.[4]

To sum up, the Turkish farm mechanization program has probably been justified to the extent that it has been an independent factor in increasing the nation's total cultivated area. Where it has involved taking over land previously cultivated by owner-operators, its economic contributions are less certainly positive but its social costs have been negligible. But insofar as it has displaced tenants and sharecroppers

[4] Cf. Ankara Faculty, *op. cit.*, pp. 48-50; and Richard D. Robinson, "Tractors in the Village—A Study in Turkey," *Jour. Farm Econ.*, 1952, pp. 451-462. By way of contrast, mechanization of cotton in the Southern United States has thus far avoided these important social costs. While the effects of mechanization upon tenure status have been strikingly similar in Turkey and the American South (cf. Harald A. Pedersen and Arthur F. Raper, *The Cotton Plantation in Transition*, Miss. Agr. Exp. Sta., Bul. 508, 1954), few if any sharecroppers have been displaced in the United States. Instead, as better job opportunities have developed elsewhere, largely outside of agriculture, Southern sharecroppers have voluntarily migrated on a large scale. With agricultural labor becoming scarcer and wage rates higher, Southern plantation owners have been forced to mechanize in economic self-defense.

without concomitant expansion of better alternative employment opportunities, it has already pressed beyond the limits of Turkey's present socioeconomic structure. Certainly, any further increase in the number of what might be called "Bayar's oxen" (tractors) should largely await more "Ataturk minarets" (smokestacks).

Nevertheless, the recent great influx of tractors (less than one-fourth of them via Marshall Aid) has become the symbol of Turkey's agricultural renaissance. And one should not dismiss too lightly the importance of symbols in inspiring an underdeveloped country with a strong will to make progress. However, the arrival of the tractor has not directly helped Turkey's 2.5 million small farmers, of whom a substantial majority still use wooden plows and a negligible number use even simple animal-drawn grain drills. At the same time, the tractor has diverted far too much public attention and resources away from the peasantry which constitutes 65-75 per cent of the Turkish population.

Thanks to a combination of favorable circumstances (including the fortuitous events of the Korean commodity price inflation and extraordinarily good weather), Turkey has achieved a remarkable agricultural expansion since 1950. In view of this success, the recommendations of our Bank mission report respecting Turkish agricultural development undoubtedly appear, with the benefit of hindsight, to have been overly cautious and pessimistic. It is clear that we underestimated the opportunities for expanding rapidly Turkey's total area under cultivation. We recognized in 1950 that Turkey faced considerable difficulty in even maintaining its modest level of per capita food consumption. We further recognized Turkey's heavy dependence upon raw agricultural land (in 1951 purchased inputs and depreciation amounted to only 19 per cent of Turkey's gross farm income) in expanding its food supply but failed fully to appreciate the possibilities of increasing the inputs of cultivated land. Apart from increased cultivated area and shifts from livestock to cereals, Turkey can increase its agricultural output relatively little by transferring resources from nonfarm sectors to farms because so large a part of its total resources is already employed in agriculture and because so small a part of its agricultural resources is supplied by the nonfarm sector which, in turn, cannot readily employ those agricultural resources (labor) in substantial oversupply. In the United States, on the other hand, less than 10 per cent of its resources are committed to agriculture and a large part of American agriculture's production expenses (two-thirds of gross farm income) represents inputs that are either purchased from, or can readily be employed in, the nonfarm sector. Under these circumstances, the United States can (with little change in the area of agri-

cultural land) quickly expand its food supply in time of war or other emergency whereas most underdeveloped countries (given a constant land supply) can expand theirs little if at all. (T. W. Schultz, "The Supply of Food in Relation to Economic Development," *Economic Development and Cultural Change,* December, 1952, pages 245-247.)

During 1948-53, while the total area planted to crops expanded from 23 to 32 million acres, the area in cereals grew from 20 to 27 million acres and, according to Brandow, may ultimately reach a maximum of 32 million acres. Clearly, most of this increased cultivated area must have been developed with traditional primitive methods, implying that many nonmechanized farmers were given the incentive (through higher prices and improved highways) to work harder than before. Furthermore, it demonstrates that the elasticity of supply was higher at relative farm prices above those prevailing in 1950 than we might have expected.

To a large extent, however, this kind of elasticity was not relevant because 1950 prices (particularly for cereals) were already too high, given existing high-cost, low-productivity production methods. In other words, much of the land recently brought under cultivation would have continued, except briefly during the Korea-induced price flurry, to be submarginal (with unchanged techniques) in the absence of the subsequent rise in already high cereal price supports. Furthermore, since the expansion of cereals was largely at the expense of pastures, a serious imbalance in Turkey's livestock economy is at hand. During 1948-52, the total number of sheep, goats, and cattle continued to increase in spite of a 20 per cent reduction in pasture area, resulting in intensified overgrazing even though moisture conditions (except in 1949) were very favorable. Hence, pasture and livestock improvement and expanded production of feed grains, largely neglected to now, loom even larger as a major problem for further agricultural development. Only insofar as the increased area in cultivation would have resulted, at constant or lower relative prices, from recent improvements in transportation, the growth of government buying stations, and mechanization (i.e., by factors which shift the supply curve to the right) can it be counted as a clear economic gain. Otherwise, only if the reduction of the supply in response to a lowering of price supports would be substantially less elastic than the actual expansion which followed higher price supports (a not unlikely prospect) can the expanded area be counted as a further partial gain to the Turkish nation. Hence, given the relatively small importance of improved practices (apart from those resulting from mechanization) in Turkey's recent agricultural expansion, our recommendations for increasing the productivity of existing cultivated land, pastures, and livestock need renewed emphasis at the present time.

III. *Lessons from the Turkish Experience*[5]

Clearly, Turkey deserves credit for recognizing, after a long period of forced-draft industrialization and sad neglect of its agriculture, that agricultural development is of co-ordinate importance in balanced economic progress. However, as the recent unfavorable turn of events in the Turkish economy is now making clear, the Korean war and several years of excellent weather were not unmixed blessings despite the temporary boost they gave to agricultural development in Turkey. While the Korean war gave a brief fillip to world wheat markets, the subsequent rise in world wheat stocks (at an all-time high in mid-1954) and the fall in world prices have pushed Turkey into increasingly heavy export subsidies, estimated at more than 40 million dollars on the 1953 wheat crop alone. Only thus were the Turks able to dispose of their wheat surplus abroad (much of it through unsatisfactory barter deals with Spain, Yugoslavia, and other non-EPU countries) in spite of its high price, poor quality, and lack of standardization. Furthermore, it now appears that Turkey's poor 1954 cereal crop has eliminated for the current year any export surplus, some 500,000 tons of wheat imports being in prospect to meet urban food needs. Similarly, during the 1953-54 season, Turkish cotton exports of 377,000 bales (60 per cent of domestic production), while at the second highest level in history, were down 14 per cent from a year earlier. More significant, the bulk of Turkey's cotton trade shifted from Western Europe to non-EPU countries with which it made barter arrangements, the latter's share increasing from 22 to 64 per cent in a single year.

Thus, the temporary opportunity to sell at high prices cereals and cotton of poor and unstandardized quality was permitted to divert attention from the importance of adopting promptly a modern system of grades and standards, in the absence of which Turkish exports now face strong resistance in world markets. Furthermore, cereal production has not yet been put on the more efficient basis necessary if it is to be able to continue at its recent high level without abnormally good weather and present huge public subsidies. In the process, the hope that wheat and cotton exports would play a large part in solving Turkey's foreign trade problem has been only partially realized, while the considerable potential of its livestock economy has continued to face official neglect. Meanwhile, in seeking to encourage agricultural expansion, the Turkish government has used centralized con-

[5] The data in this section are based upon Brandow, *op. cit.*, pp. 9-10, 53-56; Ankara Faculty, *op. cit.*, pp. 25, 30-31; *The Economist*, *loc. cit.*, pp. 1-2, 4-5, 10-12; and recent issues of United Nations, *Monthly Bulletin of Statistics*, and U. S. Dept. of Agric., *Foreign Crops and Markets* and *Foreign Agriculture Circulars*. Cf. *The Economy of Turkey*, *op. cit.*, especially pp. 67-73, and William H. Nicholls, "Domestic Trade in an Underdeveloped Country—Turkey," *Jour. Pol. Econ.*, 1951, pp. 463-480.

trol—exercised through the public grain monopoly and the nominally private large agricultural sales co-operatives—to short-cut many problems of transport and communication (e.g., grading and market news service) whose solution is a prerequisite to the development of efficient private farm marketing enterprises.

Despite the tremendous expansion of agricultural credit, the Turkish government has continued to concentrate (apart from machinery credit) on short-term production credit which has done little to build up the farmer's permanent productive capacity. Insufficient attention has been given to the evaluation of loan applications and to the supervision of the uses to which loans are put. As a result, the distribution of farm credit has been far more haphazard and less productive of improved farming practices, especially for small farmers, than it might have been. Similarly, high price supports have not benefited small peasants much because they have so little to sell. In fact, it is clear that, with half of its budget devoted to defense, Turkey has pushed agricultural development largely via the inflationary route. The manifold increase in credit, especially agricultural, and the method of financing the cereal subsidy and the stock-piling of cereals have greatly increased inflationary pressures. At the end of 1953, the supply of currency and demand deposits was 75 per cent above its 1950 level. The cost-of-living index (1948 = 100) moved from 104 to 112 during 1950-53 and then in the first half of 1954 climbed rapidly from 113 to 128. Despite the poor 1954 cereal crop, Turkey should be able to maintain during the current crop-year its per capita cereal consumption at somewhat above its 1948-49 level. However, the probable high income elasticity of demand for cereals among urban workers with rising money incomes may (despite large wheat imports) favor further inflation, especially if producers should maintain their recent level of cereal consumption at the expense of marketings.

By mid-1954, the Turkish government was forced by these unfavorable developments to embark on at least a limited anti-inflation program, largely aimed at restriction of nonessential imports. It is to be hoped that, in the process, it will also scale down to more manageable levels its overgenerous distribution of agricultural credits (particularly for machinery), its present excessive cereal price supports, and its more ambitious large-scale irrigation schemes, while imposing a heavier tax burden on at least its richer farmers. Such a reversal of recent inflationary policies, which helped to keep Turkey's Democratic Party in power by a thumping majority in May, will require much economic wisdom and political courage in high places. Nonetheless, it should now be clear that indiscriminate mechanization, credit expansion, and price inflation have not proved to be the easy way out that they once ap-

peared. While moderation of these policies might result in some contraction in present agricultural output, the social costs of subsidizing present inefficient farming techniques are far too heavy for so poor a country as Turkey to bear them much longer.

Such counsel does not mean an abandonment of Turkey's agricultural development objectives. Rather, it means a shift of emphasis to other means which will more adequately meet the fundamental needs of Turkish agriculture, particularly those of its dominant peasant population, over two-thirds of whom have landholdings of less than 12.3 acres and nine-tenths less than 24.7 acres per family. In my opinion, the solution lies in the Turkish government at last facing up to the plight of its villages by developing a direct, determined, and persistent attack on the low productivity which lies at the roots of village poverty. Like other underdeveloped countries, Turkey has tended to follow—first in industry, now in agriculture—what I would call the "showcase" type of economic development. That is, at the expense of the population at large, some small group of producers is heavily protected, subsidized, and otherwise favorably treated to become a symbol of progress in which few of their fellow citizens can share. A few islands of privilege in a sea of poverty are nonetheless a poor indicator of economic development. Genuine economic progress must be broadly based, usually requiring a radical reorientation in government policy toward much larger investments in rural welfare.

To a large extent, this means a substantial increase in public investments in the education, health, and practical agricultural knowledge of village people and in the technical personnel to make such investments effective. Particularly, the quality of present agricultural research personnel and programs needs much improvement and the achievement of a broader and more effective dissemination of research findings at the village level deserves top priority. Since a mere 1 per cent increase per year in Turkey's total agricultural output would pay for its present annual investment in the activities of its Ministry of Agriculture, far greater budgets (particularly for its extension service) appear to be warranted. These public services should give a major part of their attention to developing simple improvements in tools and practices which will raise productivity per worker without reducing present per acre inputs of labor or animals, which must in any case be maintained on small holdings. Nor, in Turkey's case at least, does it appear likely that an increased rate of net population growth in the rural areas will dissipate the resulting gains in output. If a choice must be made between maintaining the present levels of agricultural credit and improving and expanding the research and extension services offered to small holders, the latter should be given priority. However, such agricultural

credit as is extended to small holders should be closely co-ordinated with the recommendations of the extension service and might often be more effective if granted in kind (pure seeds, fertilizer, etc.) rather than in cash.

Apart from encouraging the widest possible adoption of practices which enable the peasant to produce more with his limited land resources, the extension service should also seek to find means of occupying profitably the otherwise idle time of village people. The development of village industries—such as farm tools and implements, hand-woven textiles and other handicrafts, and the simpler agricultural processing industries—recommends itself. So does the encouragement of co-operative endeavor on small-scale public works—such as village roads, schools, and local irrigation projects—with the government furnishing only the materials and technical assistance. Such projects have the advantage of low capital requirements, which with further economic progress will involve no important liquidation loss, while meanwhile improving the levels of living and working habits of otherwise underemployed villagers. At the same time, they can arouse in the villages the spirit of community self-help which, while now sadly lacking, is a cornerstone of general progress anywhere.

On the marketing side, Turkey needs to make more public agricultural investment in its commodity standardization and inspection program. It also needs to develop satisfactory current agricultural marketing and price statistics, agricultural outlook information, and research and extension in farm marketing and prices—basic public services which are essential if Turkish peasants and private middlemen are to be integrated into an efficient and well-organized national economy without continued centralized dominance and control of agricultural markets by the government itself. Even the large-scale agricultural sales cooperatives virtually owe their existence to the government, which has maintained rigorous control over their management and finances. The development of more democratic farmer co-operatives at the local level by an expanded extension service would appear to be highly desirable on both economic and social grounds. Finally, there is a need for a continuing program of industrialization in which the simpler agricultural processing industries, both co-operative and private, play a larger role than they have done in the past.

Upon the basis of Turkey's experience, I may sum up in a few words my conclusions on desirable directions for agricultural development policy in underdeveloped countries. In general, more agricultural investment should be allocated to people (intangibles) than to things (tangibles) but, insofar as it is allocated to things, it should usually emphasize the small and inexpensive rather than the large and spectacular. Simple

and obvious though such conclusions may appear to be, they are almost universally ignored by underdeveloped countries. However, in so doing, underdeveloped countries confuse the symbols with the substance of the industrial nations of the world. Important though symbols may be in sparking a spirit of progress, their continued overemphasis can only mean the substitution of recurrent boom-and-bust for the slow-but-sure. Conservative that I am, I would strongly recommend to such countries the latter course.

DISCUSSION

EVERETT E. HAGEN: These two stimulating papers bracket a wide range of problems in economic development. To hold my comments to brief compass I shall limit myself to two topics: the implications of the two papers concerning general criteria for the allocation of investment in economic development, and some aspects of the specific model presented by Professor Chenery.

From the titles of the papers and from the fact that Professor Chenery's paper stresses the development of industry and Professor Nicholls' that of agriculture, it might seem that the two papers are in conflict concerning the question of emphasis on industry versus that on agriculture in the process of growth. In fact they are not—or, if they are, it is because of a difference between them relating to another issue to be discussed below. Professor Nicholls' paper does not deal with agricultural development versus industrialization; it deals with choice between two methods of developing agriculture. On the other hand, while Chenery states that he is concerned mainly with selection among manufacturing and processing industries, he really presents a method of selection among all sectors of the economy without prejudice to the degree to which agriculture on the one hand or industry on the other will be indicated by the analysis.

Further, there is no issue between them concerning linear programing versus partial or marginal analysis as the best tool for determining the allocation of investment which will maximize the increase in national income. There is little question that for such analysis linear programing is the superior tool. There is no issue here—except perhaps between the enlightened and the unenlightened. Linear programing can deal with the composition of output, price relationships, foreign exchange relationships, and alternative production methods far better than can partial analysis. These advantages result from the nature of economic development; namely, that the economy is changing, not marginally, but in all parts at once and in some parts by large increments or large structural changes. Change in each part of the economy affects other parts, and none can be analyzed without simultaneously analyzing all.

However, this is not a principle which Mr. Nicholls disputes. I think that there is an implicit issue dividing the two papers, but it is not this. It is rather the issue of what criteria should govern the selection of economic sectors for development and of development methods.

Chenery discusses only one criterion: "to secure the greatest increase in . . . income (total or per capita) . . . in the long run," or, what amounts to the same thing, to minimize the inputs required for a given increase in output. But Nicholls, after tipping his hat with a properly respectful gesture to the principle of maximization of income, spends his energies discussing two other criteria.

One of these is the distribution of the economic benefits of development. He refers, not in a simple way to the distribution among income size groups, but to the distribution among sectors of the economy. His concern is valid. The criterion is sound. The benefits of industrialization will for many years reach only a limited group.

Introduction of new industry at a rate sufficient to give employment to 1 per cent of the labor force per year is an extremely rapid rate of industrial expansion. Over a period of a decade, even this rate of industrialization will benefit directly only approximately 10 per cent of the population if indeed the available human skills and motivations are such that in the short run it benefits anyone, which it may not. The agricultural population from which the new industrial workers are drawn will benefit only insofar as reduction of underemployment increases per capita income, or insofar as the industrial workers contribute to family income back in their home villages.[1] In economies in which two-thirds to three-fifths of the population is dependent on agriculture, this is not enough. It is quite possible, and even probable, that in most predominantly peasant societies a government cannot remain in power for a dozen years by democratic means unless its development program gives direct widespread benefits to the peasant population.

It is worth noting that agricultural development may be desirable for a quite different, purely economic reason; namely, to provide a market for industrial output. The extent to which any given increase in agricultural income will increase the demand for industrial products can be estimated by a linear programing model. It is the social rather than the purely economic benefits of agricultural development with which a linear programing model cannot cope adequately.[2]

A third criterion, with one aspect of which Nicholls also deals, is the social costs of the increase in aggregate output. He discussed the displacement of peasant operators by the mechanization of agriculture. The social costs involved in rapid industrialization are less simple to portray but nonetheless real. Obviously, a development program must not be abandoned merely because it involves such social disruption. Otherwise there will be no development. Any economic change involves social change, and social change involves social cost. But the social costs must not therefore be ignored. The costs involved in one program differ in amount from those in another, and even

[1] J. H. Adler noted in discussion from the floor that industrialization will itself benefit the agricultural population in some economies by increasing the demand for the type of food or other agricultural product produced in peasant farming and thereby raising the price of those products. This qualification is of course entirely appropriate. Adler noted that in this case there is often a tendency for the government to restrict the rise in price of agricultural products because of its interest in protecting urban workers from the increase in living costs.

[2] In comment from the floor, Professor T. C. Schelling suggested that a linear programing model can explore the benefits of increasing income throughout agriculture by dividing labor into several groups, one of which is agricultural labor, so that the model will show the demand for agricultural labor. However, if at the completion of the analysis there is still surplus agricultural labor, so that the shadow price of labor is still zero, a linear programing model will indicate nothing whatever about the desirability of emphasizing agriculture rather than industry in a development program. This is the deficiency which I have in mind.

though they can be measured only imperfectly they should be taken into account in determining the optimum allocation of effort.

Linear programing disregards these two criteria. It does not follow that linear programing must be rejected. It follows merely that the answers given by even a perfect linear programing model are only partial answers, and that even though the three criteria are only imperfectly commensurable, all must be taken into account.

So much for the general implications of the two papers concerning criteria for investment allocation. Now, a few comments concerning the specific development model presented in Chenery's paper.

The virtues of the linear programing process, even with regard to the one criterion of maximizing aggregate income, do not necessarily inhere in any given linear programing model. The simpler and computationally more manageable a model, the greater is apt to be the margin of error in the estimates it yields.

Professor Chenery's model illustrates this point. It is after all a very limited model. For ease of computation it contains only fourteen sectors; aggregation to this point certainly blunts the edge of the tool. Further, the model assumes fixed production functions. Variation in the ratio of labor to capital is accomplished only by choice from among different products. But one of the considerations which may be of primary importance in the development of many economies in which underemployed labor is present on a large scale is the question which among alternative methods of producing a given good will yield the greatest total employment or the greatest aggregate income. Again, the model makes only crude allowance for capital durability. It assumes, that is, that if one capital good costs three times as much as another, it constitutes three times as costly an input in production. It does not take account of the fact that the more expensive capital item may endure six times as long, and thus constitutes roughly only one-half as costly an input in the production process. This, too, is an important omission. For in an economy short of capital, as elsewhere, the question of the roundaboutness of production, the period of gestation, the appropriate rate of discount of future income is of first-rate importance in planning economic development. Fourth, Chenery's model does not take into account the relative feasibility of alternative development programs. It assumes that management ability, technical knowledge, skills, and motivations will be as available for one type of project as for another. In fact, they may not. Finally the model makes no allowance for increase in productivity in specific production processes, though it makes allowance for increase in productivity for the economy as a whole through the utilization of previously unutilized labor. Yet almost by definition the process of economic development must include improvements in productivity in specific production processes.

But even though the model is less informative than otherwise because of these restricted features, this fact does not imply any criticism whatever either of linear programing models in general or of Chenery's model in particular. This is true for three reasons:

1. Each of the five limitations indicated is due either to deliberate simplification for purposes of easier presentation or to the absence of the data needed to expand the model. In all of these five respects a linear programing model can be made more realistic if the occasion demands and the data exist. The only other factor hindering the elaboration of the model is the resulting computational demands, and these are easily handled on present high-speed computors. Each type of missing information—knowledge of alternative production functions, of capital durability, of human bottlenecks, and of prospective increases in productivity—can be used at least as effectively in a linear programing model as in a "partial analysis."

2. Certain aspects of Chenery's model which are inappropriate to many countries are probably appropriate to Southern Italy. For example, in Italy managerial and technical talent and motivation may be available for almost all projects (given only top-level impetus to the total development program).

3. Finally, it seems very probable that even a rather simple linear programing model does not in general mislead because of its simplicity—that is, does not give worse results than partial analysis—but on the contrary provides valuable aid not otherwise attainable. It is worth while to remember Friedman's well-known article noting that the test of an analytical model is not the realism of its assumptions but the predictive value of the conclusions drawn from it.

Chenery has constructed a structure with imagination and a high order of craftsmanship. Its construction has given a demonstration not available previously of the power of a linear programing model in a practical situation and should be a stimulus to advance in this field. It will be recognized, I think, as a modest but nevertheless appreciable contribution to the advance of economic science.

WILFRED MALENBAUM: These two excellent papers use what are obviously different techniques in reaching decisions on patterns of investment in development programs. Professor Chenery's results come from a general equilibrium analysis in which account is taken of the interdependence of the "individual" sectors of the economy. A decision with respect to investment in industry—the sector upon which he focuses in this paper—depends upon resource availabilities and complementary investment requirements in all sectors. The analysis requires a significant amount of information, both on intersectoral flows in the economy and on the capital and labor requirements for expanded domestic output (capacity) in each sector. Comparison of some of his results with those which might be obtained by partial, sectoral analysis points up, he feels, the need for such an over-all approach.

Professor Nicholls simply asserts the need for agricultural investment in the development program of underdeveloped countries. Within agriculture, he stresses the importance of direct attention in the development program to the plight of the smaller, poorer, more underemployed agriculturist. Steps must be taken to improve his efficiency and to provide him, more or less *in situ*, with supplementary nonagricultural employment opportunities, both

of an industrial and public utilities nature. Nicholls' analysis of Turkish agricultural development activity in the recent past makes clear that the government program has not begun to meet the problems of the peasantry which constitutes 65-75 per cent of the population. Nevertheless, Nicholls' basic "explanation" for the need to concentrate directly upon this subsector of agriculture remains his insistence "that agriculture and its major subsectors must share in any real gains in per capita national income if we are to consider the claims of successful economic development valid." There is no demonstration that observed gains in per capita national income, if not shared by important sectors of the economy, are bound to prove transient and illusory. Nor is there any refutation of the proposition that real gains in per capita national income from a development program will somehow be shared by component sectors simply because of the structural interdependence of the economy.

This said, I hasten to associate myself fully with Professor Nicholls' recommendations. On the basis of my own observations and study in underdeveloped areas, both with and without "population problems," I sympathize with Nicholls' feelings that there is need to concentrate directly upon improving the economic well-being of the large rural populations. Nevertheless, it would be of great value to find in the structural interrelationships of (at least some) underdeveloped economies a more satisfactory rationale of the need for such direct action—and some measures of the relative importance to be given this type of investment in the total development program. For this, Chenery's work on general equilibrium solutions is clearly of great importance. There is no necessary inconsistency between the Chenery results and the Nicholls suggestions for emphasis in investment patterns. Chenery confines his final conclusion to the need for over-all analysis in planning industrial development. (By their very nature, however, his results also provide appropriate levels for agricultural and other nonindustrial investment.) He also indicates that investment allocations might be somewhat greater in the nonindustrial sectors of economies like that of Turkey where the marginal capital requirements in agriculture, for example, appear to be relatively much lower than in Italy.

The general model necessary for Turkey (as for India and many other underdeveloped areas) would be quite different from the one used by Chenery. In particular, the "one-way interconnectedness" of the economy which he discovers in the Italian data would, I think, have but limited relevance in these other countries. The material for Italy suggests that as per capita income increases (its distribution notwithstanding, presumably) the proportion of income spent on manufactured consumers' goods may be expected to increase. Other demands are derived from the demand for these goods. "The justification for investment in 'social overhead' facilities and in much of primary production" stems from this dependence. It is precisely the absence of evidence of such derived effects in important sectors of the economy of Turkey which provides the case for direct investment in the small-scale agricultural areas. Expansions in per capita national income arising from industrial growth or from the mechanization of a small component of agri-

cultural output do not seem to be transmitted to other important parts of the economy. For whatever reasons, consumption and production responses do not occur or are too limited in magnitude.

I must admit some uneasiness about a model of growth based upon such one-way interdependence and the simultaneous recognition of significant structural disequilibrium in the use of factors of production. Is not the existence of large underemployment in the rural sector testimony of the lack of such interdependence—in the past, if not at present? I presume that the two to three million of additional nonagricultural employment that will be provided in Chenery's investment plans are of a magnitude which will, over the decade, absorb not only the net growth in employables but also the currently underutilized labor supply. Perhaps this will indeed occur. I would feel much happier if this solution were not so apparently out of line with what has occurred in Italy in the past.

In sum, the general case for the use of the over-all approach to the determination of investment allocations is sound. Chenery's model may provide useful answers for Italy. Its extension to other underdeveloped areas requires much more knowledge about the appropriate pattern of sectoral interdependences than is yet in hand. Pending such knowledge, there is much to be said for development programs which seek to assure Nicholls' sharing of growth in per capita incomes by direct concentration upon those sectors of the economy which seem to lag.

ECONOMIC DEVELOPMENT: CASE STUDIES

COMPARATIVE ECONOMIC DEVELOPMENT: CANADA AND THE UNITED STATES

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Comparisons are odious, whether involving individuals or nations, and in presenting some tentative results of a comparison which I am now making of the Canadian and American economies to a gathering containing nationals of both countries, I am aware of the delicacy of my position. It would be easy to concentrate on technical questions or to give a glowing account of the success stories of the two economies and thereby avoid controversial issues, but since it seems to me important to bring into sharp focus some of the basic characteristics of the Canadian economy, I am going to speak somewhat more frankly than might seem appropriate at this time and place. As anyone familiar with the two economies knows, the United States has enjoyed and does enjoy a substantial advantage over Canada in terms of what used to be called economic welfare. Many of us would not spend much time studying economics if we did not think that economic welfare, even if only measurable in a rough way, was of some importance. On the other hand, all of us—and here we would have the support of eminent economists from Adam Smith to Sir Dennis Robertson—would be prepared to argue that some things may be more important than opulence. Perhaps if we could broaden our concept of welfare to include political, legal, religious, and other aspects of well-being, a comparison between the United States and Canada might yield a different answer.

If we restrict ourselves to economic considerations, however, the answer is fairly plain. One hundred and thirty-six years ago a Scottish clergyman who was traveling down the St. Lawrence and had spent the night in Prescott on the Canadian side of the river, had occasion to cross over to Ogdensburg on the American side, and being struck by all the activity he saw there, remarked that "the contrast of apathy and inexertion on the one side of the river, with bustle and business on the other, cannot but be mortifying to one of genuine national feelings." He was not the first to observe a difference in the Canadian and American economies and certainly not the last. I cannot pretend to any extensive knowledge of the history of either country, but even my cursory reading has revealed that in almost every decade since the beginning of

the nineteenth century, one does not have to search far to find observers making an unfavorable comparison between the economic performance of Canada and that of the United States. It is true, that as the decades pass we hear less of apathy and inexertion and more of wage and income differentials, but the contrast remains up to the present day. Perhaps the reasons are obvious, but as Schumpeter once remarked in another connection, "Nothing is so treacherous as the obvious." The more I look into this question the more I am impressed by its complexity, and the need for a step-by-step analysis which attempts to probe beneath the surface and expose the fundamental factors at work. Given the limitation of time and space, I am going to restrict myself to a preliminary discussion of some of the consequences for the Canadian economy of two of the great national policies which have helped to create an independent political and economic unit in the northern part of the continent; namely, the transportation and tariff policies of the Dominion government. There are those who would echo the words of Pilate, "What I have written I have written," and argue that there is no point in analyzing the consequences of decisions which, while once debatable, are now a more or less permanent part of the landscape. To them I can only point out that what is said here is not intended to have any direct implications for policy whatsoever, but only intended as a contribution to a better understanding of the comparative economic performance of the Canadian and American economies. Before turning to this question, I would like to sketch in outline the comparative economic growth of the two economies in order to establish the general setting.

Any starting point is arbitrary, but given the limitations of the statistical data before 1870 and the fact that the American Civil War and the Confederation of Canadian Provinces in 1867 mark a dividing line of sorts in the histories of the two countries, the period around 1870 is as good as any. Over the whole period since 1869-70 the two economies have achieved a high rate of economic growth, and the most obvious generalization which emerges from a comparison of their economic development is that their relative position is much the same today as it was over eighty years ago. The population of both countries has increased about fourfold during the period and today, as in 1870, the continental population of the United States is around eleven times that of Canada. It is more difficult to say anything precise about the rate of growth of income per capita. In the most recent Annual Report of the National Bureau of Economic Research, Solomon Fabricant remarked that the rate of expansion in income per capita of 1.9 per cent per annum, which according to Bureau estimates has been sustained by the American economy for over eight decades, would, he suspected, find

little precedent or parallel in any other age or nation. I think from what we know of the comparative position of the Canadian and American economies in 1870 and today, it would be safe to say that for the period as a whole, the difference between the rate of growth of per capita income in Canada and the United States must have been very small indeed.

This does not mean that the rates of economic growth of both countries have been equal throughout the period. We know that in the latter three decades of the nineteenth century the population and labor force of the United States grew more rapidly than that of Canada, and we also know that positions were reversed after the turn of the century with Canadian population growth higher in each decade since 1900. There are some grounds for thinking that there has been a similar divergence in the rates of growth of real national product, but much work needs to be done before we can speak with much confidence. The publication of Dr. Firestone's ambitious attempt to measure Canadian economic growth will undoubtedly stimulate a good deal of interest in this subject and future commentators will be able to be more precise.

Taking 1900 as the dividing line on the comparative rates of growth of the two economies, the early period from 1870 to 1900 is perhaps the more interesting. Prior to or during this period several fundamental decisions were made which played an important role in determining the path which would be followed by the Canadian economy. If these decisions, which involved the economic independence of the country, had gone the other way in favor of economic integration with the United States, then the course of Canadian economic development might have differed significantly. This early period, which includes what was once known as the Great Depression, is also interesting because of the different reputation it enjoys in the economic histories of the two countries. The American literature quite rightly draws attention to some of the difficulties associated with falling prices during parts of the period, but it is also recognized that these were decades of very rapid economic growth, which in a single generation brought the American economy from a position of parity with the economies of other industrial powers to a position of world leadership. In Canadian economic history on the other hand, the period 1870-1900 gets an almost uniformly bad press. Professor Brebner has suggested that "the twenty-five years following 1873 were probably the grimmest quarter century in the history of Canada."[1] The late O. D. Skelton headed his chapter on this period with the words, "The Days of Trial,"[2] while the Royal Commission on

[1] John Bartlett Brebner, *North Atlantic Triangle* (Yale University Press, 1945), p. 221.
[2] O. D. Skelton, "General Economic History," in *Canada and Its Provinces* (Toronto, 1914), Vol. 9.

Dominion-Provincial Relations, whose report and its appendices have had something of the status of textbooks in recent years, summed up the period in the following words: "The collapse of the boom in 1873 brought on the Great Depression which continued for twenty-three years. A persistent decline in prices, a sharp shrinkage in international lending and a low level of investment caused a general stagnation. The Canadian economy strongly affected by these influences virtually stood still, although there were many internal shifts and changes."[3] This is an extreme position, but while other treatments of the period, including that of Dr. Mackintosh in his monograph for the Commission, reach a more balanced conclusion, pessimism is the rule rather than the exception. I do not think that an overly pessimistic view of this period is consistent with the evidence, but it is easy to see what could lead to such an appraisal of Canadian economic development during these years. For one thing, the level of foreign trade in both money and volume terms fell after 1873 and its growth during the rest of the period was sluggish. The importance of foreign trade in nineteenth-century Canada as a source of tariff revenue as well as a factor of prosperity, and perhaps the availability of foreign trade statistics, gave this particular barometer of national well-being too great a prominence. A similar use of foreign trade statistics would have led to a pessimistic interpretation of the American experience. Not only did foreign trade fail to expand as rapidly as had been hoped, but also the decennial censuses revealed a disappointing increase in population. During the last three decades of the nineteenth century, in spite of the fact that around one and a half million immigrants were reported landing at Canadian ports, Canada failed to retain even its natural increase. The drain to the United States was a constant source of comment, particular regret being expressed for the loss of those born in Canada. It has been estimated on the basis of United States censuses of population that over one million Canadian-born moved to the United States during the period 1870-1900, while the opposite movement of American-born to Canada was hardly one-tenth as great. By 1901, about 20 per cent of living persons born in Canada were residing in the United States. This is an old story to Canadian economists and historians and so much has been written on the subject that little need be said. There is no mystery about the motives which led too migration. Both Canadian-born and immigrants to Canada were attracted by better employment opportunities and good free land, the superior attraction of the United States during this period being closely related to geographical factors as well as world conditions. The movement of the Canadian frontier was halted by the Canadian shield which

[3] *Report of the Royal Commission on Dominion-Provincial Relations* (Ottawa: King's Printer), Vol. I, p. 52.

barred the way north of the Great Lakes, while the American frontier continued to move westward, south of the lakes and beyond, until as good free land became scarce in the United States the flow was diverted north again, and returning Canadians and emigrant Americans joined those from Eastern Canada and Europe who were taking up land in the Prairie Provinces of Western Canada. Other factors played a part but the lack of a rapidly moving frontier had a serious effect on the development of Eastern Canada during these years and the flow of immigration from Canada was directed not only to the American frontier but also to the industrial towns of New England and the border states. I commend this story of an area with no frontier to those Americans who feel that the debunking of the frontier school of American history has gone too far.

While performance fell far below expectations, this does not mean there was stagnation in Canada during these years. From 1870 to 1900 the population of the country increased by just over 45 per cent—a rate of increase considerably lower than the 90 per cent growth of American population during the same period but somewhat higher than the rate of growth of American population during the thirty-year period from 1920 to 1950, and not very much lower than the 60 per cent increase of Canadian population during the same period. Moreover, such rough indicators as we have of the increase of output in manufacturing, agriculture, and other sectors of the economy point to a substantial rate of growth in output.

Around the turn of the century, the combined effect of a number of changes, including the revival in the demand for Canadian exports and the gradual filling up of the American West, began to be felt, and Canada entered the period which has come to be called the "wheat boom"—a period of rapid growth which overshadows both the twenties and the recent rapid expansion as the greatest boom in Canadian history. This period of Canadian economic history has achieved something of an international reputation from Professor Viner's use of it to test the classical theory of the mechanism of adjustment and there is no need to go into details. Within thirteen years from the census of 1901 to the outbreak of the first World War, with settlers pouring into the wheat lands of the Prairie Provinces, the population of Canada increased by almost 50 per cent as compared with an increase of only 28 per cent in the United States. Rates as high as that achieved by Canada during this period were practically the rule in the United States prior to the Civil War, and it is interesting to note that in terms of both population size and rate of growth the period 1901 to 1914 in Canada is a remarkably close parallel to the period 1801 to 1814 in the United States. That Canada has not been able to maintain a pace of popula-

tion growth as rapid as that of nineteenth-century America is evidenced by the fact that in 1854 the population of the United States had reached 26.5 million, while Canadian population in 1954 is only around 15.2 million.

Once the effect of the wheat boom had been absorbed by the Canadian economy, there is considerable similarity in the path of economic development of the two countries. The data are sketchy for the twenties and it is not until the most recent period, from 1929 to the present, that we have reasonably reliable estimates of gross national product in constant prices for both countries. For the thirties, the series for both countries show only a small increase, but from 1939 to 1953 it would appear that in both countries per capita output has increased by around 60 per cent, with the population of Canada increasing by just over 30 per cent as compared with a growth of just over 20 per cent in the United States. Given a continuing inflow of immigration into Canada, which relative to population has been about six and a half times as high as the American level during the postwar period, it appears likely that Canadian growth will continue to exceed that of the United States by a substantial margin.

To sum up, in the last three decades of the nineteenth century the population of Canada increased by 45 per cent as compared with a growth of 90 per cent in the population of the United States. Since the turn of the century, on the other hand, the population of Canada has increased by 160 per cent as compared with a growth of only 100 per cent in the United States. Such cross-section studies as I have been able to make, using estimates of wages, prices, and other information, leave me with the impression that at the terminal points of these major periods of economic development—1870, 1900, and today—the ratio between income per capita in the two countries may not have changed very greatly, although there have undoubedly been fluctuations in this ratio which will be revealed when and if we obtain better information.

Such comparisons are obviously very rough and the difficulty of obtaining any accurate measure is evident from the variety of estimates of this differential which have been made in recent years. It would be interesting to discuss some of the theoretical difficulties which arise in such a comparison, as well as the problems involved in the selection of the appropriate variant of national income, the adjustment to a common definition, and the difficulties involved in collecting price data covering not only consumers' goods and services but also investment goods and services and the cost of government services. If we are to have a reasonably precise estimate of the difference in average per capita income and product which will command general agreement, a fairly considerable research project will be required, but for present purposes a cruder

measure will suffice, and I shall simply assert that from the little work I have been able to do on this question, I would regard estimates (using either Canadian or American prices) which place Canadian gross national product per capita in the range 70-75 per cent of that of the United States, as probably accurate within a few percentage points either way. Some of this difference is more apparent than real. For example, the occupational distribution of the Canadian labor force differs from that of the United States. In the postwar period, around 20 per cent of the Canadian labor force has been engaged in agriculture as opposed to 12-15 per cent in the United States. As we all know, income statistics tend to show substantially lower incomes for those engaged in agriculture, but there are some who would argue that such statistics tend to overstate urban incomes and understate farm incomes, so that at least part of the difference arising from occupational distribution may be illusory. Much the same kind of observation could be made about another factor which plays a part in the income differential. It appears that labor force participation rates among married women and teen-agers are lower in Canada and this tends to lower per capita income relative to the United States. Whatever might be said about teenagers, we all know that there is only a statistical justification for omitting the value of housewives' services from national income, and that Pigou used the example of the fall in national dividend which results if a man marries his cook to show the arbitrary nature of such measurements. Lower participation rates for married women in Canada is incidentally an interesting piece of evidence of a difference between the two countries in what Professor Rostow has called the propensity to seek material advance. At one time the difference in the international investment position of the two countries was a factor of considerable importance, Canada being the world's largest debtor country prior to World War II, with net payments of interest and dividends equal to almost 6 per cent of national income in 1939. In recent years this has fallen to around 2 per cent, while net receipts of interest, dividends, and branch profits have totaled less than 1 per cent of US national income. Once we have stripped away differences arising from occupational distribution, the share of the population in the labor force and the international investment position of the two countries, we are still left with a hard core which appears to be of long standing.

The persistence of this income differential over a long period suggests that the factors at work are fundamental in character—perhaps even physical and climatic. Before accepting an explanation resting upon natural factors, however, we should remind ourselves that the economic development of Canada has not followed the lines dictated by "natural" economic forces. It would appear that at least two of the major Cana-

dian policies implemented during the period since 1870—transportation development and tariff protection—represent in part an interference with natural economic forces and have been widely accepted as the price to be paid for a measure of political unity and independence.

The decision to use transportation facilities as a means of unifying the country dates from at least the time of Confederation and perhaps earlier. Promises of a transportation link were made to both the Maritime Provinces and British Columbia as a condition of entry, and the Intercolonial and the Canadian Pacific Railways redeemed these promises. When offered a chance to participate in the building of the Pacific railway, the Grand Trunk, Canada's principal railway of that time, was unwilling to accept unless the transcontinental line passed south of Lake Superior. It should perhaps be pointed out that the Grand Trunk was a British company and throughout the story showed scant respect for the niceties of national boundaries. The government of that day insisted on an all-Canadian route, and Canada's first transcontinental railway, instead of joining the eastern and western portions of the country through high traffic-potential American territory, was pushed through almost a thousand miles of barren country with little immediate or even long-term prospects as a source of traffic. The same policy was at work during the next great railway building era following the turn of the century. During this period of unbounded optimism, it seemed obvious that the country needed not one but three transcontinental railways, all of which would run north of Lake Superior. The Grand Trunk again appears on the scene this time with a projected railway which would join its western subsidiary to the East by way of Chicago. Again its offer was rejected in favor of a more expensive alternative, and the National Transcontinental was built by the government from Winnipeg to Moncton on the Atlantic seaboard. The epic story of the third transcontinental railway—the Canadian Northern—deserves more than a sentence, but when it was decided that it too needed a route to the East, after a mild attempt to economize on transcontinentals by encouraging the Canadian Northern to double up, it was able to press ahead with help of guarantees from the federal government. This railway expansion was mainly financed by British investors, and in view of the fact that a substantial proportion of the capital inflow from 1901 to 1914 was used for building railways, it is easy to understand Viner's conclusion that "the borrowings during this period were in large part invested in economically unsound ventures."[4] Unfortunately for the Canadian taxpayer, the federal and provincial governments were deeply involved and when the time came to pay the piper, he had to foot the bill. Perhaps

[4] Jacob Viner, *Canada's Balance of International Indebtedness 1900-1913* (Harvard University Press, 1924), p. 305.

this was no less than just, since the government had received substantial public support for its transportation policy, although later commentators have argued that the government might have been able to drive a better bargain when it became necessary to do something about the incipient bankruptcy of the Grand Trunk, the Grand Trunk Pacific, and the Canadian Northern. It was decided that the liabilities of these railways should be assumed by a nationalized railway system—the Canadian National Railways—which took over control of all private and government railways in Canada, with the exception of the properties of the Canadian Pacific Railway. With an unco-ordinated group of railways, often with duplicate facilities, and with a capital structure which bore no relation to potential earning capacity, the Canadian National Railways was unable to meet its fixed charges, and in each year from 1923 to 1952, with the exception of two years in the twenties and the war years, the nationalized railway system showed a deficit. These deficits were at one time rightly regarded as a pretty serious business for the economy as a whole, but in general it can be said that either the deficits or the adjustments designed to put the capital position of the Canadian National Railways on a more realistic basis are a good deal less awe-inspiring today. One would expect to find some reflection of these financial difficulties in the physical statistics of Canada's railways, and it does appear that in terms of traffic output per employee the US railways make a better showing. How far this is due to the inadequacy of the measure, how far it can be attributed to the enthusiastic railway expansion of the past which has contributed to lower traffic density on the Canadian railways, and how far it is related to other factors is something on which I hope to have more to say at a later date. When reference is made to the transportation difficulties under which the Canadian economy operates, it must be remembered that while some of these can be attributed to natural factors, at least in part they are a reflection of an economic policy which accepted higher transportation costs in pursuit of other ends.

It is more difficult to assess the consequences for Canadian economic development of the policy of tariff protection. Broadly speaking, Canada at Confederation was faced with a choice of one of three alternatives: first, the retention of a low revenue tariff in the face of the high tariff maintained by the United States; second, the promotion of freer trade between the United States and Canada by prevailing upon the United States to agree to reciprocal tariff reductions; and, third, the choice which was made when attempts to obtain reciprocal tariff reductions proved fruitless: the adoption of a policy of tariff protection for Canadian manufacturing industries. This policy has been a continuing element in Canadian economic development since 1879, although through-

out the latter decades of the nineteenth century, and indeed up to the rejection of the reciprocity proposals of 1911, large-scale reciprocal reductions of the Canadian and American tariffs were always regarded as a possible alternative. Given these three possibilities, it may appear idle to speculate on what would have been the consequence of accepting some other course of action than the one actually adopted, but in order to decide whether the apparent economic costs are or are not real ones, some such attempt must be made.

Following the first course of maintaining a low revenue tariff while the United States retained a high tariff was an unattractive alternative as compared with some measure of continental free trade. One is bound to feel some sympathy for Canadian manufacturers of the 1870's who were quite prepared to meet American competition under free trade conditions but who argued that if the American manufacturer were accorded protection in his market, it was only just that the Canadian manufacturer should receive protection in his. At the same time it is well known that in the absence of rather special conditions, there is a presumption that, regardless of the tariff policies followed by other countries, any one country merely injures itself further by raising its own tariff. Are there any grounds for thinking that the special conditions required if a tariff is to provide a net economic benefit have been present in Canada? In the most detailed study of Canadian tariff policy presently available, it is held "that if Canada had kept her tariff at revenue levels without receiving reciprocal concessions in foreign markets the results would not have been advantageous as compared with existing levels of national income enjoyed."[5] It should be possible to justify this contention by reference to international trade theory, and certainly no one can complain that students of international trade have not devoted considerable ingenuity in recent years to discovering possible advantages for protection. Quite the contrary. Yet a search through this literature fails to bring to light any good argument for Canadian protection if we restrict ourselves, as we should, to long-run economic considerations. The long-run elasticities are almost certainly high enough to rule out the possibility of gaining by an "optimum" tariff any higher than a revenue level. Bargaining tariffs have a tendency to build up vested interests which prevent any easy reversal of policy, and the Canadian tariff has been no exception. Infant industries should be nurtured, but if their problem is their inability to compete with American industries enjoying the advantage of a large domestic market, then it is a fair bet that they will tend to remain infants. It is true that capital and enterprise can be attracted by a tariff, and the establishment

[5] Orville John McDiarmid, *Commercial Policy in the Canadian Economy* (Harvard University Press), p. 385.

of American branch plants in Canada has been one spectacular product of the Canadian tariff, but the flow of capital and enterprise into the unprotected sector of the economy is discouraged, and there does not appear to be any way in which a balance could be struck in deciding whether Canada has achieved a greater or smaller inflow of capital and enterprise by having a tariff. Ohlin—who discussed the Canadian case specifically—reached no definite conclusion. But if capital and enterprise have been attracted, this would imply that the return to these factors has been increased over what it would have been in the absence of a tariff. This in turn must have lowered the returns to either one or both of the other factors: land and labor. Since Canada has not had and does not have a wealthy landlord class which can be exploited, the burden then necessarily falls on the ordinary farmer or on the ordinary employee or on both. Those who argue that the tariff has had the effect of reducing migration to the United States are in effect arguing that increasing the disparity of income tends to discourage migration. The one good argument for Canadian protection is probably the one which has often been used rather cynically, and that is the conservation of natural resources. Given our inability to handle conservation problems at all neatly, since they involve uncertainties about future technological possibilities and, like other types of saving, intergeneration comparisons of utility, it is doubtful if anything useful can be said about this proposition, although it may have more validity than more popular arguments.

If a revenue tariff might have been less injurious than protection, clearly continental free trade offered even more favorable prospects. Professor Dales has shown that Ontario and Quebec—the two provinces which are said to have benefited from the tariff—have the locational attributes of industrial regions, and if there could have been a measure of certainty on the continuance of some form of commercial union, it appears not unlikely that there would have been a considerable movement of American industry to these areas. Under these circumstances, we might have had Professor Seymour Harris scolding Canadians instead of Southerners for attracting industry by unfair means and books might have been written entitled, "Why Industry Moves North." As things turned out, a view gradually developed that the economic links forged by the tariff were necessary if a measure of political unity and independence were to be achieved. It is true that this view was less popular in the Maritimes and the Western provinces than in the provinces which thought they had benefited from the tariff; but there was a sense in which the national policy lived up to its name.

The consequences of the Canadian tariff can be observed in Canadian

manufacturing today. There are a number of protected manufacturing industries which although paying a level of wages 15-20 per cent lower than that prevailing in the United States appear to be unable to quote competitive prices. Canadian manufactures in these industries have traditionally argued that apart from the fact that their materials, parts, etc., are subject to a tariff, the main reason for their relatively high costs is that they are producing for a market one-tenth to one-fifteenth the size of the American market. I have been attempting to test this proposition through the use of data on labor productivity in different types of Canadian industry and by gathering information from American and Canadian manufacturing firms carrying on operations in both countries. While I am not yet in a position to reach any firm conclusion, the results achieved so far give some support to this point of view. This is of some interest to economists in both countries, since it tells us something about the importance to the American economy of the size of the American market. The late Professor Allyn Young, in his famous *Economic Journal* article on "Increasing Returns and Economic Progress," suggested that "in certain industries, although by no means in all, production methods are economical and profitable in America which would not be profitable elsewhere. The importance of coal and iron and other natural resources needs no comment. Taking a country's economic endowment as given, however, the most important single factor in determining the effectiveness of its industry appears to be the size of the market." In comparing Canadian-American industry, I have found some evidence which would suggest that at least the first part of Young's observation is quite valid. American firms with American management and what in many cases is in effect the equivalent of an American working force, producing for a market which in many cases has American tastes, find that highly mechanized production methods which are economical and profitable in America are not profitable in Canada. There is one disturbing factor which may weaken this evidence. The level of wages is lower in Canada and sometimes, because of the tariff, the cost of machinery is higher. What about the possibility that substitution has an important effect? As everyone knows, marginalism never comes off very well in crude empirical studies. Here and there one finds a businessman who has either remembered something from his introductory courses in economics, or pays more attention to relative factor prices than his fellows, or has more possibility of substitution in his industry (in the language of activity analysis has a greater choice of activities) and he will report that relative factor prices have influenced his decision. But they are few and far between, and while I would be prepared to agree with those who argue that more substitution goes on

than reaches the consciousness of businessmen, if almost all one hears by way of explanation of different technology involves the scale of the market, this begins to carry some weight.

Bearing in mind the fact that limitations on the scale of the market facing many manufacturing industries in Canada are not natural phenomenon but rather the consequence of Canadian and American tariff policies, it would be interesting to know how important this factor is in lowering per capita income in Canada. I do not know the answer and I do not expect to know the answer with any precision, although I think something useful can be accomplished by further work on this question. It may turn out to be of considerable importance, although it must be remembered that the scale of the market is only of prime importance in manufacturing, and that in spite of the implicit assumption which seems to be made in many discussions of economic questions there are a few people employed outside manufacturing. In Canada in the postwar years around one-quarter of the labor force has been employed in manufacturing and a somewhat higher fraction of national income has originated in that sector. Since a portion of Canadian manufacturing is producing for the North American or world market while another portion is relatively unaffected by the limited size of the Canadian market, we are talking about a piece of the economy which is only a fraction of a fraction. As a result, the impact will be considerably lessened when measured against the economy as a whole.

I have concentrated on the transportation and tariff policies of the Dominion government, not because I think that these are the only factors in establishing and maintaining an income differential, nor even the most important ones, but simply because there is a limit to what can be said in one short paper. There are a number of other issues which might have been discussed. Differences in sectors of the economy outside manufacturing and transportation may give important clues in unraveling the puzzle. The part played by population also raises a number of interesting questions, which have come in for considerable discussion in recent years but on which the last word has not yet been said. Finally, there is that fascinating generalization that Canadians are in some sense less enthusiastic in their pursuit of economic ends than their neighbors—a proposition which is not easily testable but which may yet be attacked by an orderly collection of evidence.

We have seen that in at least two respects the policies followed by the Canadian people have had the effect of interfering with the free play of what might be called the natural economic forces, and it has been argued that this interference has had the effect of lowering per capita income in Canada. This does not mean that if viewed in a framework broader than that provided by economics, these policies were, and are,

wrong. The idea of a country extending from sea to sea which would have an independent existence and not be simply a northern extension of the United States came to be widely accepted as a worth-while goal in Canada, and it is often argued that these policies have made an important contribution to this goal. The result has been an experiment which is unique. A string of communities, stretching in a broken ribbon across a continent and in the case of two-thirds of the population with no language barrier separating them from nearby communities across the border, has developed into a national entity with a life of its own. This process has involved certain economic costs, but there is every reason to suppose that those who launched this experiment would not be displeased with the results which have been achieved.

ANGLO-AMERICAN PRODUCTIVITY DIFFERENCES: THEIR MAGNITUDE AND SOME CAUSES

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Differences between underdeveloped and industrialized countries in labor productivity usually are so marked that one can take their existence for granted. When, however, the contrast is between two industrialized countries, such differences are less obvious and, for any particular industry, often uncertain as to direction. Here statistical measurement serves as a useful prelude to any discussion of causes. The size and pattern of differences, together with additional information emerging as a by-product of the measurement process, are themselves indicative of the paths which explanatory hypotheses might take. Thus, though this paper is given in the main to a discussion of certain hypotheses commonly advanced in explanation of Anglo-American productivity differences and also to suggestions regarding a few others, its first section presents some statistical comparisons for the two countries and describes briefly the methods employed and the limitations of the findings.

I. *Productivity in Thirty Industries*

The table below, based on the United States *Census of Manufactures* for 1947 and the United Kingdom *Census of Production* for 1948, compares British with American labor productivity for thirty industries. The comparison is made in terms of ratios of physical output per worker and physical output per man-hour in the US to the same measures in Britain. Ratios also are given comparing size of market and size of plant. Collectively, the industries represent about 13 per cent of value-added in US manufacturing and 16 per cent of that in British (GB) manufacturing. Their selection was based solely on the amenability of census data to the purpose at hand. Product homogeneity and product classification in census reporting were principal considerations.

A variety of difficulties attend any statistical endeavor to measure industrial productivity, and the difficulties are compounded in the case of international comparisons. Both US and GB censuses pose obstacles

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TABLE 1

PRODUCTIVITY IN THIRTY BRITISH AND AMERICAN MANUFACTURING INDUSTRIES*

Industry	Physical Output per Worker† US/GB Ratio	Physical Output per Man-hour‡ US/GB Ratio	Size of Market (Physical Output) US/GB Ratio	Size of Establishment	
				By Number Employees US/GB Ratio	By Output US/GB Ratio
Tin containers	4.96	4.97	9.49	1.04	5.43
Cardboard containers	4.20	4.24	11.75	.94	*4.59*
Pig iron	4.17	4.91	5.78	.80	3.42
Wool yarn	*4.03*§	*4.53*	2.52	*1.27*	*4.85*
Radio receiving tubes	*3.36*	*3.74*	11.55	*1.06*	*5.58*
Cigarettes	3.25	3.63	3.80	2.77	16.96
Wool carpets and rugs	*3.15*	*3.28*	5.76	*2.01*	*6.04*
Glass containers	2.87	3.06	5.73	1.58	4.67
Soap	*2.81*	*2.89*	4.29	1.02	*2.45*
Paper sacks	2.71	2.77	10.61	1.81	5.05
Matches	2.48	2.59	5.53	2.86	7.06
Ice cream	*2.11*	*2.14*	28.24	*.73*	*1.43*
Animal feeds	2.11	2.06	9.75	.60	1.28
Biscuits	2.04	2.18	3.29	.76	1.72
Malt liquors	1.98	1.98	2.13	1.53	2.40
Grain milling	*1.94*	*1.86*	2.59	.82	1.64
Bicycles	*1.80*	*1.88*	1.37	—	—
Rubber tires	*1.76*	*2.03*	6.98	*1.39*	*2.64*
Jute yarn	*1.69*	*1.77*	—	—	—
Sugar					
US Cane, GB Beet	*1.66*	*1.82*	2.03	1.96	*4.05*
US Beet, GB Beet	*.77*	*.85*	.67	.51	*.46*
Building bricks	1.66	1.77	1.35	.95	1.48
Paint brushes	*1.63*	*1.74*	*2.65*	—	—
Cotton piece goods	1.62	1.78	6.32	4.56	7.64
Boots and shoes	1.51	1.67	4.30	1.42	2.96
Rope and twine	*1.51*	*1.61*	*2.28*	*1.48*	*1.94*
Margarine	1.21	1.23	.81	.52	.66
Cement	1.15	1.39	3.72	.99	1.06
Razor blades	*1.09*	*1.12*	2.80	—	—
Cured fish	.95	1.20	.30	1.08	1.07
Manufactured ice	.75	.69	41.27	.59	.46

* For the US, the figures relate to establishments employing ten or more persons. For Great Britain they cover establishments employing eleven or more persons.

† The term "worker" includes all employees—production workers (operatives) plus administrative, technical, and clerical workers.

‡ "Man-hours" refer to the man-hours of all employees, not just production workers (operatives). To obtain man-hours on this basis, adjustments to the data additional to those made in the computation of output per worker are required. These adjustments are necessarily somewhat arbitrary and subject the output per man-hour figures to a slightly greater margin of error than applies to the output per worker figures.

§ All figures are rough approximations. Because of methods of census classification and reporting, certain figures are subject to a larger margin of error than others. Such figures are italicized.

SOURCES: The figures are derived from data in the U.S. Department of Commerce, *Census of Manufacturers: 1947*, Volumes I and II (Washington: U.S. Government Printing Office, 1949), the Board of Trade, *Final Report on the Census of Production for 1948*, selected volumes (London: HMSO, 1951, 1952, and 1953), and the *Ministry of Labour Gazette*, Volumes LVI, No. 10, LVII, No. 3 (London: HMSO, 1948 and 1949).

to the computation of matching product and employment categories. Both permit but imperfect determination of man-hours worked. Both leave unanswered many questions with respect to product classification and definition; in no instance, not excepting pig iron, brick, and cement, is the product truly homogeneous; yet without the knowledge of specialists one cannot be certain how the findings are thereby affected.

Comparative treatment of the two censuses gives rise to two further problems of major importance for physical productivity ratios. One of these concerns the lack of comparability in product structures between the two countries. Within many product complexes, for example, the British produce subproducts in different proportions from the Americans. This circumstance, together with lack of a uniform terminology and differences in methods of product classification, renders impossible in most instances a comparison of identical products. The most to be hoped for is a "reasonable" similarity in the things being compared. What is meant by reasonable is, of course, an open question. Even were it possible, it would not necessarily be best always to delimit the comparison to identical products or to adjust the data to eliminate the influences of product variety. For certain industries, differences between the two countries in the kinds and degrees of differentiation of both intermediate and end products have a good deal to do with differences in production methods and per worker output levels. In assessing the significance of this factor, one has need of productivity figures that reflect its effects.

The second problem relates to differences between the two countries in the degree to which various of the industries are vertically integrated. It relates also to differences in methods of reporting by the censuses. In the US, for example, establishments brewing beer also bottle it while in Britain bottling is done by establishments classified to a separate industry. Thus though both censuses report output in numbers of barrels, US output is subjected to additional processing which the figures do not reflect. In this instance a corrective adjustment is possible, but for most industries, it is by no means clear from the censuses whether significant differences in the degree of integration or in the methods of census reporting exist. Even where the nature of any difference is known, adjustment for it may be quite impractical. The virtues of physical productivity ratios—their seeming comparison of like with like and their freedom from the influence of fluctuating prices —are lessened by problems such as these.

Because of occasional variations in methods of reporting and details given by the censuses of both countries, complete uniformity from product to product in computational procedure was not possible. For

Britain, customary procedure, in barest outline, was to begin with sales figures together with corresponding physical quantities for a given product and then to adjust them for that fraction of sales made by establishments classified to other trades and for inventory changes during the year. A matching employment figure was then obtained by adjustments, where necessary, based on the value of output figure. Information on man-hours worked, drawn from the *Ministry of Labor Gazette,* supplemented the employment figures. For the US it was usually necessary to begin with total shipments of a product, and then to adjust the figures for shipments by establishments classified to other trades and for inventory changes during the year. The census gives total man-hours directly. (See footnote ‡ of the table on this point.) Adjustments to this figure to eliminate the influence upon it of production by an industry of certain secondary products yielded matching quantity and man-hour data.[3]

The statistical findings in the table represent at best a rough approximation of the facts. The limitations enumerated above—as well as the more commonly recognized ones attaching to all productivity comparisons, whether between countries or over time—must be kept in mind. Fortunately for what follows, we need not rely on the exactness of the figures.

Our physical productivity ratios range from a high of about five in the manufacture of tin containers to a low of about seven-tenths in the manufacture of ice. From among all of the ratios, only this latter one and those for beet sugar and cured fish (output per worker only) show an advantage for Britain. No obvious explanation of the ranking of the ratios suggests itself. Several industries employing comparatively roundabout and highly automatic methods possess high ones; e.g., cardboard containers, cigarettes, tin containers. At the other extreme, the ratios for cured fish, paint brushes, and bicycles—all relatively labor-intensive industries—rank fairly low. But the tendency is not clear cut. Rubber tires, boots and shoes, and margarine make use of highly indirect production methods, but the US/GB productivity ratios are not correspondingly high. More worthy of note is the fact that the US advantage, rather than being limited to any few products or production

[3] As noted in the text, a consideration in selecting industries for comparison was product homogeneity. For that reason it sometimes proved feasible in arriving at an output figure simply to aggregate the quantities given for the various subproducts of a product group. An alternative procedure, employed in instances of more complex product structure, was to select a "typical" subproduct common to both countries and to adjust the other subproducts to a comparable quantity base through use of an implied price for the typical subproduct. Here the quantity of any subproduct is given by the formula $\Sigma v_i/p_A$ where v_i is the value of output of any subproduct and p_A the price of a unit of the typical subproduct. This procedure was used only in instances when the typical subproduct constituted a major fraction of the total value of the product complex.

methods, is more or less pervasive through technical processes and product types.

So large a margin as that which, on the average, favors the US cannot have developed over a few years or even over a decade or two. It must be the outcome of a lengthy process of differential economic change between the two countries. If the hypothesis is accepted that the relationship was reversed, with GB productivity the higher until perhaps the early part of the nineteenth century—in what degree remains quite uncertain—then the inception of Britain's relative disadvantage belongs back a hundred or more years.

Of interest are some comparisons of productivity for prewar years. As might be expected, they evidence a marked advantage for the US. Basing his calculations on census data for the 1935-39 period, Rostas found for some thirty-one manufacturing industries an average US/GB ratio of output per man-hour of 2.8. He found, further, that the US advantage was not confined to the manufacturing sector but extended with even greater force to mining, public utilities and communications, and transport while significant but less marked disparities prevailed in building and construction, distribution and services, and government.[3] Estimates utilizing supplementary materials confirm these conclusions. (See P. Barna, *Oxford Institute of Statistics Bulletin*, July, 1946.)

Comparisons for yet earlier periods support the impression yielded by those for later ones. Taussig has provided information on physical output per worker in 1907-09 for several industries.[4] His US/GB ratios are 2.2 for pig iron, 2.1 for steel, 2.9 for tinplate, 2.0 for cement, 1.7-2.0 for sugar refining, 1.5 for flour milling, and 1.2 for ice. Another set of data, based on money values and of considerably greater scope, are available for the same period.[5] Here the average of ratios for thirteen industry groups is 2.26. In light of the available statistics, there can be little doubt not only that US labor productivity greatly exceeds that of Britain but also that the US advantage extends back well into the last century. The sources drawn upon and methods of computation employed differed for each of the several sets of data referred to. For this reason no attempt should be made to compare them directly or to compute from them, for particular industries or economic sectors, rates of change in productivity differences over time.

[3] L. Rostas, *Comparative Productivity in British and American Industry* (Occasional Paper XIII, National Institute of Economic and Social Research, 1948), pp. 27, 89.

[4] F. W. Taussig, "Labor Costs in the United States Compared with Costs Elsewhere," *Quart. Jour. Econ.*, November, 1924, pp. 96-114. The data are based upon some unpublished figures of A. W. Flux and the 1907 and 1909 censuses for the UK and US, respectively.

[5] A. J. Flux, "Industrial Productivity in Britain and the United States," *Quart. Jour. Econ.*, November, 1933. The author also has provided data for 1924-25.

II. *Explanatory Hypotheses*

International productivity comparisons sometimes are regarded as indicators of the relative effectiveness with which countries allocate and utilize their resources. At stake whenever they are so regarded is the particular definition of efficiency being used. Output per man or man-hour measurements ignore capital inputs which are equally with labor a real cost, albeit less easily measured. They ignore the fact that differing demand structures might require somewhat different types and qualities of product. They neglect the fact that differing resource conditions sometimes require different techniques of material handling and fabrication. It is conceivable, in some instances certain, that conditions in two countries so diverge as to call for quite different methods and organization of work. Where therefore one's concern is with the effectiveness with which resources are being used, what is needed are not man-hour output figures or even data covering direct and indirect expenses reduced to some unit of real cost. Ideally, what is needed is some notion of optimum resource use and the degree to which practice falls short of attaining it.

These considerations render improper use of the US/GB ratios as measures of the success or failure of either country in realizing economic utilization of its resources or in achieving best practice in its production methods. Nevertheless, it is well to remember that just as increases in productivity over time within a country signal reductions in real unit costs, though not necessarily in the same degree as productivity increases, so differences in productivity between countries generally indicate differences in real unit costs. A commonly espoused objective of economic policy is the continued increase over the years in productivity levels. Inquiries into the causes of US/GB differences may yield clues on how best to further this objective.

Resource Differences as a Cause of Productivity Differences. Resource conditions may affect productivity levels in a number of ways. An obvious direct effect is on the level of productivity in the extraction industries. Coal and iron ore are familiar examples. Supplies of these minerals are more accessible in the US than in Britain, permitting shallower mines and a larger proportion of opencast mining. These facts go far to explain the higher US/GB man-hour output ratios for iron ore and for coal. Resource conditions also act indirectly on productivity in mining by fixing limits to the degree and kind of mechanization permissible. Much modern cutting and conveying equipment is unsuited to British coal mines, where shafts are deep and seams thin.

The impact on the resource-using industries, exerting itself in many directions, may be equally great. American productivity in the manu-

facture of paper and paperboard containers and in the packaging process itself benefits considerably from abundant supplies of good quality paper and board.[6] Not only do the raw materials lend themselves to mechanized methods of processing more readily than do those imported by Britain, but their lower cost relative to labor and machinery costs conduces to employment of material-using, machinery-using laborsaving methods.[7] In steel-making, the much lower phosphorous content and somewhat lower sulphur content of the basic iron available to US plants aids their productivity,[8] while access of US steel foundries to cheap Bentonite and high quality silica sands facilitates their use of laborsaving techniques.[9] Again, US productivity in the manufacture of shoes benefits from the better quality of leather available here than in Britain.[10] Where the raw material is a fuel, its scarcity and consequent high price is likely to restrict the development and application of various kinds of power equipment. Absence of petroleum and natural gas deposits in Britain doubtless has something to do with the extent and quality of mechanization in its industries.

One of the commonest theories accounting for America's comparatively high productivity relates to the indirect effects of the country's abundant land supply. Opportunities on the land and at the frontier during the last century, it is said, reduced the supply of labor available in industrial areas. Resulting factor price ratios conduced to mechanization which, it is claimed, is the principal determinant of labor productivity. The conventionally assumed relationship between land supply, wage rates, and intensive mechanization is not contested here. Whether mechanization is the primary factor accounting for high productivity in the US is discussed in a separate section below.

It is worth noting that some of the handicaps faced by Britain in

[6] Anglo-American Council on Productivity, *Packaging* (London and New York, 1950), pp. 23-24. (Reports by this organization which are the outcome of visits to the US by British teams of specialists will henceforth be designated Productivity Reports.) The report states: "The U.S. not only is a producer of basic paper and board fibres, but converts them into finished materials of outstanding quality. These fine materials are the foundation of the ultimate packages and of the mechanical packaging process. This relationship cannot be ignored." It notes that in the US conversion industries, "slow running, stoppages and excessive spoiling due to faulty materials are rare, while in the packaging industries higher output can be obtained . . . because of the higher degree of rigidity and consistency in the material and a consequent greater structural strength."

[7] *Loc. cit.* Resource shortages may be "induced" as well as "natural." Rostas, *op. cit.*, p. 203, refers to the heavy duty on raw tobacco in Britain. In face of the resulting high price, the industry, to minimize the wastage of leaf, strips a sizable proportion of it by hand and operates its stripping machines at comparatively low speeds.

[8] British Iron and Steel Federation, *The Iron and Steel Productivity Report*, p. 6. This document summarizes the productivity report published by the Anglo-American Council on Productivity.

[9] Productivity Report, *Steel Founding* (1950), p. 34.

[10] ". . . the outside cutter, applying the die to a clear surface skin, finds it necessary to spend less time moving his pattern, selecting the appropriate quality of the surface and feeling the skin for texture, weight or quality." Productivity Report, *Footwear* (1951).

the way of raw material shortages or materials of inferior quality are not attributable directly to natural conditions but are in part the outcome of British industrial and technological practice. Thus, in fabrication of tin containers, British producers must rely on the hot mill plate in general use in their country while US producers benefit in terms of cost and productivity from abundant supplies of technically superior cold-reduced tinplate. "The saving in cost is not just in the material itself but in the higher production speeds which can be attained with a consistent raw material having the appropriate qualities." (*Packaging, op. cit.*, page 19.) Similarly, the machinability of steels and cast iron used for machine tools in Britain generally is inferior to those used in the US,[11] and the surfaces of metal products often are poorer and require more finishing.[12] Instances like these tell us something about methods in the industries consuming intermediate products but little about conditions in the industries supplying those products. To assess fully the influence of resources on productivity, conditions in both groups of industries must be considered.

From the evidence it is fairly clear that resource differences contribute in many instances to productivity differences. However, resource differences seem not to suffice as a general explanation of productivity differences. As noted earlier, the US advantage is not confined to any special group of industries. It pervades the new as well as the old, the highly mechanized and standardized as well as the less technically developed, and those producing consumers' as well as producers' and durable as well as nondurable goods. The differences are not limited to the manufacturing sector, but appear to extend to others. Hence it cannot be said that they relate only to those products or processes that might be exceptionally affected by resource or similar endowment factors. It must also be remembered that technological and product requirements fix limits—occasionally narrow ones—within which modern industrial communities must work, and to this extent the scope for intrusion of the resource element as a factor affecting productivity is reduced. To the same extent, productivity differences will reflect the impact of other factors. In many instances, moreover, raw material costs are but a minor element in total fabrication costs.

Equipment Differences as a Cause of Productivity Differences. The one element which is held more frequently than any other to be mainly responsible for high productivity in the US is the extensive use of capital equipment. Indeed, high productivity and the employment of

[11] Productivity Report, *Metalworking Machine Tools* (1953), pp. 1-2.

[12] Productivity Report, *Metal Finishing* (1951). "The excellent surface condition of the metal available in the U.S. . . . may be the most important factor in the comparison of American and British (polishing) practice, since this condition controls the number of stages of polishing and the effort required at each stage" (pp. 21-22). See also p. 12.

large amounts of machinery are usually regarded as inseparable. Almost without exception, the recently published Anglo-American Productivity Reports comment on the greater aid received from machines by the American worker than his British counterpart. Writing half a century earlier, the Mosely Industrial Commission (a team comprised largely of British trade-union representatives) emphasized the same point.[13] Still earlier, in the 1850's, members of another British group stressed the tendency of American manufacturers to adopt machinery wherever possible and took cognizance of its effect in many industries. In the view of one of these observers, America's pace of industrial development was attributable to "the universal application of machinery effected with a rapidity that is altogether unprecedented."[14] Without doubt, equipment differences comprise a large part of the explanation of observed productivity differences. For the present discussion, however, the relevant question is whether equipment differences, by systematically accounting for US/GB productivity differences in a range of industries, suffice as a general explanation of the latter.

For want of suitable data, the question is difficult to test. To the writer's knowledge, estimates of the quantity of capital employed industry by industry for both countries are not available. Horsepower figures provide a crude substitute measure. Such figures are not given in the 1947-48 censuses, but Rostas has taken them from censuses for earlier years.[15] His findings show for the manufacturing sector as a whole that the horsepower per worker ratio for the two countries is about 2 to 1. (These horsepower figures, along with Rostas' productivity figures, may also be used to throw some light on the question of whether equipment costs offset the laborsaving advantage possessed by the US. Since, for the manufacturing sector as a whole, the US/GB ratios of output per worker and horsepower per worker are both about 2 to 1, horsepower per unit of output is about the same for both countries. Insofar as horsepower is a measure of capital cost, it follows that lower unit labor costs are achieved in the US without any increase over Britain in unit capital costs. US productivity defined as 1/unit labor cost plus unit capital cost, while somewhat less than twice as high as UK productivity, is still substantially higher than the latter.) This 2 to

[13] Mosely Industrial Commission to the United States of America, *Reports of the Delegates* (Manchester: 1903). See, e.g., pp. 6, 8, 14, 110, 150, 216.

[14] *New York Industrial Exhibition: Special Report of Mr. Joseph Whitworth*, British Sessional Papers, House of Commons, 1854, XXXVI, p. 5. Observations on the unusually extensive application of machinery are found frequently both in this report and the companion report by Wallis (*Special Report of Mr. George Wallis*). See also D. L. Burn, "The Genesis of American Engineering Competition 1850-1870," *Economic History*, Vol. II, pp. 292-311.

[15] Rostas, *op. cit.*, pp. 52-54, 68-70. He gives figures for the UK for 1930 and for the US for 1929 and 1939. The UK figures are for horsepower in use and the US figures for horsepower installed.

1 ratio approximates that which he obtained for output per worker, suggesting a relationship between productivity and equipment used. But when the situation is examined industry by industry, the most one may say is that in the US productivity is greater by varying amounts and so is horsepower, with little systematic relationship between the two. US industries enjoying relatively large productivity advantages over corresponding GB industries are not regularly aided by amounts of horsepower proportionately above the average, and those with smaller productivity advantages do not always use horsepower in amounts proportionately below the average.

Unfortunately, no simple quantity index of equipment used will adequately measure its effects. Equipment differences are of a qualitative as well as a quantitative kind. Machines need not be more costly or of greater power to perform better. The very essence of an innovation is the qualitative change in technique it entails. Moreover, equipment differences usually carry with them ancillary differences—in plant layout, material flows, labor skills, and the like. In short, equipment differences reflect themselves in many dimensions other than the quantity of capital dimension. In practice there is no easy way of deciding which of these ancillary differences or what part of any one of them to associate with equipment differences, nor is there any easy way of measuring their importance. This is not to say that the relationship between productivity differences and equipment differences must be greater than the horsepower figures indicate. But these considerations suggest that it probably is more fruitful to regard differences in the quantity of capital employed as but one of several factors contributing to productivity differences rather than to seek for or expect any simple relationship between the two.

The Market-size Hypothesis. The theory that the productivity level is related to market size is a corollary of that which ties the productivity level to the quantity of capital employed. Modern methods are founded, by and large, upon the use of machinery and equipment in fairly large blocks. In face of the resulting indivisibilities and the need for a high degree of specialization in the application of mechanical and human effort, small markets constitute a handicap. Relatively large markets make for reduced costs and high productivity by facilitating an optimum division of function within the plant, within the industry, and throughout the economy. Upon this generally recognized fact are founded most of the observations which attribute Britain's low productivity to the nature of its markets. These, it is said, are too small to allow for both sufficient variety of product and at the same time its output on a basis permitting the economies of standardized mass processing.

In all but three instances—cured fish, beet sugar, and margarine—our data indicate that the US market, as measured by national output, is larger than the British market. In several instances—like paper bags, radio receiving tubes, cardboard containers—it is more than tenfold larger. But there exists no regular relationship between differences in market size and differences in productivity. Given differences in productivity sometimes are associated with much larger differences in market size, as with animal feeds and boots and shoes. At other times given differences in productivity are associated with much smaller differences in market size, as with cigarettes and wool yarn. With few exceptions, the inclusion of small establishments—those with ten or fewer employees in Britain and nine or fewer in the US—would have little effect on the ratios for size of market given in the table. For jute yarn, cured fish, and manufactured ice the ratios would be reduced somewhat. Rostas (*op. cit.*, p. 59), analyzing his data for the thirties, found some interrelationship in that for several industries where the US productivity advantage was greatest the US market was comparatively quite large. Otherwise he found no close relationship. If the data are adjusted by eliminating those industries whose markets tend to be narrowly regional or local rather than national, a moderate relationship between the two variables is observable. But then the task arises of ascertaining the sequence of cause and effect. Are large markets responsible for high productivity and the low real costs usually accompanying it? Or is it that large markets follow upon the prior establishment of high productivity and low costs? The mere coincidence of large markets and high productivity would not answer these questions.

Unless one assumes what it does not appear valid to assume—that productivity levels vary directly and continuously with market size and that the highest productivity levels are associated with the biggest possible size of market—then 50 or 60 million potential consumers should offer as great or almost as great an opportunity for gains from specialization and mass production as 150 or 160 million. Probably all industries are capable of exhausting potential economies within notably smaller confines. Britain's population, moreover, occupies an area but fractionally as large as that of the US. As a result, its industry confronts a market unbroken by the vast transport distances which make for spatial differentiation and hence for smaller effective market size. Thus market size, properly qualified, far from exerting a differential adverse effect on British industry, may constitute a positive advantage to it. E. Rothbarth ("Causes of the Superior Efficiency of U.S.A. Industry as Compared with British Industry," *Economic Journal*, Septem-

ber, 1946, page 384) notes that in 1870 the US market as a whole was probably smaller than the UK market as a whole, while the population of the latter was much more compact and accessible. Yet it appears that US productivity in some industries was even then significantly greater.

Effective market size involves much more, of course, than the sheer number of potential consumers. A fact often referred to is the importance of foreign markets in Britain's demand structure. These, it is said, vary in their nature from one importing country to another, imposing upon the British producer heterogeneous requirements with respect to type and quality, so that economizing long runs of standardized products become impossible. The data confirm that for several of the industries treated in this paper, a fairly large percentage of total output is for export. Of the thirty product groups in the table, British exports as a percentage of production in 1948 exceeded 25 per cent for seven of them. The approximate figures are: bicycles, 69 per cent; razor blades, 46 per cent; fish curing, 41 per cent; wool carpets and rugs, 37 per cent; cotton piece goods, 36 per cent; rubber tires, 32 per cent; sugar, 25 per cent. But even assuming a marked divergence in product requirements between the domestic and the foreign market, the possibility appears for only a few of these that the balance left to the home market is so small as to preclude the use by the bulk of industry of high labor productivity methods. There is no obvious reason—at least to this writer—why the special fabrication techniques that may be suited to export production need set the pace for the economy.

If there be any useful hints in the market hypothesis, they relate not to market size per se nor to export markets but to both the structure of the industry and the structure of the market as these have developed in relation to one another. Where markets for a given product-class are large but highly differentiated, product specialization by firms to a degree compatible with use of highly laborsaving methods may prove impossible. This difficulty is relevant, not only to markets for final products, but equally to intermediate producers' markets. If the demand for semiprocessed raw materials, for components, or for machines is structured and variegated rather than reasonably standardized, the suppliers of these items will experience the same handicaps as will suppliers of finished consumers' goods.

Evidence suggests that in Britain, over a wide range of industries, success in standardization and simplification (the two terms, while sometimes finely distinguished from each other, are here used interchangeably) has been markedly less than in the US. It is uncertain when divergence between the two countries became pronounced. But again differences, as reflected in the degrees to which specialization is carried

out within and among plants, are observable as far back as the middle of the nineteenth century. Especially pertinent are the views expressed by a British group in the early 1850's. It took particular note of the adoption over much of American industry of the "manufacturing principle," according special mention to locks, clocks, small arms, and many branches of the woodworking trade. In the manufacture of locks, for example, it was noted that "all the locks of each description produced are identical, and their parts can be interchanged,"[16] and in woodworking, attention was called to the division of work "into special manufactures where only one kind of article is produced, but in numbers . . . almost in many cases incredible."[17] The British commented on these features of American production because they contrasted with those typical of their own country.

Comments in later years ran in a similar vein. Writing during the first World War, the Board of Trade Engineering Trades Committee called attention to the multiplicity of products produced in each establishment, to the pride many of them took in this state of affairs, and to the confusion and diseconomies that resulted. It compared the situation to that in the US (and Germany) where variety in each plant was limited, and concluded that future progress in the British engineering trades was contingent upon greater standardization and output on a larger scale. (Committee on Industry and Trade, Part IV, *Survey of the Metal Industries* [London: HMSO, 1928], page 146.) The testimony presented during hearings before the Balfour Committee, in the latter part of the twenties, confirmed the need for greater standardization. It indicated also that differentiation was not merely tolerated but in many instances was actively encouraged. (*Ibid.*, page 162, for examples in agricultural machinery and jute.) Suggestions that simplification and standardization policies ought to be pursued often were met with expressions of apprehension that such policies would endanger quality or lose markets through customer dissatisfaction or fix a pattern which time and change would make obsolescent.[18]

Emphasis upon the advantages accruing from standardization and its greater ubiquity in the US than Britain comes also from more recent inquiries. Many of the Anglo-American Productivity Reports stress

[16] *Report of the Committee on the Machinery of the United States of America* (hereafter referred to as the Machinery Report), Sessional Papers, House of Commons, Vol. L, 1854-55, p. 12.

[17] *Ibid.*, p. 64. Observations of the kind cited in the text are found frequently in this report.

[18] *Survey of the Metal Industries, op. cit.*, pp. 162, 221, 227-228. In connection with bicycles, industry representatives questioned the desirability of a standardized, mass-produced article on grounds that "popular taste would not favor a very cheap bicycle" (pp. 227-228).

this issue. Limitation of variety is strategic because it intertwines with a complex of other factors affecting the productivity level. Its relationship to the use of automatic machinery was mentioned. Such machinery tends to be highly specialized in purpose and hence advantageous only where the identical operation must be repeated many times over. Long runs of uniform products permit this repetition; output in small batches of varied products usually does not. Besides facilitating the use of machinery with its resultant saving in direct labor, product uniformity also saves skills, as machine-tending and the performance of fairly rudimentary tasks replace the craftsmanship of the artisan. It permits more systematic plant layout and workshop organization, so paving the way for continuous processing methods and yet greater division of labor. It eases problems of material handling and warehousing. The interchangeability of parts it entails contributes to reductions in servicing and replacement costs and in the size of inventories. Contrary to popular opinion, product uniformity also may mean better product quality. Constant technical advance has brought improvements in the quality of mass-produced items and worked to diminish quality differences between them and similar items produced by less roundabout methods. Precision methods and quality control—integral features of the newer techniques—have furthered this end. For illustrations, one may look to the auto and textile fields. The distinction between quality goods and standardized goods, always in favor of the former, is no longer so generally acceptable insofar as it relates to the functional characteristics of those goods.

It is not intended here to grant any causal priority to differentiation or lack of it as it relates to other factors affecting productivity. It is not to say, that is, that use in the US of more equipment and less skilled labor results from standardization and simplification. On the contrary, it is plausible that a scarcity of labor, notably skilled labor, in the formative years of this country's industrialization led to mechanization which brought with it, from necessity, a high degree of standardization as well as other features. In the judgment of Wallis (*op. cit.*, pages 2-3), the invention of laborsaving machines, "whose automatic action so completely supplies the place of the more abundant hand labor of older manufacturing countries," was stimulated in the US by the difficulty of securing skilled and disciplined labor. Other hypotheses may well be equally or more plausible. But it seems fairly clear that standardization is closely tied in with other factors bearing on productivity levels and that its presence or absence is associated with the presence or absence of these other factors.

The Effects of Plant Size on Productivity. The question of how plant

size relates to efficiency has been much discussed and investigated. The same question may be raised here, provided we substitute "labor productivity" for "efficiency." The rationale for expecting plant size and productivity to move upwards together is not unlike that for expecting market size and productivity to move upwards together. Opportunities for the application of machines, for specialization, and the division of labor increase as the plant becomes larger, just as they increase, at least up to a point, as the market becomes larger. In the latter case the economies attainable are of both the internal and external kinds while in the former they are mainly internal to the plant.

If the plant size-productivity hypothesis is useful in the present case, then we would hope to discover that US plants are, on the average, larger than British plants and possibly also that the amounts by which they are larger from one industry to another vary with the amounts by which US productivity is greater from one industry to another. The effort to test this hypothesis brings forth a fundamental difficulty: Should plant size be measured by output or by employment? If it is measured by output, then, as with the market-size hypothesis, what cause-and-effect sequence does one impute to a positive finding? Is the productivity of US plants higher because their output is higher, or is their higher output a consequence of the lower costs (or higher incomes) associated with higher productivity? Or is there a mutual interaction between the two variables?[19] Moreover, the output measure neglects the influence of vertical integration which can result in an increase in size (in one sense) without affecting output. If in any industry the plants in one country subcontract much of the work while in another country the plants perform most of the tasks themselves, this difference will not be reflected in an output measure. The 1949 Productivity Report, *Simplification in Industry* (page 8), calls attention to a stronger tendency in the US than Britain toward subcontracting and expresses the view that it aids productivity by making for greater specialization. It also may bias our productivity figures since it reduces the likelihood that a given product classification in the respective censuses will cover the same range of processes.

If employment is used to measure size, these handicaps are bypassed to a certain extent. Differences in both vertical and horizontal integration will generally be reflected in employment differences. The problem of causal sequence will be less serious since, while high productivity,

[19] One might argue that in the US resource conditions, factor-price ratios, and demand conditions favor the construction of large plants (high output plants) and that large plants facilitate high productivity. Or one might take the view that unique managerial talents, cheap resources, or some other circumstance result in high productivity which in turn leads to large plants to meet a large sales demand. There probably is merit in both approaches.

insofar as it implies low costs, will tend always to result in high output, it may conduce to low as well as to high employment. This follows because though high productivity brings high output, thus tending to increase employment, it also brings reduced labor requirements per unit of output, thus tending to reduce employment. Unfortunately, these two offsetting tendencies deprive us of easy criteria for evaluating our statistical findings. Plants are built with the primary object of accommodating a certain output or range of outputs, not a given size labor force. Should we, then, expect employment to be high where productivity is high? Or should we expect it to be low? The answer turns on the elasticity of demand, which is known to vary substantially among industries.

The foregoing qualifications would make difficult interpretation of most data on plant size and productivity, whatever those data showed. If the qualifications are less troublesome for the present study, it is only because the data support no obvious hypothesis. If size of plant is measured by number of workers, then average size of plant is found to be larger in the US in sixteen industries and larger in Britain in eleven industries. In a few instances—biscuits, animal feeds, ice cream, manufactured ice—British plants are considerably larger than those in the US. In a few other instances—cigarettes, cotton piece goods, matches—the picture is reversed, with US plants being two to three times as large. But the data do not sustain any generalization for the whole of industry. The picture does not change when differences between the two countries in plant size are related to differences in productivity. US productivity is usually higher where plant size is smaller. Where US plant size is larger, the degree to which it is larger evidences little relationship to productivity.

Measuring plant size by output, British plants prove to be larger for margarine, beet sugar, and manufactured ice. US plants are considerably larger for cotton piece goods, glass containers, cigarettes, matches, paper bags, radio receiving tubes, tin containers, and wool carpets and rugs. For all the industries, British plants are larger in three cases and US plants in twenty-four cases. While in a few instances—glass containers, tin containers, cigarettes, radio receiving tubes—large differences in plant size coincide with large differences in productivity, no definite judgments on this score seem warranted.

III. *Some Other Factors*

Many factors, in addition to those cited above, affect productivity levels. Only three others—managerial and union attitudes and structures, workshop organization and control, and specialization of labor—will be taken up here. Their effects on productivity are less tangible

and less measurable than the ones already discussed. Perhaps for this reason they rarely are accorded the status of general hypotheses in explanations of Anglo-American productivity differences. Yet their influence appears to be great, particularly so where their interactions with one another or with the resource, equipment, and market factors are considered.

Differences between the two countries in management's approach to production problems and in worker attitudes and practices have been a subject of comment by both early and contemporary observers. The comments differ in detail. But the impressions conveyed by these observers, whether they wrote five, fifty, or a hundred years ago, are in rough agreement on a number of points.[20] Management in the US is less bound by traditional techniques; it responds more readily to new ideas and is prepared to implement them more quickly than its British counterpart. It is more attentive to and seeks more thoroughly to control costs; relatedly, it is more output- and productivity-conscious than is British management. It is comprised to a greater extent of men who have received technical and engineering training and it makes greater use of highly trained specialists of all sorts in filling its advisory and supervisory needs. It is less centralized in its control, is less hesitant to delegate responsibility, and provides at all levels greater opportunity for advancement to those with ability and initiative. Not least, it is more functional and utilitarian in its approach to product design and finish, emphasizing service for the job rather than appearance or quality that is beyond requirements. On the labor side, US workers resist technical changes less than do British workers and accept time-study and job reclassification more readily than do the latter. Less extensive and less rigid craft demarcations also ease the introduction of organizational and technical changes, and a more simplified wage structure contributes to the same end. These same circumstances give management greater latitude generally in administering operations and in adjusting to shifts in demand and supply and so aid accordingly in attainment of higher productivity levels.

Conditions relating to work shop organization and production control seem also to favor the US and to have favored it for a long time. Observers in the last century made reference to the "almost perfect system of manufacture" found in many American trades[21] and to the

[20] These remarks are in the nature of broad impressions conveyed by the literature cited elsewhere in this paper: the Productivity Reports, the Balfour Report, the Mosely Report, the reports of Wallis and Whitworth, and the Machinery Report.

[21] ". . . a considerable number of different trades are carried on in the same way as the cotton manufacture of England, viz., in large factories, with machinery applied to every process, the extreme subdivision of labor and all reduced to an almost perfect system of manufacture." Machinery Report, *op. cit.*, p. 32.

greater attention paid to all details by American manufacturers.[22] The more recent Anglo-American Productivity Reports, in contrasting British and American conditions, refer to good production planning in the US which avoids waste of man and machine hours,[23] to smoothness and continuity in the work flow, to the "excellence of the organization which provides (workers) with . . . the parts and materials they require at the right places at the right times,"[24] to plant layouts that minimize the need for handling and for intraprocess storage,[25] and to a timely and speedy analysis of information on operations that enables production control to operate with maximum effectiveness.[26] Observations of this type occur with such frequency and with reference to so many industries as to suggest applicability to a wide range of manufacturing activity.

Evidence of the greater extent to which the specialization factor has operated in the US is found in two areas. The first concerns the quality of skills among production workers in the two countries. This quality seems always to have differed, with US factories employing a smaller proportion than UK factories of craftsmen who have undergone lengthy apprenticeships. American reliance upon semiskilled and unskilled labor implies specialization of function, just as British reliance upon skilled labor implies its opposite. It is through the breakdown of complex tasks into a lengthy series of rudimentary ones that this country has been able to utilize a comparatively unskilled labor force so effectively. This type of specialization obviously is closely related to the degree of mechanization, since it is mechanization which greatly reduces skill requirements. The second area where labor specialization manifests itself is in administrative, consultative, and supervisory ranks. American industry employs production and design engineers, job-rating men, cost control experts, maintenance personnel and other specialists in larger proportions than British industry.[27] Such specialists are, on the whole, quite skilled. Their employment compensates for the lack of all-round, artisan-type skills available on the shop floor. It also is tied in with the degree of mechanization in that capital-intensive methods are dependent on such skills for their efficient functioning.

An assessment of the relative importance of these factors as against others like resources, equipment, and market structure is no easy task and will not be attempted here. With respect to all of them, however, at

[22] See the article, "American Manufacturing Methods," by a correspondent with the Mosely Commission in *The Times*, Dec. 26, 1902, p. 9-1.
[23] E.g., *Rigid Boxes and Cartons* (1951), p. 5.
[24] E.g., *Diesel Locomotives* (1950), pp. 2-3.
[25] E.g., *Packet Foods* (1951), p. 17.
[26] E.g., *Production Control* (1953), p. 12.
[27] See, e.g., *Diesel Locomotives*, p. 2; *Brushes*, p. 17; *Heavy Chemicals* (1953), p. 9.

least three things seem fairly certain: First, they are factors of consequence and merit a place in any explanatory framework. Second, they register their effects not in isolation but through complex interaction with each other. Third, none is of recent origin, but their roots go back at least to the middle of the nineteenth century. Their different impacts in the US than Britain are the outcome of rather basic differences between the two countries in the processes of economic development.

DISCUSSION

D. H. STEINTHORSON: Data concerning productivity in individual industries are valuable for their own sake, but Mr. Young and Mr. Frankel are interested in these data primarily for the light they throw on the larger problem: What determines the productivity of an economy? In the analysis of this problem, both authors make fruitful use of the comparative and historical approaches. My comments are restricted to Mr. Young's paper.

The productivity of an economy can be inferred from income (or expenditure) data, as well as estimated more directly from production data. Mr. Young's point of departure is a comparison of per capita income in the United States and Canada. Although income per capita reflects other factors in addition to productivity, I think it would be worth while to have estimates, however rough, of per capita income in the regional and industrial components underlying the national totals. These estimates would be useful background to the subsequent discussion and might help meet the objections of those skeptics who doubt whether the two economies are sufficiently similar to make comparisons of average income meaningful.

Having commented on the difference in income per head between the two countries, Young says that after making allowance for labor force participation ratios, the international investment position, and the industrial distribution of employment there remains "a hard core which appears to be of long standing." Allowing for labor force participation and the international investment position is necessary in moving from an income concept to a productivity concept, but I think it is illegitimate in this context to make an allowance for the industrial distribution of employment, since this is partly a product of the process of historical development which has been influenced by tariff policy and other factors the influence of which Young wants to estimate.

In comparing Canadian and American economic development, there are three ways in which we can make experimental reconstructions of history. One way is to ask how the economic development of North America would have worked out north of the Rio Grande had the formation of the Canadian nation in 1867 been followed fairly soon by something approaching economic union with the United States. This would have implied transcontinental railways south of the Great Lakes and free trade between the United States and Canada. In practice, free trade would have meant Canada's adopting the American tariff against other countries. These and other implications would have meant that economic union would have been tantamount to abandoning the idea of building a sovereign nation north of the United States.

The second way of reconstructing history is to ask a more restricted question. We may accept the decision to build a Canadian nation and ask what would have happened if different trade and transportation policies had been adopted. Canada might have had only one railway north of the Lakes (but one would be the minimum) and might have adopted a low tariff in the face of the high American tariff.

The third, and simplest, approach would accept the present situation for what it is and attempt to measure the economic burden borne by Canadians today on account of such avoidable and unavoidable overhead costs of nationhood as the railways across the Shield. This more modest level of abstraction offers less scope for imaginative thinking than do the others but it does have some immediate relevance to public policies affecting income distribution regionally and otherwise.

All three types of question are interesting and legitimate but we must be very careful to distinguish between them. Comments applicable to one type may not be applicable to the others.

Young hopes on some later occasion to say more about Canadian railway policy. For the moment, he reminds us of some familiar historical facts and refers to a tentative comparison of traffic output per worker on Canadian and American railways. To describe the contribution of railways to Canadian productivity one would need to know the productivity ratios for other inputs into railways and the traffic requirements per unit of output in other industries. But neither this information nor the financial data cited by the author would suffice to disentangle the consequences of duplication from the consequences of the geographical distribution of North American resources or the recent competition from other modes of transport. More detailed analysis would be required.

When he turns to tariff policy, Young addresses himself primarily to a question of the second type. He asks whether a low Canadian tariff in the face of a high American tariff would have affected Canadian economic development in such a way as to give Canadians a higher per capita income today than they now have. In suggesting that an appeal to international trade theory should settle the issue, Young is perhaps more optimistic than some others would be. The problem is not to find an optium allocation of resources in given circumstances but the even more tricky one of reconstructing a hypothetical pattern of economic development. There are theoretical models in which a very small exogenous change in the early stages of growth may have very large consequences later on. This proves very little, but it is a warning to us to be careful in performing intellectual experiments on economic history. In referring to a much broader topic than that before us—the evolution of the world economy since the mid-eighteenth century—W. W. Rostow wrote that "modern economic theory offers its students no agreed framework for disciplining such a story of growth and fluctuation." I think that in attempting to discipline the story of Canadian growth and fluctuation Young has tackled a very difficult job.

He will have to face certain questions, particularly those having to do with the applicability to the Canadian situation of the explicit and implicit assumptions belonging to the theory of resource allocation that he cites. Questions concerning the internal and external mobility of resources are crucial to allocation questions. Has protection diminished the flow of resources into the more desirable (unprotected) industries in allowing them to flow into the less desirable (protected) industries? Or has protection merely meant a

larger economy? Has it simply allowed a protected sector of the economy to develop alongside the unprotected sector without diminishing the size of the latter? And if so, to what extent has the greater size of the economy helped to reduce the per capita overhead costs of nationhood and to enlarge the market, incidentally increasing the opportunities to take advantage of economies of scale? And is it really true in the historical circumstances that if the tariff has attracted capital and enterprise, the returns to labor and land must necessarily have been reduced relative to the American? What are the implications of economic fluctuations and unpredictable swings in world demand? Has the presence of protected industries meant that per capita income has been higher than it otherwise would have been when foreign demand for exports has been slack? What have been the dynamic repercussions within the system of economic growth? Have the indirect consequences of protection stimulated economic growth or per capita income? Or the contrary?

Theory creates a very strong presumption that the Canadian tariff is guilty of the charge brought against it, but I should prefer, for the present, neither to acquit nor to convict but to fall back on that Scottish verdict, "Charge not proven." The author deserves our hearty thanks for having embarked on this piece of criminal investigation. He has picked an intriguing subject, and in his further pursuit of it he deserves all our good wishes for resounding success.

W. DUANE EVANS: I found both of these studies interesting and informative. I shall direct my remarks principally to Mr. Frankel's paper, although the later comments will, I think, have some relevance to the origins and persistence of the Canadian-American productivity differences which Mr. Young finds.

Mr. Frankel begins his paper with a table of rough US/UK productivity differentials for some thirty manufacturing industries (developed from the US *Census of Manufactures* for 1947 and the British *Census of Production* for 1948). He stresses the approximate nature of such comparisons, even when they are based on supposedly comparable physical measures of production, and his points are well taken. Few Americans, for example, with personal experience of British beer are likely to be completely satisfied with any direct physical basis of comparison, and I may add that most Britons feel just the same way about it.

Mr. Frankel says of his group of thirty industries that "there is little reason to suppose that the general impression it conveys of that sector (manufacturing) is seriously misleading." I agree with the statement, but I regard it as far from self-evident. The group certainly gives inadequate representation to the entire metals-machinery-equipment and metals fabrication area of manufacturing, which is represented exclusively by tin containers, pig iron, radio receiving tubes, bicycles, and razor blades. A broader picture is conveyed in the careful and more comprehensive studies of Laszlo Rostas which, however, are based on prewar data. But these, when supplemented by the various available trend studies of Rostas and others, show that the basic

postwar situation remains much the same as before. Mr. Frankel's findings are generally consistent with this very large body of work, and it is the existence of the latter which makes it probable that no serious challenge to the impressions gained from this limited sample of industries will be offered.

The author ranks his industries by the indicated size of the US/UK productivity ratio and remarks that no obvious explanation of the rankings presents itself. I shall suggest (without pressing the point) that, with some exceptions for special reasons, productivity differentials tend to be smaller for items with a relatively static product design and/or technology of production. For example, the product design of a smoked fish remains much the same, and, referring again to Rostas, the US advantage in motor vehicle production is greater than for bicycles, where model changes are somewhat less frequent and drastic.

Mr. Frankel begins his examination of possible causal factors with a discussion of the optimum use of resources in a given environment. I think this might be clarified by contrasting the concepts of efficiency and productivity. Efficiency typically connotes comparison with some more or less explicit standard which represents perfection. Many efficiency concepts are related in a rather static way to productivity. For example, proper skill and application levels in the work force or reduction in rejects and rework in the course of manufacture are efficiency concepts which have relevance to productivity. However, all these notions have one thing in common: once perfection has been achieved, no further improvement is possible. It seems to me that the notion of optimum use of resources in a given environment is a concept of the efficiency type.

Productivity, in contrast, is a ratio without an upper limit. It is closely related in concept with the idea of a changing technology, and its connotations are dynamic. If productivity is doubled, there is no inherent reason why it cannot be doubled again and yet again, and no comparison with perfection or any standard of excellence is implied. One firm, for example, may be highly efficient in the sense that it operates smoothly and without waste of resources in the context of the technology it employs, but at the same time it may be unprogressive, only marginally profitable, and with dim prospects for the future unless changes are made. Another firm may be inefficient in obvious ways, but progressive, productive, and profitable. I think comparable distinctions are important at the national level, and it is not efficiency in any ordinary sense that will be most relevant to international productivity comparisons.

As explanatory hypotheses for the differences in US/UK productivity, Mr. Frankel lists and examines differences in resources, equipment, market size, and plant size. In his final section, he adds differences in managerial and union attitudes and structures, workshop organization and control, and specialization of labor. He suggests that inadequate weight is frequently given in current discussions to these latter factors.

I was a little surprised that no mention was made of differences in labor, management, and consumer attitudes toward competition, and the degree of

competition prevailing. It is difficult to deal with this complex question in any quantitative way, but certainly many economists have felt that it is of importance in understanding productivity differences. Other topics for examination could also be listed, such as the training and educational system for the work force and the general public, distribution and marketing practices, consumer attitudes, the social structure and its effects, the relative importance of financial or production orientation among those who control industry, and many more. However, a number of the topics listed by Mr. Frankel and those which I have added as well seem to be less explanatory hypotheses than subjects for concomitant study. For example, if it is conceded that competition in Great Britain tends to be somewhat more restrained and hence exerts less pressure toward improvements in production techniques—and I believe this to be true—a most relevant question is surely why the role of competition is so different in two countries whose origins and history are so closely linked. I believe the basis for a fairly inclusive generalization exists.

To approach this quite directly, the technical ability of man to alter and reshape his environment, which had changed but gradually over a millennium or two, has jumped in the last two centuries by one to two complete orders of magnitude. The complex modern Western industrial economy, with its vastly different demands on and rewards to the average man, has no counterpart in earlier history. This technical revolution has in turn required far-reaching adjustments and alterations in basic social and economic structures and institutions. In the relatively isolated United States, these profound adjustments were made during a period when labor was scarce, not in relation to capital, which was also scarce, but rather in relation to the abundant opportunities for productive and profitable use which were everywhere evident. Comparable adjustments in Europe and Britain had to be carried through in a period when there was a surplus of labor in relation to visible opportunity. It is suggested that the differences in social and economic institutions and attitudes thus engendered have had a profound, pervasive, and persistent effect on the course of relative technical and economic development.

Some such hypothesis might well be suggested by evidence presented in Mr. Frankel's paper. He avoids one rather common misconception. Some persons, thinking perhaps of the more prominent position of the United States in world affairs following World War I, have been receptive to the facile conclusion that its leadership in productivity is a fairly recent development. It is clear that quite different explanations for the current US advantage will seem reasonable, depending on whether this is a recent happening or one of longer standing. The evidence presented by Mr. Frankel supports this latter conclusion. Indeed, I would not too readily concede even as much as Mr. Frankel, that "the inception of Britain's relative disadvantage belongs somewhere within the past century." One can as reasonably place it earlier. The very fact that productivity differences between the countries have had so long a history is a clear indication that explanations must be sought not in the rapidly changing realm of science and technology per se but rather in the less flexible and longer lived family of institutions and attitudes.

There have been, of course, many contributory factors, some of which have been mentioned. Others might include the linking in the United States of peoples of diverse origins, thus broadening the competition of ideas and avoiding traditionalism; the cultivation of a spirit of enterprise, because those who made the journey across an ocean and to a rough new continent more than a century ago must in the main have possessed it; the spreading of people over a vast land area and the receptiveness to mobility and change this inculcated; and others. Mr. Frankel rightly calls attention to the importance of opportunities on the land and at the frontier in earlier days.

With reference to Mr. Young's paper, I shall here suggest that traditionalism and social and economic institutions patterned to a greater degree around England and the Old World are more common in Canada than in the United States, and hence may have a significant bearing on the existence and continuity of the productivity differences which he finds.

Returning to Mr. Frankel's paper, it is said "the conventionally assumed relationship between land supply, wage rates, and intensive mechanization is not contested here." I think it is quite important to do so, because the conventional approach leads so many simply to the examination of substitution effects. This is an elegant tool for analysis where technology is essentially static, but inappropriate otherwise. Take, for example, a simple situation in the area from which the marginal approach was developed. A new seed variety —and we have new ones nearly every day—may with just the same land, labor, and capital give us substantially greater agricultural production. In other words, productivity has increased in terms of each factor of input, not because of substitution among them, but through the substitution of knowledge for ignorance. Much the same thing may happen when we obtain our textile fibers from chemical equipment and industrial labor in place of the farm land and farm labor used in making cotton. The great discovery of the technical era is that, through knowledge properly applied, we may be able to attain greater realized output in terms of every factor of production. The full significance of this was in no way appreciated in the nineteenth century. I doubt that it is in the twentieth.

I cannot close my remarks without recalling a debt which we can now repay only with remembrance. I refer to the untimely death at Cambridge in October of Dr. Laszlo Rostas, whose contributions to the study of international productivity differences, in my opinion, exceed those of any other man. His initial findings on US/UK differences, published in 1943, were greeted in some quarters with varying amounts of skepticism, amazement, and even indignation. It was his introduction, after valuable earlier work in other fields, to this curious subject where those with the least right to them often seem to hold the strongest opinions. The work of many scholars in subsequent years has served, of course, to validate the conclusions he offered. I am sure that the rather intense public discussions which were aroused by Rostas' work rather startled him, since he was by nature a gentle, quiet, and scholarly man. Nevertheless, it was in the climate created by these discussions that the postwar British productivity improvement program grew. I think it was Rostas'

initial valuable work and his quiet insistence on sticking with the facts which contributed more than anything else to the creation of the Anglo-American Council on Productivity and more recently the British Productivity Council. It branched across the Channel to the productivity work of the Organization for European Economic Co-operation and the more recently created European Productivity Agency, to both of which Rostas contributed personally as well as indirectly. He has richly earned from many of us our lasting gratitude.

REGIONAL ECONOMICS

CRITERIA FOR EVALUATING REGIONAL DEVELOPMENT PROGRAMS

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I

A regional development program will suggest different things to different individuals, and to all it is likely to suggest not a single program but a number of related programs focusing on a common objective. To a staff member of a development agency, a regional development program implies literally scores of individual programs ranging from those involving engineering projects to those basically educational in nature carried out co-operatively with educational and other regional institutions. Since these programs are neither comparable nor amenable to uniform methods of analysis, a complete discussion of criteria for their evaluation cannot be attempted in a paper such as this. This paper will discuss the problem only in general terms, therefore, to sketch out at least the nature of the problem and to inquire into some general lines of approach in handling it.

To indicate the scope of the paper and to begin with a common understanding as to terminology, let us start by defining what will be meant by various terms employed.

This paper will skirt the controversy as to what constitutes a region, since opinions among economists vary from a belief that the concept of a region has little value,[2] to the opposite position that there can be demarcated "but one best set of regions."[3] For the purpose of this discussion it will be adequate to define a region simply as an identifiable geographical area, whether a river basin, an administrative unit, or a combination of administrative units, depending on the specific problem at hand. Regions to be considered will be subareas of the United States and thus will represent open economics engaging in interregional trade. The paper will confine itself, also, largely to the problem of relatively underdeveloped regions.

[1] The author is indebted to Pierre Crosson, Henry Erlanger, John Peterson, and Vernon Ruttan, of the TVA, and Otto Eckstein for their comments and suggestions on an earlier draft of the paper.

[2] Rutledge Vining, "The Study of the Spatial Structure of an Economic System." Paper presented at the December, 1951, meetings of the American Economic Association.

[3] Walter Isard, "Some Emerging Concepts and Techniques for Regional Analysis" (*Proceedings* of the Western Exploratory Group of the Social Science Research Council on Regional Economic Analysis, Berkeley, 1952).

Regional development, for our purposes, will be defined as a process resulting in a secular growth of regional output or real income. (This, of course, is less than a comprehensive definition of regional development, as many intangible aspects of development are not susceptible of measurement by a monetary yardstick.) This growth can occur autonomously. It may be influenced, also, by the impact of national economic expansion imparted through the private sector of the economy. A regional development program, therefore, must be distinguished as an influence exogenous to the normal functioning of the private economy. For the purpose of this discussion, a regional development program will be defined as a public undertaking, most likely a governmental undertaking, one of whose primary objectives is to influence the underlying factors affecting regional output.

II

Attempts at evaluating regional development programs in the past have frequently used an aggregate economic indicator, such as total personal income, to compare relative changes for a region in which a development program exists with relative changes for another region. This comparison between two regions over an appropriate time period is assumed to reveal the extent to which development efforts have been economically justified.[4] This procedure is dubious at best for it implies that the development efforts being evaluated, to be effective, must have exercised a dominant influence on regional income—and in the relatively short run. The fact that statistical data which have been compiled in such attempts to evaluate the TVA, for example, have not appreciably influenced the area of agreement regarding the value of TVA programs suggests that this approach falls short of providing evaluative results. An examination of the implicit assumptions underlying this approach will afford some excellent reasons why this may be the case.

First, an evaluation by this method implicitly assumes that the comparison being made is between a region which has a development program and one in which such a program is lacking. But if relative changes in total income are used to measure regional development, the influence of other governmental programs which affect regional income, regardless of the administrative device employed, must be eliminated from both regions to provide the *ceteris paribus* conditions essential for a valid comparison. It is seldom recognized, for example, that the

[4] See, for examples, B. U. Ratchford, "Government Action or Private Enterprise in River Basin Development: An Economist's View," and discussant Joseph Ransmeier's remarks, AEA *Papers and Proceedings*, May, 1951, pp. 304-306, 311-312. Also, "Study of Agricultural and Economic Problems of the Cotton Belt," *Hearings before Special Subcommittee on Cotton of the Committee on Agriculture* (House of Representatives, 80th Congress), p. 687.

activities of the TVA to a considerable extent supplant activities which are carried out by a number of other federal agencies in other regions. The relative magnitude of governmental expenditures for programs other than resource development is likewise almost never taken into account. Federal expenditures for military procurement in California and New York during a five-year period (1950-54), for example, were roughly seven times as large as federal expenditures associated with the TVA over a twenty-one year period. In many other states, although not as large, they were multiples of the twenty-one year total of TVA expenditures. These expenditures, along with grants-in-aid to states, federal construction, and various other direct governmental activities throughout the nation, suggest that the absence of valley authorities does not preclude the possibility that federal expenditures in regions other than the Tennessee Valley account directly for a larger portion of their growth in income.

Second, regional comparisons of income relatives as a method of evaluation implicitly assumes that the activities of development agencies are encompassed within a relatively well-defined geographical area. In reality, since development agencies usually have responsibility for a number of resource problems rather than the economy of a well-defined region, development activities may radiate into adjacent regions observing no common boundary for the entire "package" of resource development programs. For example, the water control program of the TVA is confined to the 125 watershed counties in parts of seven Valley states. The power program relates to a somewhat different area of about 170 counties in these states. TVA's activities in regard to tributary watershed development involve relations with the seven Valley states and its national responsibilities for fertilizer research and development involve co-operative relations with institutions in 35 states. The incidence of benefits stemming from these programs, moreover, is not confined to the areas in which the programs are centered. It is not meaningful, therefore, to take one area influenced by a development program for comparison with another area in which the program's influence has also been felt, in evaluating program results.

Third, such attempts at evaluation involve hidden assumptions that, despite differences in the variety and quality of resources which exist among regions, all regions have a uniform development potential. Stated differently, an assumption is implied that regardless of the differences which may exist among regions in all other determining variables, every region can be expected to develop economically as rapidly as another except for the presence or absence of a development program.

To sum up, then, past attempts to evaluate regional development

programs by comparisons of income relatives have not examined critically what constitutes a regional development program apart from the administrative machinery used to implement it, what constitutes a region for the purpose at hand, and how realistic are other assumptions which are implied to hold.

However, quite apart from these critical observations, there remains another question of importance which seemingly has escaped attention. The degree to which a program can be expected to stimulate regional development depends largely upon the magnitude of the program relative to the total of other factors affecting the region's development. A comprehensive regional development program might conceivably involve responsibilities for monetary and fiscal policy, welfare programs, regulation of transportation rates, interregional trade and migration, and other governmental activities which vary with political philosophies and the contingencies of a particular time and place. Such programs would involve all of the measures with which government can influence the level of income. By contrast, regional development efforts in the United States, such as resource development programs, represent efforts of rather limited scope when viewed in relation to the aggregate of economic activities which engage the productive factors of a region. Given the margin of error in our economic data, is the influence which such limited development programs can be expected to have on regional income capable of being accurately indicated by comparisons of relative changes?

It has been observed by Morgenstern that a probable error of 10 per cent attaches to our estimates of national income. (*On the Accuracy of Economic Observations,* page 84.) There is little reason to believe that estimates of income on the regional level are more accurate. If these estimates are used to provide measures of relative change, a regional development program would have to contribute a third to the income of a region in a given year before its influence would be equal to the range of possible error in an index whose base (prior to program introduction) and given year estimates are subject to a 10 per cent error. To illustrate: The index of estimated income payments from manufacturing for the year 1953 (1929 = 100) was 543 for the 11 southeastern states and 602 for the 201 counties representing the Tennessee Valley and TVA power service area—a difference of 59 percentage points. If we assume that the true income was underestimated for the Tennessee Valley while overestimated for the Southeast in the base year, and overestimated in the given year for the Tennessee Valley while underestimated for the Southeast (all by as little as 10 per cent), the true index would be 664 for the Southeast and but 493

for the Tennessee Valley. If estimating errors of opposite sign but equivalent magnitude for the base and given years are assumed, the true index for the Southeast would be 445 and 735 for the Tennessee Valley. With a range of possible error exceeding 200 percentage points in the index, we cannot feel secure that the indicated difference of 59 percentage points has not been influenced more by errors of estimation than by the development program. Since errors in estimating can produce a wider range of differences in our income relatives than can be realistically anticipated from the influence of limited development programs, one can legitimately question the validity of using comparative data on income (and other) relatives to evaluate development program results.

III

If relative changes in our economic data are not likely to provide a sufficiently accurate basis for evaluating development programs, what alternatives remain? The most fruitful approach, it seems, is one which examines each program activity individually for its impact on the sector of the economy toward which it is directed. One method of this type seeks to provide approximations of real income produced (or losses averted) by such programs which can then be compared quantitatively with costs. This procedure is used to indicate whether the criterion of efficient resourse allocation has been met.

This approach presents several problems. It is desirable to recognize a specific limitation at the outset. Taking the TVA as an example, again, there are programs such as flood control, navigation, and power for which the development agency has exclusive responsibility and from which direct benefits are largely tangible. On the other hand, there are programs, such as tributary watersheds, stream sanitation, forestry, agriculture, and scores of others, which are co-operative ventures involving agencies of state and local government in addition to TVA. These programs involve research, development, and demonstration, in the main, and are fundamentally educational in orientation. Given the state of our research methods and knowledge, developmental results of some of these programs are scarcely less difficult to assay than results of research and public education generally, nor can quantitative valuation be placed on them more easily. With limitations on estimating the benefits of this type of program and shared responsibility for the results, a precise quantitative comparison of benefits and costs from the agency viewpoint does not seem feasible.

This type of program, however, has accounted for only about 10-15 per cent of total expenditures in the case of TVA. The major portion of this relatively small share, moreover, is accounted for by TVA's

national responsibilities for fertilizer research and development and defense chemicals. Costs of the fertilizer and munitions program, while not found in other regions, are largely recovered by sale of munitions to defense establishments and receipts from fertilizers distributed in agricultural programs. Resource development expenditures, apart from the chemical, power, navigation, and flood control programs, therefore, have accounted for only about 3-4 per cent of total program expenditures. The remaining and preponderant portion of expenditures is more readily amenable to quantitative analysis.

Accordingly, an evaluation might proceed by analysis of each program activity which provides opportunity for quantitative evaluation. This will permit judgment as to how efficient an allocation of resources the major portion of program activities represents. In the private sector, of course, the pricing system is assumed to provide a reasonably efficient method of allocation. In the public sector, on the other hand, investment decisions must often be made to provide services which are not available through conventional markets. Public revenues from taxation can be used for these purposes without reference to costs in the capital market, while valuation of the services provided also frequently cannot be obtained directly from the operation of product markets.

This poses a difficult problem of establishing procedures for estimating costs and "returns" to which a good deal of attention has been given and to which a satisfactory solution may emerge. The continuing work of the Subcommittee on Benefits and Costs of the Federal Interagency River Basin Committee[5] is developing a conceptual framework for estimating benefits and costs of river basin projects. Briefly, the method of estimating costs recommended includes imputation of a rate of interest to cover real costs of project capital as well as other costs incurred through conventional factor markets. Valuation of services provided is made by estimating actual or imputed market value of these services, usually measured by the cost of the most economical alternate method of providing similar services. A ratio of benefits to costs is then computed and, assuming that project capital (and other factors) would otherwise be employed at the margin where returns would be just equal to costs, opportunity costs will be met for the total of factors employed in the project if the benefit-cost ratio exceeds unity.

This *ex ante* approach to investment decisions in the public sector can be extended with appropriate modification to evaluation of such

[5] *Proposed Practices for Economic Analysis of River Basin Projects;* see also John M. Clark, Eugene L. Grant, Maurice W. Kelso, *Report of the Panel of Consultants on Secondary or Indirect Benefits of Water-Use Projects,* to Michael W. Straus, Commissioner, Bureau of Reclamation.

programs after they have been in operation. Established operating programs, moreover, provide known construction and operating costs and experience on which to base estimates of benefits. In this connection one consideration bears stressing. Facilities employed in such programs have useful lives which are limited, in the main, only by technological obsolescence. Related development which such programs induce will doubtless require some time to gain momentum. Evaluation of existing programs, therefore, should avoid judgment based on the first year's or an average of early years' results. Cumulative costs over the life of the program should be considered in relation to cumulative benefits expected to accrue over the entire life of the facilities as is done in conventional benefit-cost analyses. Projections based on experience with costs and benefits to date should facilitate estimating realistic cumulative totals. The problem posed by the difference in time distribution of outlays and benefits must be taken into account, of course, and can be handled by use of the interest rate or discount factor.

While this procedure provides criteria for evaluting development programs, it has a national rather than a regional focus; i.e., it is addressed to the problem of allocating resources efficiently within the total economy. Without supplementary analysis to determine the spatial distribution of benefits from program activities, it fails to provide much insight into the effect such programs have had on the development of the region itself. To answer this question, a somewhat different approach may be preferred for analysis and evaluation from a regional viewpoint.

IV

Regional development has been defined as a process resulting in a secular rise in regional output or real income. Increasing regional output and income implies both expansion of the region's productive capacity and growth of effective demand for the region's output. Both conditions are necessary and combined will represent a sufficient condition for regional development. Attention has often been sharply focused on increasing output through stimulating increased participation of underutilized resources and/or increasing the productivity of resources being employed. Perhaps not enough attention has been directed to the process by which effective demand may be increased; namely, through retaining an adequate share of the resulting increased factor receipts in the region to increase the region's effective demand for its own output and ensuring that the increased output include goods and services for which there is a highly elastic demand both from within and outside the region.

The demand aspect in regional development becomes particularly

important when viewed in the light of a rising secular trend in national income. In fact, resource development programs derive their justification largely from the implicit assumption that long-run economic growth will require the factors or services provided by such programs. This assumption must be examined carefully, however, in connection with each program activity. But within the context of an expanding national economy, the degree to which development efforts will make a contribution to underdeveloped regions will depend on how effectively they assist the regions to supply, over time, a greater share of the total economy's expanding demand for goods and services.

Development programs may be evaluated from the regional viewpoint, accordingly, in terms of their aid in increasing the productive capacity of the region and in stimulating effective demand for the region's output.

To promote these dual objectives, resource development programs of the kind undertaken by the federal government should focus attention (within the limitation of their statutory authority) on influencing the supply functions of factors for which demand is highly responsive to secularly rising income nationally. These factors may be referred to as "strategic" factors. They will consist of productive factors for which demand will be relatively elastic both with respect to price and income, and favorably influenced by changing technology and tastes. Development programs which alter favorably the supply functions of strategic resources will improve the region's competitive position in the national market. Cross-elasticity of the derived demand for localized resources will increase the probability that location advantages thus provided will attract external capital to the region. Branch plants of national concerns represent a typical form of such capital movements in a nationally expanding economy. If the strategic factors, furthermore, are required by "growth" industries, i.e., those industries for which product demand is income elastic and influenced positively by the trend in changing consumer preferences, the rate at which capital formation will occur in response to shifts in regional supply functions and the extent to which these and complementary resources will be employed can be expected to increase.

Criteria for evaluating the developmental effects of regional development programs might then be: how effective were the development programs in shifting the supply function of factors on which their efforts were focused and how strategic, as indicated by their demand characteristics, were the factors whose supply functions were thus affected?

As an illustration, we know that multiple-purpose development of the Tennessee River, by altering the production function for the region's

power, produced a substantial shift in the supply schedule. Development of an inland waterway increased the supply of low-cost transport services. The water control program, by regularizing stream flows and permitting settling to occur in the reservoirs, lowered the cost and increased the dependable supply of high-quality processing and cooling water.

We know that these factors, i.e., power, water, and in some cases water transport, are basic inputs in the chemical industry, which incidentally has experienced a half-century of sustained vigorous growth. With respect to product demand, as an example, we know that changing technology, rising income, and changing consumer preferences have altered the derived demand for industrial fibers. There has been a spectacular substitution of synthetic for natural fibers. This in turn has fostered a vigorous demand for factors employed in the production of synthetics. In response to this growing national demand and the procurement advantages in the Tennessee Valley for basic inputs, investment in facilities along the Tennessee River for the production of synthetic fibers has exceeded 50 million dollars during the past several years alone.

Stimulating capital formation is not limited to providing procurement, processing, and distribution advantages directly for new industrial enterprise. Such advantages provided originally may be reinforced by triggered developments. The most spectacular illustration of this in the Tennessee Valley is the developing chemical complex at Calvert City, Kentucky, near the base of Kentucky Dam. The initial development at Calvert City took place in 1948 when Pennsylvania Salt Manufacturing Company erected a hydrofluoric acid plant. In addition to fluorspar, the basic requirements of the firm were relatively low-cost electric energy, large quantities of processing and cooling water, and water transportation for shipment of sulphur and salt. Concurrently, the Pittsburgh Metallurgical Company built facilities for producing ferroalloys to realize savings through joint development of docking and rail facilities. Pennsalt added a chlorine plant which, along with B. F. Goodrich's plans to locate vinyl chloride facilities in the area, prompted construction of a calcium carbide and acetylene plant by Air Reduction. The latter's location decision was influenced by the favorable source of intermediate products supplied jointly by Pennsalt and Goodrich, along with other favorable location characteristics of the area. These three firms now have two plants each at Calvert City which, along with Pittsburgh Metallurgical Company and the recent addition of General Aniline and Film Corporation, represent an investment of approximately 70 million dollars—only slightly short of the total investment in flood

control, navigation, and power facilities of the Kentucky Dam project. This capacity has been built within a period of ten years after completion of the dam. The favorable opportunities for realizing agglomeration economies provided by the developing industrial complex, moreover, suggest a triggered development which in the long run will dwarf the resource development expenditures necessary, although not sufficient, to spark it initially.

Analyses of the industrial demand for power, use of the navigation channel for movement of bulk materials, and use of industrial water supplies in the Tennessee Valley suggest that the water resource development programs have attracted the light metals industries, additive metals using electrometallurgy, synthetic fibers, and other chemical industries. Investment since the beginning of TVA's resource development activities in plant and facilities along the Tennessee River (exclusive of TVA's installations) for which industrial water supplies, water transportation, and/or relatively inexpensive electrial energy are significant location factors, totals approximately 2 billion dollars. Data and conceptual limitations preclude our measuring how much the rate of investment has been increased. We know, however, by computing the location quotients for these industries, that a larger proportion of the rapidly growing new capacity nationally has been built in the Tennessee Valley since the advent of the resource development programs than was being built prior to it.

Although the rate at which capital is formed will affect the rate at which regional income will increase, there may exist a sufficient difference in the ratio of the increment in income to the increment in investment, over time, to merit comment. Investment expenditures of a development program in an underdeveloped region, as well as triggered investment in the early phases of development, will produce something less than the full multiplier effect on that region's income. In the first place, the region will be unable to supply from domestic production a large part of the investment goods required for the program. In the second place, as indicated by family budget studies and the economic structure of underdeveloped regions, the marginal propensity to import from the rest of the economy associated with the second as well as the first round of new expenditures is likely to be very great in relation to the marginal propensity to import from the region by the rest of the national economy. This results from the low income elasticity of demand for a large portion of the region's output (agricultural products, textiles, etc.). The net effect of the regional multiplier will be restricted largely to that produced by expenditures for investment goods and services which can be supplied from regional production. Only a small

additional export multiplier can be anticipated from the national economy's imports from the region resulting from its exports of investment goods to the region.[6] Accordingly, despite the high marginal propensity to consume among low income underdeveloped regions, the region's inelastic supply of investment goods and the low income elasticity of demand for the region's output, in the short run, will limit the multiplier effect of capital formation on the region itself.

If the regional development programs have focused on development of strategic resources, increased factor receipts from the employment of these resources in the region by material-oriented industries will provide, in time, the increment to total income necessary and sufficient to attract market-oriented consumer goods industries for which final demand is income elastic. This process of development reflects a change in the composition as well as level of regional output. If the region thus increases its capacity to supply from home production its demand for income-elastic goods as well as supply a larger part of the expansion in demand originating nationally, *ceteris paribus*, the regional multiplier effect of capital formation both regionally (investment multiplier) and nationally (export multiplier) will tend to increase.[7]

The effects on capital formation and the income multiplier of development programs which improve the region's capacity to supply strategic factors can be contrasted with results to be anticipated from

[6] The regional multiplier in this case will be similar to an investment multiplier involving two countries (cf. Fritz Machlup, *International Trade and the Foreign Trade Multiplier*, pp. 175-176) where the fraction of investment goods and services provided by domestic production is equivalent to "home investment" in the Machlup formulation and the fraction of investment items provided from outside the region is akin to investment in the "second country."

The regional multiplier can be expressed as follows:

$$y = \frac{ai\ (s' + m') + m'a'i}{(s' + m')\ (s + m) - m'm} \text{ and } k = \frac{a(s' + m') + m'a'}{(s' + m')\ (s + m) - m'm}$$

Where:

y = change in regional income associated with new investment; i = total amount of new investment; a = fraction of investment goods and services provided by regional capacity; a' = fraction of investment items provided by remainder of the economy; m = marginal propensity to import by the region; m' = marginal propensity to import from the region by remainder of the economy; s = marginal propensity to save in the region; s' = marginal propensity to save of the remainder of the economy; k = the regional multiplier.

[7] The increase in regional income accompanying new capital formation can be obtained from the following expression:

$$y = \frac{ai(s' + m') + m'(a'i + i'')}{(s' + m')\ (s + m) - m'm}$$

Where all symbols remain as previously defined while i'' represents new investment nationally outside the region. The numerator will be increased by larger values of a and m' in the first term and the increased value of m' which applies to i'' as well as the decreased value of a' in the second term. The denominator will be decreased by opposite changes in values of m and m'. The latter follows because of the different order of magnitude in the change in import propensities governed by the relative magnitude of the trading areas.

programs which focus on resources for which demand is relatively inelastic. The agricultural sector is useful as an illustration in this case because final demand for agricultural products generally is characterized by both price and income inelasticity. Growth in demand for agricultural output and hence for total factor input will not be proportional to growth of national income. Although substitution of factors, e.g., fertilizers and capital for land and labor, may result in a derived demand for some factors employed in agriculture to grow more than proportionally with demand for agricultural output, aggregate factor demand in agriculture will be subject to relative decline. Given an absence of production controls and price supports, resource development programs which contribute to increasing the aggregate factor employment in agriculture would tend to affect adversely the terms of trade between agriculture and other sectors of the economy. Because of inelastic demand, income from agriculture may decline by virtue of the increase in output. For a region which is predominantly agricultural and seeks to achieve development through these means, the development effort will produce an extraregional consumers' surplus with regional benefits being eroded away by adverse changes in the terms of interregional trade.

V

In summary, one may conclude that any meaningful evaluation of a regional development program requires intimate knowledge of the many activities which it represents. Furthermore, an approach which attempts to evaluate each program activity individually for its impact on the sector of the economy toward which it is directed is more promising than one which attempts to determine justification for the entire package of development programs by analyses conducted at levels several stages removed from the area of direct program impact. Moreover, it is probable that development programs which are financed nationally have essentially a national rather than a regional focus and that benefits from such programs are not confined within the region in which the program is centered. Regional comparisons of relative change in so-called "economic indicators," therefore, are not appropriate to the problem being posed. Moreover, because of the wide margin of possible error in indexes using such data, they provide a specious measure of the success of regional development programs, even should other conditions required for their use be fulfilled.

Benefit-cost analysis, while approaching the problem at the appropriate level, is designed to provide an investment criterion rather than criteria for evaluating developmental effects on the region in which the program is centered. Perhaps more useful for this latter purpose is an

analysis of the process of development and how regional development programs may influence that process. This requires an examination of the effects such programs have on the parameters of the region's production and consumption functions over time and the trend in the structure of markets. Although precise quantitative criteria may be more difficult to formulate in this case, an approach along these lines is likely to be more relevant, more informative, and more suited for analysis of the dynamic aspects inherent in problems of economic development.

REGIONAL ECONOMIC REACTION PATHS

By F. T. Moore
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When the economy of a region is subjected to changes in the level of activity of one or more of its sectors, both the real structure of the regional economy and the monetary flow of receipts and expenditures between it and other "economies" affect the character of the response to that change. For a given change in the level of economic activity the effects on income, employment output, etc., will be described as a reaction path for the region. It is the purpose of this paper to suggest certain kinds of changes in economic activity which may prove fruitful to analyze and to attempt to measure quantitatively the effects on specific regional economies of given types of change; that is, to describe certain kinds of these reaction paths. In order to trace reactions to changes in economic activity, it is necessary that the interrelationships among the sectors in the regional economy be specified as to form and be measurable. Although it may be possible to trace through the effects without the aid of a formal model (though on this point I have my doubts), I shall use input-output (or interindustry) models for the states of Utah and of California as the vehicles for measuring regional effects of changes in economic activity; for these kinds of problems such models have virtues which I hope will become apparent as the discussion goes along.

The paper is divided into three parts. First, some discussion of static input-output models, their relevance for regional problems, and the defects which may make the purely static model inadequate is necessary. This section is limited to a very few comments which lead particularly to the model described in the third section of the paper. Second, using the input-output models for Utah and California, changes in income and employment in those states resulting from changes in the "external" demand for individual industries will be examined. The measures developed may be considered as income and employment multipliers. An extension of these techniques also yields estimates of accelerator coefficients for California. Third, the California input-output model will be adapted to the computation methods for linear programing so that a dynamic model of growth over the period 1954-60 may be examined.

I

It is too much to expect that the regions in the United States with which we work will normally exhibit a balanced economic structure.

There is, of course, room for a variety of definitions of the appropriate regional unit, and the economist, geographer, political scientist, and urban specialist can choose the unit most appropriate to his problem. There is merit, I think, in considering the state as a basic unit for regional economic models, since among other reasons, it is sovereign in an interesting area of economic policy making. Unlike the nation-state, however, the police power of the state does not extend to laying restrictions on interstate commerce. Let us consider just a few implications of that prohibition against interference with interstate commerce on the part of states, as it affects regional models and regional analysis.

In constructing a regional input-output model such as the ones for Utah and California which are used later in this paper, the analyst has the choice of letting the cells show the total demands of the industries in the state without distinguishing those demands satisfied by industrial capacity in the state from those satisfied by "imports." Alternatively, the gross flows table can be constructed so as to show the intrastate flows alone, relegating imports (and exports) to the exogenous final demand sectors. The Utah and California models follow the second method with the result that the intrastate flows are well articulated, but a side calculation of serious proportions is necessary in order to account properly for imports. All this is old hat to the seasoned input-output analyst, but consider how it complicates the analysis of regional growth. For a given regional input-output model, assume that the outputs from all industries necessary to support the delivery of a given level of final demand (for example, regional consumption and investment demands, and exports) have been computed. A comparison between the required level of output and regional capacity can then be made for each industry individually. If for one or more industries required output is greater than capacity, a choice is implied between building new capacity in the region or raising imports. Short-run prospects of drawing down existing inventories or rationing of demand through price increases are ruled out for the moment.

Whether the region becomes permanently a deficit area by increasing imports or makes a readjustment by building new capacity depends on the cost comparisons, not only in the deficit industries, but in all industries in the region. If some industries have exportable surpluses while others have deficits, comparative cost studies will determine the most desirable course of action. The deficits may accumulate; or the region's comparative cost advantages may point to a further development of the industries which already are net exporters. If a choice must be made between two deficit industries, the cost studies will show the efficient relative rate of growth of the two in the region. In any case, a formal input-output model does not automatically supply the answer

or the criteria by which the choice is to be made. All it can show is the "trade off" in real cost between development in one industry and the alternatives of development in other industries which are thereby foregone. The direct cost of adding a unit of capacity to a given industry is measured by the set of capital coefficients (i.e., the real quantities demanded in constant dollars per unit of capacity) appropriate to that industry. An increase of X units in capacity thus requires X times each capital coefficient from other industries in the state or from imports.

The problem may be framed in another way to ask whether a given bill of goods is feasible in a static model which allows capital building to take place but which makes no explicit allowance for time lags. Assume as before that for the given bill of goods the output required to satisfy it has been determined by multiplying the vector of final demand by the inverse matrix, thus getting both the direct and indirect requirements for output. If the generated output is greater than existing capacity for one or more industries, new capacity building must take place. This in turn places additional requirements on the other industries in the region, and one or more of these suppliers may in turn require an expansion of facilities. Such a model thus gives rise to two sets of equations of the following form:

$$(1)\quad X_i = \Sigma_j\, a_{ij} X_j + \Sigma_j\, b_{ij}\, m_j + f_i$$
$$(2)\quad C_i = X_i - m_i + u_i$$

where X_i = current output; C_i = initial capacity; f_i = final demand; u_i = unused capacity—all for industry i and where $[a_{ij}]$ and $[b_{ij}]$ are flow and capital coefficients respectively; m is the vector of additions to capacity built during the period under analysis.

The first set of equations shows the disposition of current output among the alternative uses of sales on current and capital account and to final users. The second set of equations simply shows the allocation of capacity to uses including the possibility of its being unused. These equations thus are perfectly straightforward applications of social accounts principles. They may be compared with the equations in the third section of this paper.

Since choices between use of resources on current or on capital account may be made, this model could be calculated by linear programing methods; but since it is a purely static model, there are no lags in building and using new capacity so that the solution, if there is one, must satisfy the final demand subject to the restriction that capacity must be at least equal to requirements for output from this industry on current account and capital account. There may be no solution, since each capacity expansion in one industry may generate new capacity requirements in other industries. The model then "explodes."

Thus, such a static model, even if expertly used, may be inadequate to handle relatively simple dynamic situations. The model suggested in the third section of this paper is an attempt to move to a more meaningful formulation for handling time lags on production and capacity building and for showing the time paths of output in growth models.

II

The changes in economic activity which affect the regional economy may arise either internally or externally; that is, as a result of growth within the region, a shift in the composition of demand in the region or as a result of changes in the level of economic activity in the nation which in turn affect the regional economy. Both kinds of changes are important; a growth in population in the region will raise consumer demand for products and services and will perhaps shift the composition of demand among industries, or it may simply raise the level of demand without changing its composition. The development of a new resource will inject new investment flows into the region; thus engaging the multiplier process with its direct and induced effects on income and employment. Or a change in the federal budget—let us say the expenditures on defense—will most certainly have an effect on the economy of California which has a big aircraft industry, shipbuilding, and many military installations. In fact, almost any change in the gross national product will change the demand for the products of some industries in the region and will, within a short time, have an effect on income and employment and the general level of regional economic activity.

In an open input-output model, all of the above types of changes are covered in one or more of the sectors of final demand. It is common to distinguish in final demand the sectors: household (consumption), producers' durables (part of investment), construction, federal, state, and local government, and exports. All of these are exogenous or external to the main structure of the model. The levels of these sectors must be specified independently. Then these final demands and the coefficients of the model determine uniquely the levels of output required. As the final demands change due to any of the factors mentioned above, the output levels (and hence income and employment) will also change. Thus for convenience in measurement, we may lump all such changes in final demand, without specifying the type of change, and go on to compute the multiplier effects on regional income and employment of any type of change in "external" demand.

The precise character of the change in external demand—whether the result of new investment arising autonomously, new consumption resulting from population growth, etc.—need not be defined at the moment, since the multiplier effects of changes in levels of activity can be carried

out in terms of a unit (one or one million dollars), and the direct and indirect effects of the changes can be evaluated.

When a change in final demand occurs in one or more industries, the immediate impact is likely to be on finished inventories of the product but as soon as production plans can be readjusted, the change will be translated into changes in output. Let us call this direct output change (equals change in final demand) for industry k, Δf_k and let us assume it has the unit value of \$1.00. Now the direct effects on income can be obtained by multiplying Δf_k by the household coefficient for industry k, that is, a_{hk}. This coefficient, it will be recalled, shows the payments made to households (as a sector) in the form of wages, salaries, etc., per dollar of output.

The direct change in output in k will, however, cause the industries supplying k to readjust their own production plans, and these adjustments will set off third- and fourth-order reactions. A cut in output by industry k will thus have both direct and indirect effects on output, each industry both affecting and being affected by the "rounds" of these changes. The direct-plus-indirect effects on the output of all other industries of a change in output by industry k are shown by the ***k-th*** column of coefficients in the inverse matrix. Define $(I\text{-}A)^{-1} \equiv [d_{ij}]$. Then d_{2k} shows the direct-plus-indirect effects on industry 2 of a \$1.00 change in the final demand for industry k. Then it follows that the direct plus indirect effects on income of a \$1.00 change in final demand for industry k are given by: . .

$$a_{h1}\, d_{1k} + a_{h2}\, d_{2k} + \ldots . + a_{hn}\, d_{nk} = \Sigma_j\, a_{hj}\, d_{jk}$$

and the direct income effects (for the \$1.00 change) are given simply by the household coefficient, a_{hk}. We now define the "simple" income multiplier as the ratio of the direct-plus-indirect income effects to the direct alone.

$$M_k = \frac{\Sigma\, a_{hj}\, d_{jk}}{a_{hk}}$$

where M_k is the simple income multiplier for k. Such multipliers have been computed for each industry in the regional input-output models of Utah and California; they are shown in Table 1 together with multipliers computed from a US model for comparison.

Each column of multipliers taken separately can be interpreted without confusion, but comparisons among the multipliers for a given industry in the three models should only be made with the greatest of care. As can be seen from Table 1, the number of industries is not the same for the three models; thus the aggregations which have been performed result in industry classifications which are most difficult to compare. The California model was aggregated from a basic 56 order

model and Utah a basic 26 order model. To what extent the multipliers are sensitive to the changes in the classification system resulting from different kinds of aggregation is not known with any certainty, but the sensitivities are perhaps substantial. In California, petroleum is by far the largest part of the industry, "petroleum and fuels," whereas in Utah coal is larger. Contrasts of the same kind could be made for other in-

TABLE 1

SIMPLE INCOME MULTIPLIERS FOR CALIFORNIA, UTAH, AND THE UNITED STATES

Sectors	Simple Income Multipliers		
	California	Utah	United States
1. Agriculture and fishing	1.66	*	§
2. Mining	1.25	1.15	**
3. Petroleum and fuels	1.83	1.41	2.12
4. Processed food	3.32	2.39	4.45
5. Textiles and apparel	1.55	†	
6. Lumber, furniture, paper	1.68	†	2.08
7. Other manufacturing	1.72	†	3.40
8. Primary metals, foundries	2.21	‡	2.54
9. Fabricated metal products	1.75	†	2.29
10. Nonelectrical machinery	1.63	†	2.03
11. Electrical machinery	1.70	†	
12. Transportation equipment	1.63	†	2.72
13. Utilities	1.04		††
14. Trade	1.21	1.25	2.62
15. Services	1.43		

* Included with processed foods.

† In the Utah model these industries were lumped under "Other manufacturing" with a value of 1.44.

‡ In the Utah model this was split into two industries with multiplier values as follows: iron and steel, 2.52; nonferrous metals 8.11.

§ Included with processed foods, textiles and apparel.

** Split and included both in primary metals and petroleum and fuels.

†† Included with petroleum and fuels.

SOURCES: The California figures are from an unpublished input-output study by the writer and Dr. James Petersen, of the RAND Corporation; the Utah figures are from a paper, "Regional Analysis: An Interindustry Model of Utah," by the same authors and soon to be published in the *Review of Economics and Statistics;* the US figures are from a model basically representing an aggregation of the 1947 national interindustry model. All three models are in 1947 dollars.

dustries, so that the economic meaning of comparisons of the multipliers may be greatly vitiated.

There is one explanation for the differences between the state and US multipliers which is at least consistent with the observed numerical results. For every industry in Table 1, the multipliers applicable to the national model are larger than for the corresponding figures for Utah and California, and in virtually every case the difference appears to be substantial. It is reasonable to explain the lower values for the states as due to the leakages which occur in the purchase of goods and materials as successive industries readjust their output. There are a num-

ber of products which Utah and California do not produce or do not produce in sufficient quantity to satisfy the demands of other industries in the state as the general level of economic activity increases. Even when a decrease in activity is experienced it is not certain that imports will suffer the most since the state industries may not possess cost advantages. Imports and exports bulk larger in the states' economies than in the nation's. If output is to be expanded, a part of the expenditures will go for imports, thus leaking out of the receipts and expenditure stream of the state. On the other hand, of course, exports inject new flows of receipts into the state. Whether on balance the leakages occasioned by the imports are larger than the stimulus supplied by exports will depend on the specific circumstances. It is probable that the effects will be different for a general increase in output from what they are for a decrease. In both the Utah and California models, imports seem to be of such importance in the large that the income multipliers are smaller than for the corresponding industries in the US.[1]

A look at the income multipliers for a state such as California reveals some interesting things. The larger the multiplier, the greater is the relative impact of that industry on the economy of the state, and conversely the closer the multiplier value is to 1, the smaller is the relative effect. In California, the processed food industry with a multiplier value of 3.32 and the primary metals and foundries industry with 2.21 have the greatest relative impact. In Utah, the nonferrous metals industry with a multiplier value of 8.11 is by far the most important to the economic health of the state. These multiplier values have a simple interpretation. In California, if output in the processed foods industry drops in such a way that direct income payments are cut by 1 million dollars, the effects of the cut in production would result in California income dropping by 3.32 millions in total. The indirect effects account for 2.32 million dollars of the total.

We must beware, however, of confusing the relative impacts, which these multipliers measure, with the absolute impacts on income of any given industry. In some industries wage and other income payments are a larger percentage of total cost than in others; so that output need change smaller amount to account for a $1.00 change in direct income payments. Furthermore, we have not said anything about the sensitivity of output to change (i.e., the frequency of change) nor the expected

[1] In his study of the Pacific Northwest, Paul Simpson derived a formula for a static regional multiplier which leads to smaller values than for the nation. The formula is $m = \frac{1 + q_o}{q_s + q_o}$ where m = regional multiplier; q_s = marginal propensity to save; q_o = ratio of imports of consumption goods to home produced consumption goods. Cf. *Regional Aspects of Business Cycles and Special Studies of the Pacific Northwest*, a study prepared for the Bonneville Administration, June, 1953.

amplitudes of change in unstable conditions. Both the frequency and the amplitude of change in output in the cycle are important measurements to consider when assessing the absolute impacts of a change in production in a given industry on the state's income.

It will be noticed that Table 1 is labeled "simple" income multipliers and for good reason. There the initial changes in final demand were translated into the direct-plus-indirect effects on income via changes in industry output. No other relationships were admitted to the calculation. But as incomes change, consumer expenditures on goods and services will ordinarily change also. Since consumer expenditures are one sector of final demand, the changes in income described above will lead to changes in final demand (i.e., consumer expenditures), and that in turn will set off a second round of readjustment of output, of income, and back again to final demand for the third time and so on to fourth, fifth, and later rounds. This circular flow of adjustments in final demand, output, income and back again can be carried through directly via an iterative procedure in the manner just described or the consumption-income relationship can be incorporated in the structure of the input-output model by augmenting the original inverse matrix by the household row and column. If this is done, the coefficients in the household row of the augmented inverse show the total effects on income (that is, the direct, indirect, and induced effects) of a $1.00 change in final demand for each of the industries in the model. What has been added by this procedure are the effects on output and income induced via the consumption-income relationship. This relationship may be linear and homogeneous or may be of the more usual form, linear and nonhomogeneous: that is, of the general linear form $a + bx$; b, as the marginal propensity to consume, can be used in the household column of coefficients with which to augment the original inverse and the values of a remain in final demand as a stipulated amount of consumption expenditure. Income multipliers of this sort have been computed for Utah alone.[2] As might be expected, the effect of the introduction of the consumption-income relationship is to raise all the multipliers.

We might now relax the conditions of the problem to admit one more type of relationship: the effects upon investment induced by the changes in income and consumption. This calls for a calculation of accelerator coefficients for the region, but it also does more than that. We should have to predict, for given time profiles of production associated with given bills of goods, the division between the additions to demand satis-

[2] Cf. forthcoming articles on "Regional Analysis: An Interindustry Model of Utah," *op. cit.* In that paper employment multipliers are derived as a supplement to the income multipliers. They are of interest in themselves, but the technical problems of their derivation are similar to those for income multipliers so that it was felt that they could be omitted from discussion here.

fied from imports from those satisfied by new capacity building in the region. Now we have come back full circle to one of the problems mentioned briefly in the first section of the paper. To get first approximations to accelerator coefficients is easy. The numerator of the formula for the calculation of the income multipliers can be used, but with the capital-output ratios or coefficients substituted for the household coefficients. For industry k the accelerator coefficient would be estimated by

$$R_k = b_1 d_{1k} + b_2 d_{2k} + \ldots + b_n d_{nk} = \Sigma_j b_j d_{jk}$$

where R_k = accelerator coefficient (per \$1.00 change in final demand for industry k); the b's are the capital-output ratios and the d_{ij}, as before, are the coefficients in the inverse matrix.

These coefficients can only be interpreted as measuring the amount of capital directly and indirectly utilized per \$1.00 of final demand for the given industry.[3] It is possible to argue that these are only average accelerators and that the marginal figures would be quite different. But over and above the difficulty of agreeing on some numerical values, there is the greater difficulty of meshing the various economic events: the initiating change in some sector of final demand; the changes in output, income, and consumption resulting therefrom; the changes in investment induced as a result and the multiplying effects on income flowing from it; and so on and on. An extremely meticulous analysis of each of these phenomena might make some progress laboriously toward a solution, but since time lags and the necessity of choosing the activities to be performed are met at each point, a model structure to handle these problems and to program them efficiently seems essential.

III

The input-output models described above are static, although by a careful dating of each event an analysis of the whole system may be made which does resemble the dynamic situation. Tracing through the effects of an initial given change in final demand, as was described in the derivation of the multipliers, is a step in the direction of dynamic analysis; but it may be possible to include most of these economic events in a model which will determine whether given schedules of final demand over time are feasible and what the efficient programs are. The model structure to be developed will be used to predict growth in the

[3] Accelerator coefficients were computed for the California model. On the assumption that they may have some curiosity value, the following numbers are presented: Agriculture 2.48; mining .68; petroleum 2.47; processed foods 1.63; textiles .72; lumber, etc. 1.11; other manufacturing 1.22; primary metals 1.85; fabricated metals, 1.31; nonelectrical machinery 1.59; electrical machinery 1.09; transportation equipment 1.34; utilities 2.62; trade 1.25; services 1.20.

California economy for 1954-60, although the model may be used for analysis of many problems other than growth.[4]

Let us start with some simple social accounts and ask how current output is divided among these accounts. We shall denote the output of industry i in the time period t, X_{it}, $i = 1,2, \ldots n$; $t = 1,2, \ldots T$. A part of this output will go to final demand which consists of the output taken by private consumers, by federal, state, and local governments, by exports, and by autonomous investment. As we shall see, investment is divided between that which is determined by exogeneous factors (autonomous investment) and that which the model plans itself in order to meet a desired build-up in final demand (i.e., induced investment). The drain on output arising from the former is included in final demand, and, using the same symbols for i and t as before, final demand is denoted f_{it}.

Output of industry i is also required by other industries in the model, first, in order to produce their own current output and, second, in order to build new capacity (i.e., induced investment planned in the model). Demands on industry i on current account by industries $j = 1,2 \ldots n$ are given (per unit of output) by the standard flow coefficients, a_{ij}, and those on capital account by the capital coefficients, b_{ij}. If all industries are considered together, these two requirements are denoted by the matrices, $A = [a_{ij}]$; $B = [b_{ij}]$. Induced investment, to be determined in the model, is denoted m_{it}. Our last account represents that part of output which is not used up in the current period but which is stored for use later and is denoted, s_{it}.

Now let us account for the capacity, c_{it}, which exists in a given industry at any time t_k. It is simply the initial capacity in the industry plus any capacity built in the periods $t = 1,2 \ldots k\text{-}1$ (we shall assume that autonomous investment is zero in this model); this capacity is distributed between the production of current output and that part which lies idle or unused, u_{it}. Thus at t_k initial capacity is equal to output in t_k plus unused capacity in t_k minus the additions to capacity up through t_{k-1}.

The accounts for all industries in the model can now be summarized in two sets of equations. The first subscripts denoting the specific industry will be suppressed and x_t, f_t, m_t, s_t, c_t, and u_t will designate column vectors; together with the flow and capital coefficient matrices

[4] I am indebted to several people for their help in developing this model: to James Petersen, RAND Corporation, with whom I worked, and am continuing to work, on the California flow input-output model; to Harvey Wagner, formerly of the RAND Corporation, with whom I discussed at length models of this type and who developed a short-cut which halved the number of equations in the computation (his technique has great promise for other linear programing models); to William Orchard-Hays, RAND Corporation, who carried out the calculation on the IBM 701.

A and B they constitute the materials for setting down the accounts in the form of difference equations.

$$(1)\quad X_t = f_t + AX_t + Bm_t + S_t - S_{t-1}$$

$$(2)\quad C_t = X_t + u_t - \sum_{t=1}^{t-1} m_t$$

The first set of equations simply accounts for current output, allowing for the storage of output and for the use of output produced in the preceding period. The last two terms thus show the net change in inventories of finished products. The second set of equations relates initial capacity to its usage in current production, that which is unused, and the programed new capacity.

By using the equations above but casting them into a convenient

STRUCTURE OF LINEAR PROGRAMING FORM FOR THE CALIFORNIA GROWTH MODEL, 1954–60

Activities (Over Time)

	X_1	m_1	s_1	u_1	X_2	m_2	s_2	u_2	⋯	X_T	m_T	s_T	u_T
f_1	(I−A)	−B	−I										
c_1	I			I									
f_2					(I−A)	−B	−I						
c_1		−I			I			I					
.		.				.							
.		.				.							
.		.				.							
f_T									.	(I−A)	−B	−I	
c_1		−I				−I			.	I			I

FIGURE 1

linear programing formulation and writing out the form for a series of time periods, the model appears as in Figure 1. Some rearrangement of the terms in the first equation has also been made. The column heading (e.g., x_1, m_1, etc.) denote the activities occurring over time; each of the headings x_1, m_1, x_2, etc., denotes a column vector which is to be premultiplied by the matrices appearing in its own column. If that is done, the equations above are reconstructed. The equality sign appears just after the column vectors of final demands and initial capacities.

From Figure 1 certain potential properties of the model become evident. As it is written there are no production lag times for current output. Output is produced and is available within a single period. The use of $(I\text{-}A)$ in each period shows this to be the case, and for this particular model for which one year is the unit of time it is quite reasonable

to suppose that no lag times are greater than one year. But assume that each period were fifteen days long. Then it would be essential to specify the lag (or lead) times on each input coefficient as well as to date the maturing of output. This can be done easily (if the information is available) by placing the a_{ij}'s with the appropriate leads in the various time periods. To take one example, suppose that of input a_{12}, 40 per cent is required in the first period, 30 per cent in the second, and 30 per cent in the third, and that if that is done, one unit of X_2 matures in period four. Then one column vector under X would contain $.4a_{12}$ in $t = 1$, $.3a_{12}$ in $t = 2$ and so on, with one unit of X_2 in $t = 4$. This splitting of the time period would also affect the storage activities, s_i, since stock piling could occur and be available only after the output had matured.

In this model there is a one-period lag on the building of new capacity, however. The capacity which is ready to produce in t_k must be built (i.e., the inputs must be available) in t_{k-1}. Again it would be possible to have varying lead times on capital and on the availability of capacity. If partial production can be obtained before the new capacity is completely finished, the identity matrices I under m can be modified to show the availability of this partial production. They can also be modified to show the depreciation of capital. In this model capacity built in t_1 is available without depreciation in all subsequent periods, but if depreciation of capital of, say, 10 per cent per year is to be shown the identity matrices I under m would have values of $.9I$ in the third time period and successively lower values thereafter; thus reflecting the depreciation of capacity.

As a final point it should be noted that the flow and capital coefficients, A and B, can be changed in any period so as better to represent costs or production methods through time. The production functions can be modified to show shifts among suppliers, changes in techniques, or increases in efficiency. The capital coefficients can be adjusted to show the marginal costs of adding to capacity in a given industry. However, these changes must be foreseen in setting up the original model since they affect the whole structure.

The California model has sixteen industries, two sets of equations (one referring to current output and inputs; one to capacity utilization), and there are four kinds of activities; hence the original form of the model contained $2nt = 192$ rows, and $4nt = 384$ activities. A computation short cut reduced this to 96 rows and 288 activities, which is the form in which the model is computed. The model is to test the feasibility of certain rates of growth in final demand over the six years. Final demand in this case means consumer demand and federal, state, and local government purchases of final products. Autonomous invest-

ment was assumed to be zero, and imports and exports were specifically excluded for reasons discussed later.

Estimates of final demand in 1953 for each of the sixteen industries were made by starting with the 1947 final demand from the California gross flows study and by projecting forward to 1953, taking into account population growth, shifts in the relative positions of industry, etc. Capacities for the industries in 1953 were also estimated and the final demands and capacities for the model are shown in Table 2. In a short paper such as this, it is impossible to show the matrices of flow and

TABLE 2

1953 FINAL DEMAND AND INDUSTRY CAPACITIES FOR THE CALIFORNIA GROWTH MODEL

Industry	Capacity*	Final Demand*
1. Agriculture and fishing	3722	861.8
2. Mining	215	3.4
3. Petroleum and fuels	1945	245.0
4. Processed foods	4612	2248.7
5. Textiles and apparel	805	481.7
6. Lumber, furniture, paper	1422	151.3
7. Other manufacturing	2300	574.1
8. Primary metals	1002	1.8
9. Fabricated metal products	1708	80.9
10. Nonelectrical machinery	1430	55.8
11. Electrical machinery	1054	76.0
12. Transportation equipment	6559	189.4
13. Utilities	2898	845.7
14. Trade	3884	2664.3
15. Services	8000	6089.9
16. Construction	4000	181.8

* Millions of 1947 dollars.

capital coefficients which were used. Suffice it to say that the flow coefficients were derived from a gross flows table of sixteen industries which itself was a condensation of a fifty-six order table, and the capital coefficients were estimated using the series of input-output studies on capital coefficients as guides. All figures used in the model are in 1947 dollars.

The feasibility of various rates of growth of final demand over the six-year period are being considered in the computation. Seven separate cases in ascending size have been specified, consisting of uniform or mixed percentage increases over 1953. These seven cases are: (*a*) 2 per cent increase in final demand per year for all industries; (*b*) 2 per cent for industries numbered 1, 2, 3, 4, 6, 8, 9, 12 and 5 per cent for industries numbered 5, 7, 10, 11, 13, 14, 15, 16; (*c*) 5 per cent for all industries; (*d*) 2 per cent and 10 per cent for the same respective groups as in *b*; (*e*) 5 per cent and 10 per cent; (*f*) 10 per cent;

(*g*) 15 per cent. The differential rates of growth for the two groups of industries require at least a passing explanation. A summary appraisal of the industries, gained in part during the course of the projecting final demands from 1947 to 1953, seemed to indicate rapid growth in demand for some industries such as trade, services, and construction, since, with the growth of population in California, these industries which minister most directly to consumers may be expected to experience extensions of their markets at least equal to the rate of population growth and perhaps much more. Other industries which depend on export markets or which are tied to a natural resource might be expected to grow more slowly; that is, the percentage of final demand satisfied by the industry in the state as compared to the percentage from imports might be expected to decline. In this rather labored way, a very rough justice had to be done to a number of factors. The possible combinations of rates of growth between 0 per cent and, say, 25 per cent for sixteen industries is infinite, and some reasonable combinations of growth rates had to be selected. A more painstaking analysis might demonstrate that the rates selected are nonsense, though I believe on first blush that they are at least within the acceptable range.

The model was first asked to produce in all periods a feasible program for 1953, the initial year. By definition such a program exists if the coefficients, final demands, and capacities are accurate. Then the rates of growth corresponding to the first case described above were introduced in the final demand positions of the equations and tested for feasibility. The successively higher rates of growth will be tested until infeasibility occurs for some rate. Then the optimum program for the highest feasible rate will indicate the growth pattern for the state. At the time of writing, only the 2 per cent case had been computed. The remaining cases for the higher rates of growth are still to be tested. For each industry in the model an increase of final demand of 2 per cent per year has been found feasible, and the program of outputs has been determined. The ninety-six activities to be performed are not worth while setting down in detailed tabular form since the 2 per cent case is just an intermediate stage in determining the maximum rate of growth of which the California economy is capable over the period 1954-60, but some general summary of the character of the results may be instructive:

1. Given the final demand and capacities of Table 2, the 2 per cent case required new capacity building in only three industries: utilities, trade, and services. All other industries had sufficient capacity.

2. Because of the existence of excess capacity, particularly in the first two years of this period, stock-piling activities were carried on quite

extensively in many industries. The levels of stock piling attained in some industries were surprisingly high, indicating that for that low a rate of growth there is a great deal of slack, probably too much to represent any real situation.

3. Since new capacity building was at a minimum, industries such as fabricated metals and construction, which feed capacity expansions, had a great deal of idle capacity.

4. Output levels over time have different patterns as among the industries in the model: some output levels rise steadily (e.g., agriculture); some remain constant (e.g., primary metals); some fluctuate up and down (e.g., lumber, furniture, paper).

It seems evident that higher rates of growth than 2 per cent will prove feasible, since excess capacity exists in most industries. Subsequent calculations will determine how high a rate may be obtained. However, the low level of output of some industries such as transportation equipment is significant by itself because of what it implies about our definition of final demand. Only consumers' demand and the demands of government are included, whereas in the input-output study of California a large portion of the output of this industry was represented by "export" demand. Had we included exports in our final demand vector, this idle capacity probably would not have occurred and the programs of output would have been quite different. Our higher rates of growth will reduce this idle capacity, but the situation is really one of having a set of requirements which does not "drive" the model in the way that seems most realistic. We shall have to redefine our final demand in some way to incorporate the important category of exports in our regional growth models.

There is another important variable whose exclusion biases or limits the applicability of the results, and that is imports. The California model really asks what rate of growth is feasible with the resources (capital) immediately at its command; but the import of certain kinds of products such as steel probably will always be the largest part of total demand in the state. The cost comparisons between domestic products and imports, which are so important for the individual industry decisions to invest in new plant and equipment, are missing from the model. If there is a need for new steel capacity and the resources are available, the model will program such building. Now imports might have been included in the model in several ways; by adding them without a constraint on their amount, any rate of growth is feasible. But ask yourself: what sets the limit to imports into a state? It is most perplexing to try to set down the factors limiting imports. Is it limited by public and private savings plus the ability to create new debts, and if so, what

limits the latter? It is obvious that we need some careful analyses of regional money flows, of regional demand patterns, of comparative costs of producing different products, etc.

Imports might have been included as an additional activity in the model and then either an absolute limit put on the total, thus allowing imports to occur freely up to that point, or by the choice of an appropriate price for imports the model might have been computed so as to minimize the cost of imports. Once the model is computed, the shadow prices which automatically emerge can be used to determine whether any new activitiy will be profitable or not. Explorations along these lines will undoubtedly prove fruitful for more sophisticated and realistic regional analyses.

A second factor about whose impact we know very little is the number of periods over which the model is run and the period(s) at which peak demand occurs. If the model had been run over, say, twenty periods with different time profiles of final demand and with peaks for some industries occurring in staccato during the planning period, the programing of capital building and current output might well exhibit great differences from the program for the planning horizon of five periods, and if, at the same time, each period were set equal to a quarter rather than a year we might well believe that each group of four periods (a year) would show marked differences from this particular model. It will be necessary to run a number of calculations on one model, varying some of the parameters each time, in order to determine the effects of these and other factors. Plans have been made by the author and others to run a small (perhaps ten-industry) national model a number of times, varying some of the parameters each time.

Some of the problems which appear to be quite difficult for a regional model would be less so for a national model, and I believe that the model is much better suited to analyze growth or fluctuations in the national economy. I suspect that the limitations imposed by labor availability, imports, etc., can be handled in more realistic fashion for the nation than they can for a state. Part of this stems from the better data and more thorough background analyses made of data on the nation, and part is attributable to the fact that whereas the commerce between states is subject to very few restraints, the commerce between nations is frequently such that it is not too hard to guess how the choices are going to be made.

DISCUSSION

PHILIP NEFF: These two papers provide a number of exceedingly interesting contrasts, due principally to a basic difference between the approaches adopted by the authors. Mr. Krutilla sees the shortcomings of data as so significant as to preclude the quantitative evaluation of any but direct effects of development programs, preferring, apparently, informal appraisals of all secondary or "triggered" development. Mr. Moore, while undoubtedly well aware of the shortcomings of his basic data, obviously prefers to use it to the limit, perhaps hoping the statistical requirements of his model will affect (possibly "fix") the operations of agencies responsible for the collection and publication of economic statistics. My own inclination, in spite of serious doubts that a unique value attaches to the input-output approach (a virtue that I fear some of Mr. Moore's "seasoned" input-output analysts claim), is to side with Mr. Moore. A flight from quantitative measures to subjective appraisal is a temptation to any social scientist. The identification of cause, the appropriate weighting of multiple causes, and the evaluation of multiple effects is certainly discouraging, difficult, and sometimes impossible given the analytical tools and the data now available. But this does not justify recourse to methods or approaches that at best can provide tentative and partial answers to important questions.

Mr. Krutilla rejects the evaluation of regional development programs by comparisons of growth rates of aggregate income series among regions on the following grounds:

1. Other governmental programs, e.g., military procurement, affect regions differentially.

2. It is impossible to establish one set of boundaries within which are confined all effects of such programs.

3. Such an evaluation assumes that all regions have equal development potential.

4. Development programs are relatively unimportant compared with all other influences on regional income. Since it is impossible to abstract from these other influences, income comparisons fail to measure the contribution of such programs.

As I see it, these four limitations add up to the fact that results (income changes) cannot easily be traced to their many causes: resource complement, export demand, military procurement, government investment, etc.

Mr. Krutilla offers as a substitute a method for evaluating the comparison of costs and benefits of those programs of a development agency such as flood control, navigation, and power for which the development agency has exclusive responsibility "and from which direct benefits are largely tangible." Somewhat later in the paper he implies that costs and benefits are readily measurable, too. Cost estimating apparently worries Mr. Krutilla not at all. In fact the market value of the services rendered by the agency is usually measured by the cost of the "most economical alternative method of

providing similar services." The development program's success is then apparently indicated by the extent to which the benefit-cost ratio exceeds unity. My doubts that costs and benefits are tangible and measurable are only strengthened by the author's admonition that expected costs and benefits over the life of the facilities should be compared.

Mr. Krutilla's analysis of the factors affecting the impact of a development program on a region, its effectiveness in increasing the supply of resources, and the characteristics of demand for these resources is certainly interesting, and his emphasis on the importance of demand for a region's output is thoroughly justified. But again I think the analysis is oversimplified. Clearly, the discovery and production of petroleum in Arabia has increased that country's resource supply functions, and oil is a product the demand for which is highly income elastic. Few, however, would argue that Arabia has been blessed with substantial development. Secondary industrial development has been almost totally absent. Certainly such matters as income distribution in the underdeveloped area, the propensity to invest locally, and other factors must be taken into account.

Description of industrial development in the area served by the TVA clearly indicates the tremendous significance of secondary or "triggered" developments, but Mr. Krutilla's rejection of aggregate series as tools for program appraisal leaves these developments to informal assessment. It occurs to me that the margin of error left by such a procedure is far wider than it need be and that it can be substantially reduced by careful use of income series.

Mr. Moore is highly successful in demonstrating that the input-output approach provides an interesting and useful skeleton for regional analysis and in doing so performs an extremely worth-while service. Too often, regional studies lack just such a skeleton. Too often, also, these studies suffer from the absence of reliable statistics, and the examination of Mr. Moore's model makes one wonder if perhaps his study does not put too much strain on the data. One cannot tell from the paper, for Mr. Moore merely confronts the reader with results—a regrettable but necessary procedure where the aim is primarily methodological and limits are imposed on length.

Devotees of this technique, I think, too often, because of persuasiveness and perhaps zeal, oversell their product. Unwary folk are trapped, failing to recognize the shortcomings and pitfalls, and look to input-output as the answer to any problem in economic method. Even if the basic data were accurate to the third decimal place, models based on this approach generally assume unchanging input coefficients. And such an assumption is more than hazardous, especially where many industries have excess capacity and when projections are made more than a very short distance into the future. Mr. Moore, in my judgment, shares this characteristic or failing with many "seasoned" input-outputers. A little more "seasoning" would perhaps not be amiss and might protect unwary regional researchers.

LEON N. MOSES: At the suggestion of this session's chairman, my comments will be restricted to Mr. Moore's paper. I shall attempt to evaluate what I conceive to be the author's two main contributions: his use of static input-output

procedures to develop a series of regional income multipliers for Utah and California; his dynamic input-output model for the state of California.

Mr. Moore's multipliers are supposed to provide an analytical tool by which "to measure quantitatively the effects on specific regional economies of given types of changes." Our evaluation of this tool will proceed in two stages. First, granted the assumptions, are Mr. Moore's multipliers based on a logically consistent model? Second, are the assumptions empirically justifiable? We may state at the outset that the author's static regional model—from which his multipliers are derived—falls short of adequacy on both counts.

There is very little discussion in Mr. Moore's present paper of how his regional income multipliers were obtained. In order to evaluate them properly, reference will be made to the author's Utah Study ("Regional Analysis: An Interindustry Model of Utah," November, 1953) which describes in detail the methodology behind these multipliers.

Mr. Moore's first step was the derivation of a gross transactions table for the state of Utah. This table shows the total purchases by each Utah industry of each good from all regions. It was obtained by multiplying known base period outputs by technical coefficients. However, the author's goal was an intraregional flow table; i.e., a table which showed the purchases by Utah industries of Utah output alone. This required that interregional and foreign trade be eliminated from the main body of the gross transactions table. To accomplish this the author first estimated Utah's final demand (household consumption, private investment, etc.) for each good. Utah's total consumption of each good was then obtained as the sum of intermediate or industrial demand and final demand. At this point, Mr. Moore assumed that his region would resort to imports only to satisfy requirements in excess of domestic capacity and that it would not export any good until domestic requirements were fully satisfied. This permitted him to "determine" imports and exports by subtracting total Utah consumption of each good from domestic production.

The gross transactions table could then be readily transformed into a table of intraregional flows. For industries in which Utah had a surplus, the amount of this surplus was entered in final demand as exports. In industries where Utah had a deficit, i.e., insufficient capacity to satisfy domestic requirements, the consumption by each Utah industry of these goods was reduced proportionately. The amount of the deficit was removed from the body of the gross transactions table and entered into the final demand sector as imports.

From the intraregional flow table thus obtained Mr. Moore derived a new coefficient matrix which we shall refer to as the regional input-output coefficient matrix. Each of its elements has a regional as well as a technical dimension. The coefficients in this matrix indicate the dollar volume which each Utah industry purchases from other Utah industries per unit of its own output. Our author computed the Leontief inverse of this matrix and used it to determine the impact on Utah's industries of changes in the final demand for Utah's goods. After determining the latter, the effect on Utah's income was calculated by multiplying each change in output by the corresponding household input coefficient and summing.

The appropriate question to ask at this point is whether Mr. Moore's re-

gional input-output coefficient matrix can logically be used to determine the impact of an increase in final demand? (We shall not deal explicity with decreases in final demand because the argument is quite similar.) The answer is that it cannot, unless his region has excess capacity in every line of activity. If even one sector has insufficient capacity in the base period, so that the region must resort to imports, then use of the regional coefficient matrix to compute the impact on Utah of a change in final demand involves a logical inconsistency. The reason for this is that the model will predict increases in the output of industries which are already operating at capacity. These increases are, of course, incorrect and—because of the interdependencies—the predicted increases in the outputs of all other industries must also be incorrect, as must the multipliers based on them.

Our discussion so far can perhaps be clarified by adopting a somewhat different approach. The major difficulty with Mr. Moore's static model is that the row elements of his intraregional coefficient matrix pertaining to industries of insufficient capacity are variables rather than structural constants. Their value is determined by the ratio of domestic capacity to total regional requirements of these goods. Since a change in any element of the coefficient matrix will—special circumstances aside—alter all elements in the inverse matrix, the latter is composed entirely of variables. Thus Mr. Moore's model and the multipliers derived from them do not provide a consistent method of estimating the impact on a region of changes in final demand.

The assumptions concerning imports and exports which underlie the intraregional flow and coefficient matrices are questionable. Given the degree of commodity aggregation used and the fact that Mr. Moore is dealing with a space rather than a point economy, it hardly seems reasonable to limit interregional trade to the disposal of net surpluses and the satisfaction of net deficits. Existing data on interregional commodity flows do not bear out Mr. Moore's assumptions. The above is only one aspect of the author's failure to give full recognition to the problems of regional interdependence. Another is his neglect of the "feed-back" effects; i.e., the impact which a region has on itself via its effect on other regions. Also, Mr. Moore has no way of determining what the impact on his region will be of changes in the final demands of other regions. He requires this as given data.

Lastly, the author himself comments on the fact that his multipliers may appear somewhat low. This is interesting, since they were computed in a novel way which exaggerates their size. Instead of forming a ratio between the ultimate increase in income and the increase in expenditure, as is usually done, Mr. Moore takes the ratio of the ultimate increase in income to the immediate increase. The latter is derived by multiplying the vector of changes in final demand by the vector of household input coefficients. The divisor of the latter ratio is smaller than that of the former, and the multipliers computed on this basis are therefore larger.

We have so far dealt only with the portion of Mr. Moore's paper devoted to static analysis. It is in the section of his paper devoted to dynamic questions that the author makes a significant contribution. The idea of combining dynamic input-output techniques with those of linear programing for determining

a maximum growth rate is both novel and interesting. However, in certain matters of detail the author could make improvements which would render his results more relevant and significant.

For example, Mr. Moore recognizes that a region can import a good rather than expand domestic capacity for its production and that it can reduce its need for additional capacity by cutting exports. Confronted with the difficult problem of specifying some optimum level of imports and exports, our author decided to assume them away entirely. By this device he destroyed the relevance of his work, since his problem became one of determining California's maximum growth rate under conditions of complete autarchy. This is unrealistic in the extreme when applied to an open economy. Moreover, if the region lacks capacity in any industry, then only a zero rate of growth is possible.

Mr. Moore has so far concentrated all of his attention on the limits to expansion imposed by existing plant and equipment. It would seem advisable to take into account explicity the limitations imposed by the nonreproducible factors—labor and natural resources—as well.

FRANK A. HANNA: So long as there are government programs which are either designed to influence the factors affecting regional output or which seek public support on the grounds that they have influenced these factors favorably, the problem of evaluating regional development programs is likely to be with us. Mr. Krutilla has done excellent service, first, in uncovering many of the pitfalls in the noncritical use of "regional comparisons of income relatives" and, second, in showing that specific development programs often may be evaluated more precisely in terms of their effects on a limited range of closely related activities than in terms of their effects on regional income, which presumably reflects all economic activity within a region.

With the rise in income in the southeastern states during the past two decades, it must have been very tempting to the supporters of TVA, to the state development commissions, and perhaps to other government agencies to point to this income rise as a sufficient justification of their programs. But knowing that income in an area has increased during a given period is not enough. We need also to know that it has increased more than would be indicated by some set of expectations that were derived from factors other than the "development programs." Typically, such a set of expectations is provided by the changes in national income. The income materials used are time series. Like most annual time series they are a mixture of secular trend, cyclical changes, and irregular or nonrepetitive changes. Somehow, from this mixture we have to extract secular trend. As both papers point out, it is only in terms of trend that changes in the underlying factors affecting regional output can be gauged. With present techniques, regional trends can only be approximated. At best, some of the irregular factors will remain, though in annual data these are likely to be small. Larger errors can arise from the imperfect exclusion of cyclical factors.

During the past two decades, the upward trend has been delayed only by the minor recession in 1937-38, the reconversion efforts of 1945-46, and

another minor recession in 1949-50. Nevertheless, cyclical forces have been present continuously. Since some industries are more sensitive to cyclical changes than others, the changes in a state's or area's income level for a given change in the national income may be expected to vary in accordance with the area's industrial composition. Rough estimates presented elsewhere (*Review of Economics and Statistics*, August, 1954) indicate that when the effects of national income changes and industrial composition are taken into account, each of the southeastern states, except Mississippi, has experienced an upward trend or favorable structural change, 1929-51. Krutilla's suggestion that individual industries be analyzed separately is also effective in eliminating cyclical influences if we can accept the very reasonable assumption that the cyclical forces associated with an industry on a national basis operate wherever the industry is found. However, when it is necessary to combine the trends for several industries, the question of appropriate weights becomes important. Use of the levels at either the beginning or the end of the period as area weights ignores differential growth of the industries. Moreover, the absolute levels of an industry in an area are the product of cyclical as well as secular forces. An industry's income, as a percentage of income originating, varies over the cycle as well as secularly. For the nation, this can be handled by averaging. But for a subarea, the averaging of relatives tends to confound cyclical and secular forces. To the extent that Mr. Moore's analysis is based on values rather than physical quantities, it suffers from the fact that the relationships he uses are based on one point in a cycle, 1947, and we might expect value relationships to vary over a cycle.

Mr. Krutilla takes a very pessimistic attitude toward the estimated error attaching to income estimates. No one knows what the errors in national income are. Consequently, they cannot assert that it is impossible for the regional distribution of errors to be as outlined in his extreme example. On the basis of what is known about the compilation of basic data, however, many would expect a more charitable distribution. The Morgenstern estimate of 10 per cent error, which he cites, is based on Kuznets' estimates in *National Income and Its Composition, 1919-38*. A careful reading of the original source shows that this is an estimate of the maximum error probable and that it applies only to the level of national income. The Commerce Department, in its *National Income, 1954 Edition* (page 66), recently stated that the levels "are subject to only a small percentage of error"—one gathers in the neighborhood of 1 to 2 per cent. There is room for larger errors in state income levels. But the error in the level tells us little about the error in the changes in national income or changes in regional income relatives. Would we not expect the errors of omission or commission in compiling the basic data used by income estimators to be of about the same magnitude each year? And would not the same errors be made in allocating income among regions? Even if the error component—the difference between the "true" and the estimated figures—behaved quite differently from the estimated figures, the error in the year-to-year changes from this source likely would be negligible.

I should now like to question whether the introduction of the "terms-of-trade" concept adds anything important to what is already provided by in-

dustry classification. Does a Midwest shoe factory have different terms of trade than a New England shoe factory? It is possible that it may have. It may, for example, be one of the sources of intraindustry dispersion in such measures as average production-worker earnings or value-added per employee. Certainly, industry could be expected to give rise to within terms-of-trade dispersion. Industrial composition seems to be highly correlated with income at the state level. If regional terms of trade are reflected by regional industrial composition, Occam's Razor would seem to indicate that we use existing industrial data rather than spending our efforts trying to create data suitable for terms-of-trade analyses. Agriculture may be an exception; primarily because little effort is made to classify farms even by the seven inadequate three-digit and four-digit categories provided in the *Standard Industrial Classification Manual*. Even in this important major group the provision of adequate categories and intensified efforts to classify farms in greater detail appear more promising than trying to analyze regional farm commodity production for the implied terms of trade.

Perhaps it is no accident that both Krutilla and Moore find their more promising techniques nationally oriented. Within a nation effectively without internal trade barriers, with a common monetary system, and with essentially similar legal and economic institutions, one might expect the income of subareas simply to reflect the industrial or occupational composition of the subarea. Indeed, on the basis of 1950 census data on occupations and on 1949 earnings, I have found that occupational composition accounts for about 80 per cent of the interstate differences in average annual earnings. With or without explicit development programs, subarea industrial compositions are undergoing constant adjustments to new economic conditions, either by selective contraction during periods of recession or by the development of new capacity during periods of full employment and national growth. Whether, at any particular moment, it is better to locate new capacity in subareas where resources are underemployed, as evidenced by low incomes, or to expect underemployed resources to seek higher incomes through migration will certainly vary by industry. By proposing to judge development programs which seek to affect this type of decision solely on the basis of the prospects and requirements of the relevant industries, Mr. Krutilla may have dampened some enthusiasm for regional programing, but he has also brought the problem into better focus.

URBANIZATION AND INDUSTRIALIZATION OF THE LABOR FORCE IN A DEVELOPING ECONOMY

LABOR ATTITUDES TOWARD INDUSTRIALIZATION IN UNDERDEVELOPED COUNTRIES

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In the literature on economic development a certain notable division between optimists and pessimists is apparent. This division is highly predictive of disciplinary origin. Economists—representatives of the "dismal science"—are customarily optimistic about economic growth and its social consequences. Anthropologists and sociologists are customarily pessimistic about the possibilities of economic growth and particularly about its consequences. Although possibly these fields attract different personality types—the manic and the depressed—it seems more likely that conventional theory and habits of thought explain the differences.

I. *Some Contrasting Theory*

Theory of Motives and Consequences. Traditional economic theory has not been strongly concerned with attitudes and motives. Human attitudes and motives are simply assumed to be appropriate to rational choice of means effective for maximizing satisfactions as economically defined and measured. Human wants are thought to be essentially unlimited and human nature pretty much the same wherever encountered. A monetary market system co-ordinates the factors of production and allocates rewards, and financial incentives are assumed to be effective in allocating labor. The movement of labor between sectors of the economy is regarded as a function of its differential marginal productivity, which is principally determined by relative states of capitalization, technique, and, possibly, enterprise.

Given these assumptions, such hints of pessimism that economists have about economic growth in undeveloped areas could be expected to center on questions of capital, market organization, and entrepreneurial activity, but not on the willingness of potential workers to work. And since the economist has a normal prejudice toward the view that improvements in levels of production and consumption are consistent with universal human aspirations, he is certainly inclined to optimism about the consequences of economic growth.

Anthropological and sociological theory stands in sharp contrast to this set of views. Attitudes and motives are of more central concern as variables, related to differential positions within any social system. The person is viewed as having been socialized in ways appropriate to differential social roles, to hold values appropriate to group activity and its survival, and to behave in ways that are "irrational" and "non-rational" as judged on economic grounds. Wants are thus viewed as limited and relative to social position. The social system is viewed as strongly resistent to change, in part because of secondary and tertiary consequences of changes in a complex functional network. If these considerations are important within societies, they are even more marked between them. To contrast with the economist's "similarity of human nature" assumption, the anthropologist or sociologist offers "cultural relativity." He is likely to emphasize differences in values and aspirations in space and time, and to be extremely reluctant to assume that Western experience can be used as a guide to behavior in Dahomey, Nepal, or even Peru.

Given these assumptions, such hints of optimism that anthropologists or sociologists have about economic growth in undeveloped areas could be expected to center on the adaptability of economic incentives and organization to traditional attitudes and patterns, and especially on the willingness of potential workers to work. And since the anthropologist-sociologist has a normal prejudice toward the view that bread or all purchasable goods and services are not enough to satisfy human values, he is certainly inclined to pessimism about the consequences of economic change.

These contrasts are, of course, moderately unfair to both professional groups. Many economists, and especially those who have studied labor mobility, the operation of labor markets, and worker behavior, have expressed strong misgivings about the assumptions of traditional theory. Many anthropologists and sociologists, and especially those who have studied the actual impact of modern economic forms in undeveloped areas, have noted the stresses and tensions in traditional systems and the considerable success of the new employment alternatives in recruiting labor.

Theory of Organized Co-operation. The literature on economic development has included very little material on another aspect of theory, on which our two professional groups tend to disagree. There are differing views on the nature of business and industrial organization as related to the attitudes or motives of participants.

Traditional economic theory has little to say on this subject, since labor is viewed as allocated by market mechanisms and the relation between employer and employee the same in principle as any other

economic contract. The productive efficiency of the division of labor has received consistent attention, but the problems associated with the authoritative co-ordination of labor implicit in factory organization remain relatively unexplored. Only in Marxist economic theory and, more recently, in attempts to construct alternative theories of the labor movement and management-union relations have variables such as power and group loyalties been considered.

Traditional anthropological and sociological theory also has little to say on business or industrial organization, partly because of a concentration on the structure of society generally, partly because economic organization was thought to be the proper province of economists. Only in Weberian sociological theory and, more recently, in attempts to treat "the factory as a social system" have variables such as the authority of office, communication systems, and "informal organization" been considered.

Despite the recent and still somewhat peripheral attention given industrial organization in the disciplines we have been comparing, some differences between the disciplines can be detected. There is a notable tendency for the economist to emphasize—and exaggerate—individual, competitive aspirations, and an equally notable tendency for the anthropologist or sociologist to emphasize—and exaggerate—group, co-operative aspirations. If the economist's model of behavior tends to be the prize fight, the sociologist's model tends to be the quilting bee.

II. *Some Empirical Evidence*

Faced with such contrasting views of economic behavior and its motivational sources, the analyst may be forgiven some confusion and permitted to seek refuge in the data. Unfortunately, evidence has not been collected or analyzed in ways that permit a clear resolution of theoretical disputes. It is possible, however, to get some leverage on the issues by reference to the reported experience in the recruitment and utilization of labor in undeveloped areas.[1]

Apathy and Opposition. A considerable body of evidence runs contrary to the notions that human wants are unlimited, that financial incentives will transfer labor from nonindustrial to industrial pursuits, and thus that the potential worker may be viewed as welcoming release from traditional restraints. Some of this evidence can be interpreted in terms of "rational conduct," but not in terms of economic maximization. The potential worker in undeveloped areas is typically required to give up traditional forms of organization and reciprocal obligations

[1] This section represents a selective summary of a rather extensive discussion, with citations to the literature, in Wilbert E. Moore, *Industrialization and Labor* (Cornell University Press, 1951).

that have combined to afford him security—both material and affective. The kinship system in any nonindustrial society is likely to provide a major barrier to individual mobility, because it is a social security system, because it is the focus of positive values and advantages, and because extended kin obligations are likely to reduce the effective appeal of individual rewards. With an extended kinship system, if the individual faces adversity, his kinsmen are obliged to come to his aid. If he prospers, he is obliged to share his good fortune with great uncles and second cousins once removed. Industrialization breaks up such units, by geographical separation and more importantly by social separation.

The other side of the coin (an inappropriate metaphor in its connotations) is the lack of appreciation of the new status system. This may take the form of a relatively low and highly particular appeal of wages, often commented on with reference to "native" laborers, but also commented on by preclassical writers in economic theory with reference to workers in the early stages of the Industrial Revolution. The principle bears extension, however. New occupations simply do not fit traditional standards of prestige, or are valued negatively because they involve manual labor and merit placement irrespective of age, kinship position, caste, or other forms of "ascribed" status. To the extent that the potential worker operates as an independent producer, the change to wage labor involves some loss of "freedom," even if it offers higher rewards. To the extent that the potential worker operates as the equivalent of a craftsman, the change to factory employment is likely to involve a loss of socially recognized skills, of "workmanship," in the division of labor and its subordination to machine processes.

It would be hard to deny that a considerable part of this apathy and opposition is "reasonable," but equally hard to deny that its explanation lies partly in social standards and values that are badly served by wage incentives and by industrial forms of labor allocation.

The Force of Circumstances. If wages have a limited appeal to nonindustrial populations and new employments are negatively valued, it is not surprising that workers are more commonly "pushed" than "pulled" into modern forms of economic activity. Much of the "push" is in fact the largely unintentional consequence of external intervention. The successful attempt to reduce mortality has the effect of deteriorating man-land ratios, thus increasing agricultural underemployment and causing the landless and impoverished rural dweller to seek and accept other means of livelihood. The introduction of cheap manufactured goods may well displace the handicraft worker and make available an additional pool of workers, either directly or through increased dependency on agriculture. Even the trader may be displaced by large-

scale distribution, or simply by more efficient organization of established markets.

The coercion of hunger does not exhaust the available pressures. Direct political coercion also has an extensive record, ranging from forced labor to the indirect coercion of taxation—a system which has been widely used as a device in colonial Africa.

Not all of the sources of pressure on the potential worker are so clearly external in origin. Any society exhibits some degree of tension and strain, some evidence of dissatisfaction. The presence of new alternatives may allow the deviant who seeks to evade the sanctions of the traditional order to escape from unwanted and oppressive controls.

Again it would be hard to deny that the attitudes implicit in these types of behavior are reasonable and again equally hard to deny that they do not correspond to notions of high sensitivity to slight differences in incentives and opportunity.

The Efficacy of Positive Inducements. Not all of the empirical evidence involves such negative attitudes toward industrial labor. Wages do have an appeal, particularly if they can be used for goods and services that form part of traditional patterns of consumption and traditional modes of relationship. The transitional forms are many, and often oddly at variance with Western conceptions of market operation. The African from the native reserve who works long enough in the mines to pay his hut tax is presumably not highly integrated into a market system. He becomes slightly more integrated when he uses wages to buy cattle for use as a bride price, but this still has a distinctly exotic flavor. His behavior fits our standard conceptions better when he proves interested in all sorts of commercialized goods and services, but of course by then he is likely to have little in common with the values of the preindustrial community.

In some places other inducements to work seem to have operated, although not necessarily in the absence of financial incentives or contrary to their economically expected effect. Patriotic motives and other collective goals have formed the basis of appeals to workers in most industrial societies, with what effect it is difficult to say precisely. Certainly it is likely that a sense of voluntary participation in a worthwhile common enterprise offers some source of satisfaction to workers, although the standard forms of productive organization are not well adapted to making the use of such appeals very convincing. Here, it may be noted, is a possible positive role for union-type organization. Certainly the communists have attempted to involve workers in undeveloped areas in the organized building of the new as well as the destroying of the old forms of social allegiance and power.

III. *Toward a Tenable Theory*

The discussion to this point implies the rejection of two extreme theoretical positions, both of which constitute alternatives available in the literature. The one extreme may be attributed to economics, although by no means all economists would subscribe. To traditional economists, labor attitudes are simply unproblematical, as they are adequately subsumed under the assumptions of maximizing want satisfactions. The other extreme may be attributed to anthropology, in the same unrepresentative sense. To the anthropologist labor attitudes are so problematical, because they are so conditioned by variable cultures, that no other general statement about them is possible.

The attempt to formulate here the elements of a low-order theory rests upon the assumption that it is possible to give a general characterization to the undeveloped areas, to specify some of the crucial characteristics of modern economic organization, and to indicate some of the labor attitudes which are significant for transition from one to the other.

The Nature of Undeveloped Areas. Although it is true that cultures differ, and in many ways, it is possible at a higher level of generalization to detect common and essential functions in the organization of society. Every society has provisions and rules for reproduction, socialization of the young, production and allocation of goods and services, adjustment to the nonhuman environment, maintenance of order. It is also possible to detect some common bases in cognitive orientations and values. All societies encourage some material aspirations (although not necessarily expanding and competitive), some rational, technical orientation to the use of the environment (although not necessarily innovative), and some positive value on health and length of life.

There are internal sources of tension and strain in all societies. The model of the perfectly integrated society is a useful analytical fiction for many purposes but ought not to be confused with primitive or agrarian societies. The sources of strain include, at least, uncertainties in socialization from generation to generation, chance innovations, and competing role demands given scarcities of time, treasure, and energy (or affective loyalty).

All undeveloped areas have already been disrupted in some form and degree by the "external" influence of Western patterns. The consequences of this interference may or may not be favorable to continuous economic development, but this will depend in some measure on the strategy adopted in view of the situation, as well as on the probable effects of influences so far.

One negative generalization is also negative in its implications for

labor attitudes toward industrialization. Mobility on the basis of individual performance is generally not markedly present in undeveloped areas and does not generally form a part of the positive value system. Whether for the individual or for the system as a whole, continuity and not change is likely to be the major value.

The Nature of Modern Economic Organization. It is now commonly recognized that modern economic enterprise depends upon a complex institutional structure. For purposes of simplification this may be partially summarized as a monetary-market mechanism for allocation of the factors of production and for achieving distribution of goods and services. For the nonhuman factors of production this involves at least transferable property rights and rational cost accounting. For the human factors of production this involves a wage system for putting into the hands of the specialized worker a medium for commanding the necessary and appropriate goods and services for himself and his dependents.

Although part of the division of labor will be co-ordinated by impersonal market mechanisms, the fixed capital and economies of scale in the productive unit characteristic of industrial organization imply also administrative co-ordination and the development of scales of authority.

If labor is to be rationally allocated and optimally utilized, it appears essential to select on the basis of technical competence, to co-ordinate on the basis of relationships specific to the interdependent tasks, and to tie rewards to types of activities. These rewards need not be exclusively financial and indeed rarely will be. All of the rewards imply a system of social ranking closely related to position in the productive system.

The Nature of the Transition. Whether one looks at undeveloped areas or the most highly industrialized ones, a fundamental theoretical point is evident. That point is the great complexity of human motivation. Men will work for as many reasons as there are values to be served by such activity and will refuse to work where that serves his values. The fact that industrial systems emphasize values that are commanded in a market and incentives that provide monetary claims on a market should not blind us to the diversity of ends or the diversity of means for their satisfaction.

The effectiveness of wage incentives is relative to the availability of goods and services in the market that form part of the effective wants of workers. This is essentially a common-sense static principle. But it is quite limiting, and failure to recognize it accounts for some of the exasperation of observers of "irrational" natives. The worker in an undeveloped area is typically not accustomed to expect, or even to aspire to, any considerable raising of ceilings on his consumption and

social position. And there are many wants typically not satisfiable by market mechanisms. So we have both limited demand and limited supply.

To translate this principle into dynamic terms, the available evidence indicates that we should expect an increase in demand through knowledge, education, and the development of new values, and in supply through the addition of goods newly available because of industrialization and the movement of services into the market.

This view of market-oriented attitudes can be broadened, with considerable benefit to its theoretical importance.

The effectiveness of the appeal of new employment alternatives is relative to the availability of need-satisfying rewards. This principle will still apply if material or even financial well-being is held constant. The potential rewards include prestige and esteem within an acceptable system of social valuation. This also is a common-sense static principle and is also limiting. The traditional system of social valuation will not typically include the new activities, and the latter compete or conflict with the former. Both change and choice tend to be devalued. If some relationship between wages and markets has been established, higher wage levels can be used as a principal lever on conservative traditions, and this has been their historic role. The private employer is unlikely to act "correctly" in this matter, and this may be a major area for governmental policy in economic development.

Over time, we should expect an increase in aspiration and the addition of values associated with status mobility, merit evaluation, and a realistic sense of choice and initiative. We should also expect new systems of social organization and stratification to which these aspirations and values are appropriate.

Economic growth, insofar as it is affected by labor attitudes, is likely to be radically retarded or contained by any one of several vicious circles. There is considerable evidence of a reciprocal relationship between low wages and low productivity, whether the connection works through mere physical energy or through more subtle frustration and apathy. The failure to detect and utilize convertible craft skills, the assumption that initially unskilled local labor is incapable of training, and the adoption of the "color bar" as an extreme and open manifestation of these practices constitute waste in the short run and possible failure of continued development in the long run.

It appears evident that neither the available skills nor the appropriate attitudes can be assumed to be adequate among the potential workers in undeveloped areas. Growth seems to have been most rapid and most probably continuous in the future where considerable resources have been devoted to formal education and where education and in-service

training have been most closely geared to the skill demands of an industrial economy. Now, clearly, education in many colonial areas has encouraged anything but the development of mechancial and technical skills, and the opportunities for use of any such skills have not been made available to natives. That the new native leaders tend to be political agitators rather than economic administrators is scarcely surprising in view of colonial political and economic policies.

The development of positive labor attitudes toward industrialization would probably be enhanced by the fostering of a sense of social participation as well as by the expansion of individual opportunity and the provision of amenities and security at least equivalent to those available in traditional organization. A sense of social participation has been consistently neglected even in advanced industrial societies. We do not know how flexible industrial organization may be made. But if economic development entails a revolutionary change in the organization of society, as it does, there is at least some theoretical reason for supposing that workers as well as managers or government officials might welcome positive participation in partial compensation for their uprooting.

In view of the complex richness of human motivation, the idea that economic and noneconomic incentives are necessarily competitive for a limited supply is untenable. Both may be increased simultaneously and continuously within very high limits.

IV. *A Methodological Note*

The reason for our interest in attitudes toward industrialization is clear and simple: we want to predict behavior relevant to the industrial mode of production. In stable situations the best predictor of behavior is previous behavior, and attitudes become "intervening variables" in the strict sense. That is, attitudes are necessary but inferential phenomena required to explain the mechanism of action to answer the motivational "Why?"

To predict behavior in novel situations we are likely to want to attach greater significance to attitudes as prime variables. Attitudes may be defined as those psychological attributes of the individual which determine his tendencies to act in specified ways in specified situations. Such tendencies to act involve both a cognitive "definition of the situation" and an affective "evaluation of the situation." Any behavior we are likely to be interested in necessarily involves affective or evaluative components—at the very least, ends and criteria of choice.

Since attitudes are not directly observable phenomena, but some such variable is theoretically required, we are faced with some nasty methodological problems. We infer attitudes from behavior, including

verbal behavior but not excluding other behavior. It is not necessarily true that verbal statements of attitudes are more reliable predictors than are previous relevant actions, if known. The reliability of attitude measures in forecasting subsequent behavior is rather low. But since representative samples of action are often difficult to observe, considerations of convenience lead many social scientists to rely on interview data. If the behavior to be predicted is itself novel, such as reactions to new modes of employment, we are likely to want several indexes of attitudes in the hope of increasing our probabilities. Given a population with some relevant knowledge and experience, personal or vicarious, it is possible that attitudes can be uncovered by techniques ranging from direct questions to projective tests. At best, we are relying on the human characteristics of generalization, transfer of learning, and transfer of values as ways of indicating probable reactions to new situations.

Asking direct questions is, of course, not the only recourse, but it is one appropriate partial procedure. The alternatives, which underlie most of the allegations in this paper, include extrapolation from "comparable" situations at other times and places, and "pure" functional analysis for locating types and degrees of vested interests, sources of tension and strain, and opportunistic and disgruntled elements in the preindustrial situation. Any prediction from any available procedure is somewhat hazardous, for experience is still the best teacher of social scientists, as it is of men.

THE CITY, THE FACTORY, AND ECONOMIC GROWTH

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I

In a generally accepted view, the economic development of underdeveloped countries is contingent upon the introduction of industry. Industrialization, in turn, is associated with urban growth, and the relationship of these two processes is usually assumed to be so close that some writers speak of industrialization and urbanization as two facets of one and the same process.[1] The evidence for the need for industrialization and urbanization is found in the historical experience of the economically more advanced countries and in general considerations on the economic situation of the farm populations in many underdeveloped countries.

The advanced countries contain the most heavily industrialized and at the same time most densely urbanized regions of the world, and to the extent to which industrial cities have grown up in underdeveloped countries, they have been patterned after British, German, and American models.

The improvement in the economic position of subsistence farmers who make up two-thirds or more of the people in many underdeveloped countries is dependent upon industrialization. There are scarcely any underdeveloped countries in which there is not a large surplus of unemployed agricultural labor. This surplus farm labor represents a tremendous waste of human resources and the obvious remedy is the transfer of these "redundant" persons into industry. But their removal from the farms may not always be sufficient and may have to be implemented by removal from the villages to urban areas.

We may conclude, therefore, that industrialization and urbanization are effective means for economic advancement. But this process is not one of unmixed blessings. The social consequences of the growth of industrial urban centers in underdeveloped countries are well summarized by Motwani, when he describes the cities of South Asia as "huge mausoleums of coal, smoke, iron and steel, of dirt and squalor, of overcrowding, of cooly-lines, and human warehouses. Long hours, low wages, bad housing, woman and child labor, infant mortality, accidents, high rents, poor sanitation, prostitution, gambling, racing, dope dealing,

[1] See for example, Kewal Motwani, "The Impact of Modern Technology on the Social Structure of South Asia," *International Social Science Bulletin*, Winter, 1951.

dance halls, cabarets and night clubs were features of all the cities. The noise corroded the nerves of the city dwellers, while absence of neighborhood restricted their social contacts and made them strangers to each other." *(Op. cit.*, page 787.)

Much of this is true. But many of Motwani's charges are not the consequences of the growth of cities but rather of general conditions of economic backwardness and poverty. His reproach against urbanization and industrialization is too undifferentiated to be fully effective. Infant mortality is much higher in the rural areas of underdeveloped countries than in their cities; the standards of housing are also, on the whole, rather better in cities; and many of the rural laborers work long hours and receive pitifully low incomes for their toil. Normally the small subsistence farmer works harder and gets less than the factory worker or the workers on plantations, mines, and railways.

But although industrialization and urbanization go usually hand in hand, there is no necessary connection between the two processes. Industries can be and have been established in rural districts and cities have grown up without large industrial plants. The introduction of new technology may sometimes change existing social relations very little and sometimes subvert them completely. The social relations created by modern factory organization, implying new hierarchial relations in the work situation, discipline, the dependence of the worker upon a money wage, the uncertainties and insecurities connected with the proletarian style of life, are not peculiar to urban industry. These same conditions hold in plantations and mines which are typically located in rural surroundings, on railways and other means of transportation and communication which stretch far out into the countryside. The almost automatic association of industrialization with the introduction of the factory system as it was practiced in the mid-nineteenth century made us forget that the advantages and disadvantages of technological change in underdeveloped countries may be attributed to different factors and that the relative contribution of each of these factors in the over-all process of economic development and technological change must be more clearly determined in order to provide guiding principles for developmental planning. For if certain "undesirable" outcomes can be associated with one factor alone, it may be possible to plan in such a way as to eliminate or at least mitigate the incidence of the undesirable outcome.

In order to isolate the impact of industrialization from that of urbanization, we will examine each of these processes separately. In the subsequent discussion the contrasts and differences will appear sharper than they are in reality. By describing situations which approximate

ideal types, some of the variables are placed in greater prominence than if an attempt is made to present a fully realistic description with all shades and qualifications. I do not maintain that the sharp contrasts which will appear in the discussion are necessarily present in all the developments described. They are emphasized here because by this method attention is drawn to the most important tendencies accompanying industrialization and urbanization in a developing economy.

II

Let us begin with the analysis of "industrial" cities. Firstly, they are new, not merely in the sense that most of them are the product of urban growth of the last 150 years, but rather that many industrial cities arose in places where earlier there had been no places of settlement at all or only small villages or hamlets. Manchester, which has been called the "first industrial city," was described by Daniel Defoe in 1727, as a "mere village." (Daniel Defoe, *A Plan of the English Commerce* [reprint ed.; Oxford, 1928], page 64.) Also other heavily industrialized areas of Great Britain—the Glasgow region, the Newcastle region, the Rhondda Valley, and parts of the Black country—were wasteland or farmland with few and unimportant settlements up to the middle of the eighteenth century. Extensive evidence for the rural character of the Ruhr Valley at the beginning of the nineteenth century was collected by Pierre Bernaerts. He cites accounts of the time which state that the Westphalian region in the 1830's presented "the character of an agricultural country with a few market towns which also had some handicraft trades. No contemporary document envisages by any means the extraordinary upheaval which, in the course of one generation, was to bring forth in this territory the most extensive industrial region of the European continent. . . . Essen is described as an agreeable small town . . . in which in summer the cows were taken to the communal grazing grounds. . . . Oberhausen, Gelsenkirchen and Ruhrort were tiny dots on the map." (Pierre Benaerts, *Les origines de la grande industrie allemande* [Paris, 1933], pages 101-102.) It is superfluous to point out that the great industrial metropolitan centers in the United States, Canada, and Australia grew in places where not even villages had been before the first settlers decided to build their cities in these spots.

The newness of the industrial city—the absence of a long urban tradition with guilds and market privileges and other vested interests—was doubtless an important factor in their rapid rise. Although urban growth in England was rapid everywhere during the nineteenth century, the new cities grew faster and ultimately reached larger size than the older cities. Manchester outstripped Birmingham and Leeds, the cotton

towns of Lancashire overtook the woolen towns of Yorkshire, the coal towns and the iron towns of the north and west overtook the commercial and small-industry centers of southern and eastern England.[2] But although the new cities did not have deep historical roots, they grew up in a cultural environment in which there existed a long urban tradition. They adopted and modified the old institutions, but there was never any question that each of these rapidly growing entities formed a "corporation," a whole of some sort, that the inhabitants of the city were the members of one and the same community, and that they had rights and privileges in the community and duties towards it. Classical liberalism at its height saw some experiments in municipal "socialism" in such fields as sanitary provisions, street lighting, gas and water works, and other services. Municipalities entered the field of education and culture: they built libraries and schools and recreation grounds and parks. In spite of the intense ego-orientation stimulated by classical liberalism in the economic sphere, the older collectivity-oriented values prevailed even in Cobden's Manchester. In spite of slums and low housing and sanitary standards in many parts of the industrial cities, the whole city formed a community. It was not merely altruism or beneficent self-interest which made some municipal reformers agitate for slum clearance and municipal improvement. An important role was played by the sentiment of community; i.e., the conception that any blemish in the city's landscape was a matter of concern for all its citizens.

This sentiment of community became concretized in at least two institutions and types of attitude which persisted thoughout the Western world. One was the tradition of self-government—of the city's taking care of its own present and evolving needs. This found expression in at least some political consciousness and activity of most of the city's inhabitants. The vigorous development of municipal self-government and public services in all Western cities is largely an outflow of this tradition, although it developed different forms in different localities. Secondly there persisted, also, even if in a changing and transformed way, the attachment of the city's population to the urban way of life. This became manifest on the one hand in the clearer distinction between city and country and the city-dwellers' self-consciousness of the differences in their attitudes and objectives from those of the countryside. On the other hand, it showed itself in the development of functionally specific areas in the city itself. The concentric arrangement of modern industrial cities, with their central business districts and their changing belts of industrial and residential areas, the location and function of

[2] On the differential growth of British cities in the nineteenth century, see the forthcoming article by Eric Lampard, "The History of Cities in Economically Advanced Areas," *Economic Development and Cultural Change* (January, 1955), pp. 111-112.

district shopping centers, the pattern of intracity communications, and many other features of a similar kind, show the interdependence of all parts of a city upon one another and are proof of the over-all unity of the urban community in spite of its internal wide diversity.

A second common characteristic of the industrial cities in the advanced countries is that their growth was by and large associated with the demand for labor and that people moved to the cities because employment opportunities existed there. This process was made possible by the often rapid growth of industrial investment in the city. Let us again take the example of Manchester, which exhibits this pattern clearly. In 1780 only one factory, that of Richard Arkwright, could be found within the perimeter of Manchester; in 1790 the first Bolton and Watt steam engine was installed in a cotton mill; and by 1820 there were sixty-six cotton, six silk, and six woolen spinning mills in Manchester. By that time factory spinning became unprofitable unless combined with weaving. As a consequence, the manufacturing capitalists were obliged to install the improved power loom and to integrate the spinning, weaving, and finishing processes in one single establishment, each of which employed between three hundred and a thousand workers. By 1845 there were more than a hundred integrated factories in Manchester and Salford. But in addition to this growth of the cotton industry, subsidiary industries were progressing. Machine makers, forges, tin plating establishments, leather workers, braziers, harness makers, as well as railways, canals, breweries, and chemical industries, were clustering around the cotton mills of Manchester and Salford. This expanding concentration of industry called forth a secularly ever increasing demand for labor.[3] As a city grew in population and as subsidiary production developed around each city's main specialized industrial branches, external economies for these main industries increased. Anyone newly establishing a cotton mill in Manchester could count not only on the presence of a skilled and disciplined labor force, but he had suppliers of raw materials, wholesalers to whom he could easily sell his output, sources of credit, repair shops for his machinery, and many other subsidiary services near at hand. This in turn encouraged the further expansion of the industrial base of the city and called for further increase in employment opportunities and a growing demand for labor.

A third characteristic of the industrial city in economically advanced countries is the role it plays in the process of cultural change. Robert Redfield and Milton B. Singer distinguished two phases of cultural change which are mediated primarily by urban centers ("The Cultural

[3] Leon S. Marshall, "The Emergence of the First Industrial City: Manchester, 1780-1850," in Caroline F. Ware, *The Cultural Approach to History* (New York, 1940), pp. 140-143.

Role of Cities," *Economic Development and Cultural Change*, October, 1953, page 59). One is the phase of "orthogenetic" and the other of "heterogenetic" cultural transformation. By orthogenetic cultural transformation Redfield and Singer mean the development of a great tradition, such as took place in the cities of ancient Egypt, China, Mayan Yucatan, and the Christian centers of the European middle ages. Heterogenetic cultural transformation, on the other hand, is a process in which acculturation—the absorption of new culture elements from the outside—takes place and in which predominantly rationalistic tendencies prevail. It is obvious that the growth of the industrial city is a process of heterogenetic cultural transformation. But the industrial cities were not the spearheads in this process but latecomers. The phase of heterogenetic transformation of Occidental cities began with the end of the middle ages, and the "spirit of capitalism" and of rationality were well developed and fully accepted into the pattern of Western social values when the rise of the industrial city began. In other words, the growth of industrial cities took place in a cultural climate favorable to the further development of the technical order in which the modern industrial city could flourish. The industrial city, in turn, soon became one of the chief centers of social change and played an important role in strengthening the new technical order based on rational allocation of resources, competitive markets, and the pursuit of self-interest in economic action.

This development in the value system of the modern world, of which the industrial city is one of the most characteristic symbols, has sometimes been expressed by stressing the presence of universalist, achievement-oriented norms, as against the predominance of particularistic, ascription-oriented norms in less developed societies.[4] This transition in value structures is accompanied by stresses and strains which tend to produce features of social disorganization in various degree. The impact of the modern industrial city, as a latecomer in this transformation process, made itself felt at a time when the basic alteration in social values had already been achieved and its main role has been to reinforce an already existing system of social values rather than to participate in the creation of new ones. Although the industrial cities of the Western countries have witnessed episodes of severe social disorganization, they have been able to overcome these disruptive

[4] The original definition of these variables was given by Talcott Parsons, *The Social System* (Glencoe, 1951), pp. 58 ff. They were applied to the analysis of economic growth in general by Bert F. Hoselitz, "Social Structure and Economic Growth," *Economia Internazionale*, August, 1953, pp. 52-72. The same conceptual scheme was applied to the analysis of the modern industrial city by William L. Kolb, "The Social Structure and Function of Cities," *Economic Development and Cultural Change* (October, 1954), pp. 30-46.

tendencies better because they were not places of cultural innovation but rather of cultural consolidation.

III

Before examining to what extent the origins of industrial cities in the underdeveloped countries differ from those of Western industrial cities, we must take a look at one group of Western cities which have performed chiefly other functions than specialization in given branches of industrial production or the supply of specified services. These cities are usually referred to as "central cities."[5] The most characteristic central cities are the capitals of some European countries; for example, Paris or London. They are not only political and cultural centers of their respective countries but also perform some of the central economic functions in the national economies. The money markets, often the transactions with foreign countries, the main wholesale and retail houses, and several important industrial and manufacturing branches are concentrated in these cities.

The history and role of central cities differ markedly from the industrial cities. Central cities—even central cities of only provincial importance—are on the whole older than industrial cities. They developed out of towns which had some political importance already in medieval times, and many of them have tended to perform "central city functions" for a considerable period of their history. Economic growth has been more halting in them, traditions have lingered longer, and their influence has often been on the conservative side. Although some capitals grew as rapidly as some industrial cities, many provincial central cities developed much more slowly.[6] They resisted industrialization more effectively than the newer cities and the influx of migrants into these central cities was motivated not always by search for employment or economic betterment but by expectations of political promotion, or simply the desire to be on the scene where important new events were taking place. The general transformation in the value system has affected the central cities as well as the industrial cities. But in the former the raw edges were somewhat smoothed, extremes were, on the whole, avoided, and the impact of the new economic ethic was less drastic. It is probably no accident that the economic ethic extolling universalist, achievement-oriented norms is referred to as "Manchesterism" and that American sociologists have chosen Chicago

[5] Edward L. Ullman, "A Theory of Location for Cities," in Paul K. Hatt and Albert J. Reiss (ed.), *Reader in Urban Sociology* (Glencoe, 1951), pp. 123-132.

[6] On the different ranks of central cities see especially Walter Christaller, *Die zentralen Orte in Sueddeutschland* (Jena, 1935), and Ullman's discussion of Christaller's schema, *op. cit.*, pp. 126-127.

as the model when they wished to describe the specific flavor of the sprawling, big, American industrial metropolis.

IV

If we now compare industrial and central cities in the underdeveloped countries with their counterparts in the advanced countries, we find several rather startling differences. Above all, there are hardly any genuine industrial cities in underdeveloped countries. Though the absolute number of cities with population 100,000 and above is as large in "preindustrial" countries as it is in industrial countries, the very fact that in the former set of countries more than half the males are occupied in agriculture, fishing, or forestry explains the scarcity of genuine industrial cities in these countries.[7] Though such cities as Kanpur or Ahmedabad in India, Maracaibo or Monterrey in Latin America, and Dharan in the Middle East may be regarded as genuine industrial cities, the majority of the cities of underdeveloped countries perform numerous central city functions and industry plays often only a relatively subordinate role. Thus, if Motwani designates the cities in underdeveloped countries as "mausoleums of coal smoke, iron and steel," he can speak only of very few such cities, and even there of relatively limited quarters only. The appropriate counterpart for comparisons with cities in underdeveloped countries is, therefore, not the industrial cities, but the central cities of the advanced countries. But the central cities have shown themselves more stable and more resistant against the introduction of new values. They have resisted, also, to some extent the introduction of industry. They have had fewer upheavals, there was less disorganization, and there was maintained a higher degree and more profound sense of collectivity-orientation than in the industrial cities of the advanced countries. It is the contrast between places like Toulouse, Munich, Edinburgh, or Albany—cities with important central city functions and fair degree of industrialization —and centers such as Dakar, Djakarta, or Mexico which comes under scrutiny.

If we trace the actual origins of the cities in underdeveloped countries, we find that many of them, like the bulk of the industrial cities of Europe and North America, are new. They were founded by foreign traders or invaders; in the colonial and excolonial countries, by representatives of the colonizing power. Some of the cities in underdeveloped

[7] Kingsley Davis and Hilda Hertz Golden have shown in the article, "Urbanization and the Development of Pre-Industrial Areas," *Economic Development and Cultural Change* (October, 1953), p. 9, that of the world's 897 cities with populations of more than 100,000 persons, 463 are located in countries with more than half the occupied males in agriculture and 434 in other countries. The total population of the first group of cities is roughly 160 million; that of the second group, only approximately 155 million.

countries are planted upon the native cities which existed previous to contact with the West. These last cities—places like Mandalay or Benares, Mecca or Fez—have shown most refractory against change and the introduction of industry. They often played an important role in the orthogenetic transformation of the urban culture of the underdeveloped country; they were "sacred" cities in which important traditions of the cultural past of these countries were preserved. They have resisted the introduction of new values and new methods and the Westerners found it often easier to leave these cities as oases of "backwardness and superstition" and to construct their own cities elsewhere. This experience has been made also in tropical Africa. The relatively large settlements of indigenous peoples which existed before the full penetration of parts of West Africa by Europeans appeared to be ideal sites for the establishment of commercial and industrial activities by the colonizing powers. Often these settlements (as for example some of the larger towns of the Yoruba in Nigeria) were situated on favorable spots on rivers or in sheltered bays. There was a large population nearby which offered a potential ample labor supply and housing of a sort already existed in the native town. Hence these towns appeared, on the surface, as the most promising places for economic development. But it was soon found that the very existence of a previous settlement, with all the customs and beliefs which had developed in it and around it, proved a very unsuitable place for rapid change. There were serious rigidities, even revolts. The generalization that a city is, in general, the readiest place for innovation proved fallacious with respect to these towns. The most rapidly growing and most "Westernized" settlements in Africa were not the places with a long urban history but the recent creations of the Europeans.[8] Thus many cities—even in countries with old cultures—are new and, in addition, creations of outsiders. Balandier says of West and Central Africa (an area with considerable urban traditions) that the only city "which has risen to the scale of a great European town" is Ibadan in Yorubaland, Nigeria (*op. cit.*, pages 3-4). All the other cities were founded and built by whites, although they are inhabited predominantly by Negroes.

It is, therefore, not surprising that in African cities a sense of community, as we know it in Western cities, is absent. This does not mean that Africans are not collectivity-oriented. On the contrary, they are

[8] On urbanization in Africa, see the Working Papers submitted at the Conference of Social Scientists on the Social Impact of Industrialization and Urban Conditions in Africa, held from September 29 to October 7, 1954, at Abidjan, Ivory Coast, under the auspices of UNESCO. Cf. especially the papers by Rhona Sofer, "Adaptation Problems for the African Population in a Society of Early Industrialization at Jinja, Uganda," and by Georges Balandier, "Urbanism in West and Central Africa: Suggested Trends of Research" (mimeographed).

more deeply tied to the group to which they feel they belong than Westerners. But the group to which the African is loyal is not the city in which he lives, but his family, his kin, his tribe, his village. African cities are, therefore, collections of many separate compounds. This characteristic of the African city is confirmed by the all too few social surveys which have been made in Africa. For example, Mrs. Sofer writes that the African population of Jinja is heterogeneous and variable (*op. cit.*, page 1), and Balandier prefers to speak of "les Brazzavilles noires," indicating that the various African quarters of Brazzaville are as many separate towns *(op. cit., passim)*. This lack of unity in urban agglomerations is found also in other underdeveloped countries. Redfield describes Mérida of 75 years ago and shows that by that time the various *barrios* formed virtually self-contained independent villages. Though some integration has taken place since that time, many particularistic features still prevail. Pierre George discusses the *bidonvilles* of the cities of French North Africa, and finds there also a high degree of particularism. Finally, and most importantly, the lack of internal cohesion and unity has been found to be characteristic of the cities of India, Burma, Thailand, and Indonesia. In fact in some Indian cities it appears to be so pronounced that some students believe this trend towards particularism to be a lasting feature of Indian urban development.[9]

One of the great contrasts between the cities of the underdeveloped countries and those of the West is therefore the absence of a "city-consciouness" in Asia and Africa and perhaps also in Latin America. This implies, at the same time, that the distance between urban and rural styles of life is less pronounced than in Europe; that the loyalties of the urban dwellers are frequently to groups whose center of gravity is outside the city; that sojourn in the city is regarded often as only temporary; that migrants to the city from one village or province not only tend to settle in clusters of their own but that even when they have become permanent city dwellers they maintain some ties with the region they came from; and that each district of the city forms a community of its own, often rigorously separated from the others. These differences have already been pointed out almost twenty-five years ago by Max Weber. He examined the pecularities of Chinese, Indian, and Arabic cities and concluded that "only the West has known an urban community [*Stadtgemeinde*] in the true sense of the word as a mass

[9] See Robert Redfield, *The Folk Culture of Yucatan* (Chicago, 1941), pp. 25-28; Pierre George, "Bidonvilles, a Form of Urban Development in Underdeveloped Countries" (lecture presented at the International Conference on Underdeveloped Areas at Milan, October 10-15, 1954); McKim Marriott, "Some Comments on William L. Kolb's 'The Structure and Function of Cities,' in the Light of India's Urbanization," *Economic Development and Cultural Change* (October, 1954), pp. 51-52.

phenomenon." (*Wirtschaft und Gesellschaft* [3rd ed.; Tübingen, 1947], Vol. II, pages 523-528, especially 523.)

The consequences of this fact are of considerable gravity. Most of the European migrants to the cities in the eighteenth and nineteenth centuries came from the countryside, just as the migrants to cities in underdeveloped countries today. But whereas the European, once he had reached the city and lived there for a short time was able to cut himself loose from his old home because he found a new home with new loyalties in the city, the Asian or African does not experience such a transfer of loyalty. He continues to "belong" to the place whence he came and he never feels fully and exclusively at home in the city. The migrant to the city in underdeveloped countries lives in a strange and foreign place as long as he remains in the city and this alone increases the psychological stress he experiences. It means, also, that he seeks as associates only persons who come from the same kinship group, village, or province as he himself. In this way almost all interpersonal relations into which he enters are strongly influenced by the pattern of group formation peculiar to his native culture. He works with men who have the same ethnic and often even the same local origin. He lives in a social structure in which authority and responsibility are distributed according to patterns characteristic of his native culture rather than that of the city and its economic needs. His relationships with his fellow-workers, his supervisors on the job, his trade-union—if he is a member of one—are expressions of the culture and social structure of his native environment and not of the city or even of the European model after which they were patterned. It is therefore not surprising that we find among the noncommunist union leaders in South Asian countries two groups, one of which "consists of leaders whose authority is local and based on native traditions" and the other of which, though inspired by Western models, tends to "resort to autocratic measures, making it hardly distinguishable from traditional forms of leadership, except perhaps in its professed ultimate objectives." (George E. Lichtblau, "The Politics of Trade Union Leadership in Southern Asia," *World Politics*, October, 1954, pages 92 and 98.)

The urban environment in underdeveloped countries has therefore the effect of increasing greatly the anxieties and uncertainties of the recent migrant, to make him look for security in familiar surroundings, and to seek out associations in which familiar patterns of social structure, authority, and responsibility prevail. This creates a sentiment of strong ambivalence towards the city, even on the part of those who have come to the city because they were attracted by its glamor and of those who have no place else to go, for whatever reason they came. It further explains why the pushes in order to impel people

to migrate to the cities must be so strong, and why, in spite of apparently appalling conditions in some of the overcrowded rural areas of India, Java, and other parts of the underdeveloped countries, people continue to remain in their villages.

V

And yet we find cityward migrations in underdeveloped countries which are larger than is warranted by the newly created economic opportunities in the cities. Here is another difference between the cities of underdeveloped countries and those of the advanced countries. The rapid growth of the industrial cities of Western countries occurred simultaneously with the rapid growth of industrial investment and hence the movement to the cities was in large part a response to the opening up of new employment opportunities there. To be sure, many migrants had only very dim expectations as to what they would find in the city, but wave after wave of migrants found even fairly optimistic expectations confirmed. Sometimes they had to suffer hardships and deprivation for a period; sometimes they flourished as soon as they arrived. But during the entire growth process of the cities in advanced countries, the expectations of the migrants to find employment and improve their economic position were confirmed in the long run.

The pattern of cityward migration in the underdeveloped countries is different. Some migrants are attracted by the glamor of the city and some by expectations of economic improvement. But most persons who come to the city are pushed out from where they have lived before by events beyond their control. In some cases it may be the rigors of famine and other material hardships which drive people out, in other instances it may be civil war and banditry, but in most cases the forces which succeed in pushing people into the cities are persistent and strong.[10]

The most rapid growth of cities in underdeveloped countries took place in the last few years. Accurate figures are not available for all countries, but we have fairly good estimates for the growth of cities in some countries. Some very interesting estimates of urban growth in Indonesia have been published by W. L. Utermark. He presents a table in which he lists fourteen of the largest Indonesian cities, dividing them into two groups. One group is composed of cities which grew

[10] See Wilbert E. Moore, *Industrialization and Labor* (Ithaca, 1951), especially pp. 48-70 and 94-96. On cityward migrations due to famine, see Owen Lattimore, "The Mainsprings of Asiatic Migration" in Isaiah Bowman (ed.), *Limits of Land Settlement* (New York, 1937), p. 133, and Walter H. Mallory, "The Northward Migration of the Chinese," *Foreign Affairs*, October, 1928, pp. 80-81 (migration of Chinese to Manchuria out of famine-stricken areas in Shantung and Hopei). The main instances of cityward migration as a consequence of civil war and banditry occurred in recent years in Indonesia, Burma, Malaya, and the Philippines.

primarily because of cityward migration from their immediate hinterland, apparently motivated largely by economic opportunities in the city. The other group is made up of cities which grew primarily because people from all over the country hoped to find in the city shelter from insecurity, political advancement, or other opportunities associated with the rapid social and political change of the times. The first group embraces seven cities which in 1930 had a combined population of 536 million and in 1950-51 of 977 million. The second group also embraces seven cities which were inhabited by 1,386 million people in 1930 and by 4,329 million in 1950-51. The rate of growth in the first group was 82 per cent and in the second 212 per cent.[11] This shows that not only are the largest cities growing faster than the smaller ones, but also that cityward movement was considerably more important for noneconomic reasons than for economic ones.

It is not difficult to assess the consequences of this form of development. In the first place, it adds to the insecurity and stress which the migrant to the city experiences. Not only is he torn from his accustomed environment, but he finds that his very existence is put in question in the city. He literally lives from hand to mouth; he is available for any job to be had (including hiring himself out for political demonstrations or rioting);[12] he becomes demoralized and an easy prey for radical agitators of all hues; he lives in unspeakably rotten slums, much worse than those of Manchester in early Victorian times, and scarcely equaled by the *bidonvilles* of North Africa and Spain. A large proportion of the poor people in many underdeveloped towns are not an "industrial reserve army," but a demoralized, unhealthy, pitiful *lumpenproletariat*. Even if a sudden spurt of industrial investment were to take place in these cities, it would be difficult to transform these people into disciplined, effective factory workers.

Another aspect of this recent in-migration is the very high sex ratio in many cities of underdeveloped countries. This phenomenon is not unknown in advanced countries. Among the immigrants from Europe to the New World, men were in the majority, and if we omit persons under eighteen years of age, the proportion of male immigrants increases. Most of these immigrants went to cities, and hence one many say that here was a predominantly male cityward migration. In the underdeveloped countries, also, men predominate so strongly among the migrants that the sex ratios in cities are extremely uneven. For example,

[11] W. L. Utermark, "Aspect économique de l'attraction exercée par les centres urbains et industriels en Indonésie," in *L'attraction exercée par les centres urbains dans les pays en voie d'industrialisation* (Brussels: Institut International des Civilizations Différentes, 1952), pp. 226-234, especially p. 232.

[12] I am told by an American official, resident in Iran, that participation in political rioting—one day for and the next against Mossadegh—was one of the more attractive forms of making a livelihood for a portion of Tehran's *lumpenproletariat*.

in four of the eight largest Indian cities the preponderance of males is very strong. In Calcutta there are 602, in Bombay 569, in Ahmedabad 764, and in Kanpur 699 women per 1,000 men. Similarly, in Africa the male urban population is much larger than the female. In the Belgian Congo, for example, there are 696 women per 1,000 men.[13] In many of the mining camps of the Copper Belt in Rhodesia or the Rand in the South African Union the imbalance is even stronger.

The consequences of these facts are not difficult to assess either. The great preponderance of males means that stable families are rarer in cities than in the country. The men who come to the city are single or leave their wives behind. In the former case they may regard their stay in the city as temporary, since they often come in the hope to make enough money to go back and get married. In the latter case they do not break their close tie with their native village; they are not city dwellers but temporary residents. This situation adds to the instability of the city population in underdeveloped countries. It also adds to the stress experienced by the new immigrants and is one of the chief causes for the prostitution, gambling, racing, dope dealing, and other forms of social disorganization and crime which Motwani notes with so much displeasure.

All these factors conspire to make the large cities in underdeveloped countries highly unsuitable places for the development of new forms of social cohesion and solidarity. Although whatever change in the culture and social values occurred in Asia and Africa did primarily take root in the cities and then emanated from them, the changes that have actually been achieved have been slow and halting, often impeded by the lack of unity, instability, and heterogeneity of the city population. If progress towards universalistic, achievement-oriented social values has been difficult in the cities of Asia and Africa, it is not due to the stubbornness of the people or their "superstitious" adherence to old ways of life, but rather to the very unfavorable conditions under which they have been brought to the city and under which they continue to exist.

VI

It is in the light of this contrasting impact which cities have exercised in advanced countries and now exercise in underdeveloped countries that we have to appraise the industrialization of less advanced countries. The analysis of this aspect of economic development can be short because there exists already a considerable literature

[13] The figures for India are for 1951. Cf. India, Census Commission, *Census of India, 1951* (Delhi, 1953), Vol. I, p. 56. The figures for the Belgian Congo are for 1949. Cf. A. J. Moeller de Laddersous, "Attraction exercée par les centres urbains et industriels dans le Congo Belge" (Institut International des Civilizations Différentes), *op. cit.*, p. 192.

forms of making a livelihood for a portion of Tehran's *lumpenproletariat*.

on this subject.[14] In principle, we may distinguish three types of impact exercised on a nonindustrial labor force by the introduction of mechanized industry. The first is the need for adjustment to the new environment, and it is particularly in this area that the new urban agglomerations are important factors in the formation of an industrial labor force in underdeveloped countries. The second impact is that of a new technology: the replacement of handicraft work by machine work, the acquisition of new skills, and the co-ordination of the tempo and rhythm of work with the machine rather than the freer and less rigorously structured pace of the artisan. The third impact is the imposition of new social relations appropriate to modern forms of industrial organization: new hierarchies, new social groups, new patterns of authority develop which are strange and often contrary to those to which the new recruits of industry are accustomed. The workers find themselves in situations which in terms of their old culture either have no analogue or which are flatly opposed to the old culture. The high-caste Indian, working side by side in a factory with a low-caste Indian; the mixture of different racial or ethnic groups in a factory who previously held strictly aloof from one another; the congregation of people who previously had known co-operation only on the basis of kinship ties, in a labor union or a sectional work crew in a factory—all are cases in point.

As concerns the impact of the urban environment, little needs to be added to what has been said before. The city in an underdeveloped country is an inhospitable environment for many actual or potential laborers. Reference is made not so much to slum conditions and bad housing, although these have detrimental effects on the morale of an industrial labor force. Nevertheless, in many underdeveloped countries actual health, sanitary, and housing standards are no worse, and often better, than in the rural areas whence the workers come. To be sure, conditions are often worse than they were in nineteenth-century Europe, but compared with what the cityward migrant in many underdeveloped countries left behind, his standard of real welfare suffers hardly any diminution. The inhospitality of the urban environment expresses itself rather by the serious psychological strains it exerts on the worker. Employment is uncertain and wages too low to permit savings for a rainy day. Families are torn asunder; the comforting security of a known and accustomed environment is lost. The number of face-to-

[14] In adition to Wilbert E. Moore's book, mentioned earlier (in note 10) there exists a considerable literature on this topic sponsored by UNESCO, ILO, and various national governments. A good bibliography is contained in Wilbert E. Moore, *op. cit.*, pp. 365-398. Other writings are listed in *Current Sociology: International Bibliography of Sociology*, Vol. I and ff.; and Daniel Craemer, *Bibliography on Income and Wealth*, Vol. II (Cambridge, 1953), especially under the headings "Estimates of Labour Force by Geographic Areas" and "International Comparisons of Labour Force Estimates."

face relationships with persons whom one does not know is increased; the loneliness of the individual becomes, in some cases, almost absolute. These conditions impair morale, discipline, and steadfast devotion to a given task. It is not the noise or the soot in the city which corrodes the nerve of the worker, as Motwani has it, but rather the "absence of neighborhood"—the anonymity and impersonality of life in a big city. These factors go far to explain the high absentee rate, the high rate of turnover, and the low standard of performance of many industrial workers in underdeveloped countries.

The impact of the new technology pales in importance besides this impact of the urban environment. New skills can be learned and have been learned with surprising speed by many peoples. It has been pointed out again and again by anthropologists that the technical manipulations involved in handling complex machines have been learned with relative facility by peoples in underdeveloped countries who had no previous knowledge of these machines and whose general standards of literacy and education left much to be desired. No less an authority than the late Ralph Linton wrote: "I would back certain Polynesian and Swahili mechanics of my experience against nine-tenths of American garagemen for ingenuity and mechanical know-how."[15] The retarding factor is not the incapacity or lack of interest on the part of workers in underdeveloped countries to acquire the requisite skills but rather the inadequate provision for training, the great shortage of educational facilities, and the impediments, in many instances, to advancement which deprive workers of incentives to improve their performance. The "demonstration effect" described by Ragnar Nurkse with reference to changes in consumption patterns of the peoples in underdeveloped countries exercises an influence here also. (*Problems of Capital Formation in Underdeveloped Countries* [New York, 1953], pages 64-65.) Imitation of Western methods and the use of Western machines are recognized as means to increased output and ultimately better living standards. What is absent is not the willingness or human ability to use these machines and methods, but the social institutions which permit their use to these ends.

This leads us to the knottiest and most difficult problem in the industrialization process in underdeveloped countries: the impact of the new forms of social structure and the requirements of new types of social behavior appropriate to large-scale production in the factory. The restructuring of social relations occurs as a result of the mobilization of the labor force implying the imposition of cannons of employ-

[15] "Cultural and Personality Factors Affecting Economic Growth," in Bert F. Hoselitz (ed.), *The Progress of Underdeveloped Areas* (Chicago, 1952), p. 78. Linton presents an exhaustive discussion of this point, giving other examples on pp. 77-79.

ment prevailing in the industrial communities of Europe and America. It is hardly necessary to point to the striking differences between the economy in the West and that of most other societies. In the West it is based upon individual effort, whereas most of the other peoples are communally-oriented. Hence in the West the worker acts as an individual. In the economies of most underdeveloped societies, on the other hand, the individual acts usually as a member of a group based on kinship or residence or both. His responsibilities are considered not merely those he individually undertakes, but also those of the other members of the group to which he pertains. Similarly any compensation he may get for his effort is regarded by him not merely as a reward to be employed for his individual purposes but to be shared with the other members of the collectivity of which he forms a part. In many societies there are still recognized traditional arrangements for the control of money income which secure to the older members of the community the major handling of what money income is secured. Laborers must share their income with chiefs, parents, fathers-in-law, or other relatives and persons of respect.[16] In addition to this, the ranking of persons in terms of traditional status relations may differ considerably from that established in a factory work situation. Older men or chiefs or other persons with high traditional status may be subordinate in the factory to younger men, and this subordination may express itself not merely in a lower wage but in actual positions of inferiority in the factory hierarchy or a work crew. The disruptive effects not only on traditional social structures but also on the psychological security of the persons involved are obvious if a man with high traditional status must obey orders of someone who in the traditional ranking is far below him simply because the new distribution and valuation of skills makes this new relation mandatory.

I do not wish to suggest that these changes should be prevented in order to maintain an old cultural tradition or even to preserve certain traditional social structures from disruption or disorganization. I am merely pointing to these changes which, in the light of productive efficiency, may be necessary in the new work situation, in order to explain why the new social relations established in the factory are often contradictory to the traditional and accepted views of a man of himself and his relation to his fellowmen. It is also important to state that, other things being equal, the farther removed the factory is from the locus in which traditional social relations exist, the easier will it be accepted and the fewer psychological or cultural impediments will

[16] A number of such instances are reported by Raymond Firth, "Money, Work, and Social Change in Indo-Pacific Economic Systems," *International Social Science Bulletin*, 1954, pp. 406-407.

stand in its way. A factory located in a village in which the old culture is still strong and vigorous is likely to create more conflict than one located in a far-away city. The more homogeneous, moreover, the labor force in a given factory the more likely will be its resistance against changes in the accustomed patterns of authority and responsibility. (An instance of this is reported by John Useem, "South Sea Island Strike" in Edward H. Spicer [ed.], *Human Problems and Technological* Change [New York, 1952], page 149-164.)

VII

We are now in a position to draw together the various strands of this analysis. The impact of the modern factory is exerted along technological and social-structural lines. The former create relatively few difficulties, but the latter may be a cause of serious maladjustments and resistances reflected in the over-all low productivity of the labor force. These impediments to effective control over the labor force result from the contradictions—which are not necessarily present, but do in actual fact occur often—between the traditional structure of the social status system and the new social relations created by the work situation in the factory. The city, on the other hand, has the definite tendency to break down the old cultural traditions and to weaken the influence of traditional status considerations and interpretations of the worker's place in the social hierarchy. Though residence in a city may create anxieties and stress often bordering on *anomie,* this very same process may produce the result of creating in the co-operation with other workers in the factory an environment in which the new social relations established there may come to be regarded rather as a positive integrating than a disorganizing factor. A recent migrant who feels lonely in the city may welcome the relative security resulting from his being placed in close daily contact with individuals whom he gets to know and with some whom he may become friendly. To be sure, one should not exaggerate the loneliness experienced by many recent migrants to the cities of underdeveloped countries. They usually find some person to whom they are related by kinship ties or ties of previous residence. And if this fails, they get in contact with persons who speak the same dialect and come from the same general portion of the country as they themselves. This enables them to maintain at least a semblance of group relations along traditional patterns. But the closer these group relations are to the traditional pattern and the more intimate the cohesion of this traditional collectivity, the greater will be the difference between the interpersonal relationships in this group and the pattern of social relations in the factory.

This then is the price paid by peasants who come to the city where

they are forced to become industrial workers. The transformation of a predominantly rural society, based on the relatively complete self-sufficiency of a village or a group of villages, into an urban society, based on the interdependence of various portions of a country, is necessarily associated with features of social disorganization. On the sociopolitical level they appear in the form of increased crime rates, imbalanced sex ratios, political extremism, and various other kinds of social maladjustment which usually demand some centrally implemented policy in order to be remedied. On the level of the individuals most concerned, i.e., the nascent industrial workers, this process manifests itself in the increase of stress either due to the new urban environment or the new social relations prevailing in large-scale factory production. In many cases these two sources of stress pull in the opposite direction. If the migrant to the city finds an already established community there in which accustomed intergroup relations prevail, the impact of the new social relations in the factory tends to produce the stress. If he does not find a group in the city resembling those of his home, the loneliness and anxiety which he experiences are the chief causes of stress, and the entry into a new system of social relations in the factory, in a work crew, or a trade-union may be a factor of mitigating this stress.

The persons who in the last few years have swelled the cities of underdeveloped countries do, in a sense, play the role of pioneers. The gap in culture, form of economic activity, and patterns of social relationships which they had to bridge will become narrower for those who follow. But the magnitude of the task and its ultimate importance for the welfare of the total population of underdeveloped countries appear to make these sacrifices justified. It is the toll which a rural peasant society has to pay to become urbanized and industrialized.

DISCUSSION

GEORGE B. BALDWIN: The title of our session can be interpreted in two different ways and this our speakers have done. Professor Moore has selected one of these phenomena—industrialization—and has addressed himself to it alone. Professor Hoselitz, on the other hand, has analyzed the relationship between urbanization and industrialization.

Moore is concerned with the development of a low-order theory of human motivation that will enable us to predict how preindustrial populations will behave in a variety of ways that are significant in determining the progress of their transition to an industrial society. Guarding his rear with a suitable set of qualifications, Moore takes to task both the economists, whose oversimplified assumptions about a universal human desire to maximize limitless economic satisfactions lead them to an unwarranted optimism about the potential rate of industrialization, and the anthropologists and sociologists, whose humility before the facts of psychological complexity and cultural relativity lead them to underestimate the possibilities for development and to be anxious about its individual and social consequences.

After contrasting the motivational assumptions underlying economic theory on the one hand and anthropology and sociology on the other, Moore reviews the empirical evidence concerning experience with the recruitment and utilization of labor in undeveloped areas. In the light of this evidence, he attempts to identify the elements out of which a low-order theory (presumably, at last, an interdisciplinary theory) can be built that will allow us to predict experience with labor recruitment and labor utilization. For a spokesman from a pessimistic breed, Moore offers us a remarkably optimistic theory: although human wants in tradition-bound, preindustrial societies are often limited in the early stages, the evidence suggests that we can expect both wants and, consequently, the supply of labor to expand with the spread of knowledge, education, and the growth of new values. This emphasis on changes in human motivations and aspirations over time—changes which can be accelerated by appropriate policies and practices—certainly makes sense in terms of my own limited research on the growth of industry in South India. Moore concludes with a general plea for the cultivation of a "sense of social participation" among workers in new industrial societies as partial compensation for their uprooting from their traditional mode of life.

Professor Hoselitz is less concerned with model building. He pursues the utilitarian objective of trying to "isolate the impact of industrialization from that of urbanization" so that economic planning can proceed on a more informed basis. We are cautioned that there is no necessary connection between urbanization and industrialization, but Hoselitz accepts the view that economic development will be well served where both phenomena occur together. He then analyzes certain key characteristics of the industrial cities of the West and compares "industrial" and "central" cities in both underdeveloped and

advanced countries. Hoselitz places much emphasis on the historical role of urbanization in confirming and accelerating the spread of those rationalistic, universalistic, achievement-oriented habits of thought on which industrialization depends. Equally important, historically, was the growth of what Hoselitz calls "city-consciousness" (closely akin, I take it, to civic pride) in helping the new urban populations of the West accept their transfer from a rural to an urban environment. This "city-consciousness" or "sense of community" seems to be almost wholly lacking in the mushrooming urban centers of Africa, Southeast Asia, and "perhaps" in South America. The result in these cities is the development of discontented, maladjusted populations that have little unity and that do not transfer their psychological roots to their new environment. As a consequence, these cities are not favorable environments for the growth of those attitudes that seem essential to industrialization.

Hoselitz concludes by summarizing industrialization's threefold impact on a preindustrial labor force:

1. People are forced to adjust to a new environment—a city. This necessarily involves social disorganization which manifests itself, at the social level, in higher rates of crime, unbalanced sex ratios, political extremism, and the like. These maladjustments require "some centrally implemented policy" if they are to be overcome.
2. People must adjust to a new technology. This is the least difficult of the three adjustments and requires mainly an increase in the number of vocational training facilities.
3. People must submit to new social relationships, particularly those required by factory organization. Hoselitz finds this adjustment the most difficult in the whole process of industrialization.

So much for the main papers. There are just three comments I want to make about them.

First, what of Professor Moore's attempt to construct a new low-order interdisciplinary theory of worker behavior? To my mind, this does not come off very satisfactorily. Indeed I doubt that this is the direction in which those of us concerned with economic development ought to push. I say this because of what I feel to be the inherent difficulty of the task: as soon as we try to build a model that will embrace the psychological complexities and cultural differences to which Moore pointed, our model becomes so abstract and qualified that it loses all usefulness as a map to the real world. At the end of his paper Moore himself seems to abandon his own proposal for something strangely similar to the economist's (or at least the labor economist's) traditional model of the labor market. However, Moore rightly points out that it may take considerably more time to realize, in an underdeveloped economy, the behavior responses that the economist's model predicts. In short, if we could agree on how long the "long run" is, maybe we would find the economist's model less misleading than Moore initially suggested. In model building, we can substitute time for noneconomic disciplines: the longer the time period, the more narrowly "economic" we can afford to be in our theory.

After all, the theoretical economist's model of human behavior is not very

much farther off the mark when applied to the preindustrial labor market of underdeveloped areas than labor economists have found it when analyzing behavior in the labor markets of Pittsburgh or Detroit. That model has not been found useless, but it has stood in need of adjustment and caution in its use. A similar adaptation seems in order when analyzing the problems of backward areas. Certainly my own attempt to understand Bangalore, South India, as a labor market was greatly helped by traditional economic theory and by familiarity with Western personnel and industrial relations practices. I mention this because it was the similarities, not the dissimilarities, that leapt to mind in comparing these Indian and American labor markets.

Having made my defense of economics, let me hasten to add that the study of economic development does require that economists pay much more explicit attention to the traditional parameters of their discipline. This will inevitably bring them into closer touch with anthropology and sociology, even with anthropologists and sociologists.

My second comment is suggested mainly by Professor Hoselitz' paper. It is the very pedestrian point that we badly need a lot more empirical research before we begin taking our theoretical generalizations very seriously. Hoselitz' paper struck me as first-rate example of the way in which an imaginative and widely-read scholar can stimulate new insights and suggest specific topics for research by giving us some bold generalizations. But he makes many specific statements to which I would take exception. We need to test such generalizations with intensive case studies of industrial growth and urbanization in a number of countries. In doing this, we shall need to break up the phenomenon of "industrialization of the labor force" into many specific problems: labor mobility, vocational training, union organization and collective bargaining, recruiting methods, wage structures and methods of payment, absenteeism, turnover, labor productivity, company welfare policies, promotion arrangements, and so on. Similarly, specific studies on aspects of urbanization are equally required. What evidence is there, for example, that the city planning movement, which is surprisingly strong in some Indian cities, may be able to affect the shape of industrialization in such a way that the less attractive features of industrial urbanization can be avoided and that a sense of community can be developed?

Finally, my third and most important comment is to point out what I felt to be an important omission from both papers; neither lays any stress at all on what I feel may be the most important factor in conducting a tolerable transition from a preindustrial to an industrial society. This factor consists of the attitudes, policies, and skills of employers. I want to suggest that perhaps we ought to hope for and encourage the spread of a specific employer policy that nearly all of us are glad is dead and buried in this country; namely, "welfare capitalism" based on an attitude of paternalism. This implies company housing, company schools, company doctors, company stores and banks (preferably co-ops, as is common in India)—indeed, whole company towns. I would even go so far as to suggest that company unions ought to be formed where no other unions exist.

The spread of such paternalistic practices will bring abuses, of course; but

more often than not the alternative in many underdeveloped countries will be no—or intolerably bad—housing, no schools, no medical facilities, the abuses of the independent moneylender, and no unions. The absence of alternative social arrangements to provide such foundations for industrial communities quickly strikes one even in such a relatively advanced country as India. There would be great difficulties in execution, in potential and real abuses, in stimulating employer concern for such behavior, and possibly in straining scarce capital resources. But I wonder if welfare capitalism does not provide the best hope of minimizing the social disruption and individual anxieties that accompany the early stages of industrialization?

WILLIAM H. KNOWLES: I should like to raise one point in Wilbert Moore's paper, but one that is relevant to Bert Hoselitz' paper as well. It seems to me that Professor Moore too casually sets up a problem of which comes first: a value system which makes a dynamic market economy possible; or a market economy which brings about a compatible value system. When I observed in the West Indies both that natural resources were not being fully exploited and that profit-making opportunities were not being taken advantage of, I was initially impressed with the argument of social anthropologists that the value system obstructed economic development. Further study, however, has caused me to reverse my stand and to believe that social anthropologists have overstressed resistance to economic progress. Instead, I would now argue that the act of setting into motion of technological change will necessarily bring about rapid cultural change.

First, the concept of society in equilibrium is as unrealistic as is the notion of an economy in equilibrium, and both economists and anthropologists are sometimes guilty of confusing their theoretical models with the real world. Change has been taking place in the cultures of underdeveloped areas ever since the arrival of the first explorer, trader, and missionary. The fact is that the societies of the underdeveloped areas are presently in an advanced stage of disintegration and this instability is the very reason why we are concerned with the economic development of such underdeveloped areas. (Dewight Cooke, *There Is No Asia* [Doubleday, 1945], pages 261-262.) The fact that these societies do not seem to be resistant to change by communist infiltration makes the real issue the question of what kind of value system will replace the old.

Second, I believe that the social anthropologists are in error in their observation that what the economist calls rational economic behavior is merely a value judgment and varies from one society to another. It is true that the West Indian peasant behaves rationally when he plants the smallest possible garden close to his door because he knows his neighbors will steal his produce. It is also rational for him to steal from his neighbor's garden. But the peasant community collectively is behaving irrationally when it suffers hunger because of such a "value system." A more likely anthropological term for such irrational economic behavior was used by Madeleine Kerr when she described the behavior of Jamaican peasant communities as pathological. (Madeleine

Kerr, *Personality and Conflict in Jamaica* [Liverpool: University of Liverpool Press, 1952].)

Third, I believe that the problem, in the West Indies at least, is more nearly one of getting a stalled or stagnate economy moving in the right direction than it is one of value systems. Professor Moore pointed out that a vicious circle often develops when employers pay low wages because productivity of labor is so low, and when employees respond by doing as little work as possible because wages are low. Both employers and employees may be considered to be acting rationally, but the resulting low standard of living leads to unstable political conditions of revolutionary proportions. Such a vicious circle is as much a problem in economics as it is one of value systems. From my research in the British West Indies, I would like to list a few specific economic problems to explain attitudes towards work and the resulting low level of economic activity.

Lack of Effective Merchandising. Businessmen do not attempt to create new consumer desires because workers lack purchasing power, and similarly labor is not motivated to work because they do not have unfilled wants. To explain this the social anthropologist concludes that man's wants are not insatiable as assumed by economists. I say that American merchandising methods can make man's wants insatiable even as they are in the United States. An American timber operator in British Honduras explained this to me by saying that industrial discipline is lacking there because the workers were not "hooked." By contrast, workers in Panama have become efficient because they are now hooked, he said. They were caught, he explained, because their wives learned to demand nylon panties and workers had to earn the money to buy such goods. On the other hand, he had just returned from his first visit in twenty years to his home town of Los Angeles and he was shocked by how hard people worked and how much they wanted. They were hooked too completely, he had decided, by American merchandising methods.

Lack of Effective Management. Labor cannot be highly motivated where it is not trained, where supervision is poor, where work standards are slack, and where workers are treated as inferior beings. Low productivity proves to management that workers are indeed inferior and it is therefore futile to attempt modern personnel practices. This vicious circle may be rational or may be part of the culture's value system, but the report of many West Indian workers to the effect that when the boss treated them as though they were dull they played dumb, makes more sense to me.

End of Racial Discrimination. When opportunity for advancement is denied on racial grounds and when wage differentials are based upon color, labor not only fails to be motivated but also indulges in mild forms of sabotage. In this case, however, it is the irrational value system of white Europeans and North Americans that causes the people of underdeveloped areas to resist industrialization and urbanization. There is no color bar in peasant villages and resistance to industrialization may be more of a resistance to undignified treatment common to urban areas than resistance to work or lack of interest in a higher standard of living.

Lack of Capital Equipment. Labor in the West Indies associates work with slavery when it is menial work done with cutlass and hoe. A study by Simon Rottenberg of labor attitudes in Antigua showed that there could be general underemployment while at the same time there was a labor shortage in the cane fields. (Simon Rottenberg and Nora Siffleet, *Unemployment in Antigua* [Labor Relations Institute, University of Puerto Rico, 1952].) Laborers are reluctant to demean themselves by wielding a hoe when they would welcome the chance to drive a tractor. Labor is motivated to work where capital equipment gives relief from backbreaking physical labor and will accept the improved dignity and status that machine operated jobs give over that of field hands.

Substantial Wage Increases for Increased Productivity. It is true that small wage increases tend to reduce the supply of labor through greater absenteeism, but sizable wage increases serve as an inducement to break with the past and assume lower middle-class virtues of hard work, thrift, sobriety, and ambition—exposing the hook to the bait. Professor Hoselitz' description of city dwellers of Asia who failed to acquire the urban value system known in Europe and North American cities was also describing workers whose wages are very low and who are chronically underemployed. Labor prefers work to the leisure of tribal or village life if the compensation is large enough to permit them to assume the urban value system.

Lack of Full Employment. The neoclassical model outlined by Professor Moore also assumes full employment, and behavior patterns are different where there is chronic unemployment. Lack of ambition is frequently lack of job opportunities, and laziness is often an unofficial share-the-work program. My colleague, Professor Anthony Koo, reports that women employees of a new textile mill in a Chinese village quickly broke from parental authority, refused to marry the husbands that had been chosen for them, and demanded that the village stores carry cosmetics and Western clothing. As we have learned in the United States in the transition from depression to prosperity, there is nothing like a job and purchasing power to change a person's values.

Poor Health and Diet. Taking issue with Professor Hoselitz, I believe that an industrial reserve army does exist in underdeveloped areas if workers are made healthy and given a blanced diet. Professor Reynolds' example of the happy Bantu whose value system caused him to prefer leisure to work is probably idyllically descriptive of the fact that the Bantu is suffering from malaria, tape worms, and malnutrition. Thus the Bantu value system is probably based upon physical inability to work. The Republic Steel Corporation was able to exploit the rich iron ore deposits of Liberia, where other companies had failed, by first restoring the native workers to health and then giving the proper food and housing.

Stated in these terms, the worker's attitude toward industrialization and urbanization is not so much a problem of value systems as one of vicious circles which lead to irrational behavior for society as a whole and, in turn, to revolutionary upheavals. In his presidential address before the Industrial Relations Research Association, Clark Kerr stated, correctly I think, that the revolution of the peoples of the world was in favor of industrialization and this creates problems in the rearrangement of social structures. We have the

know-how to bring about the industrialization and we have the merchandising and personnel techniques to overcome the economic stalemates listed above. Our central problem is to keep the manpower and resources of underdeveloped areas from succumbing to communism. The creation of jobs, purchasing power, and a desire for a higher standard of living can be our chief weapons in the fight. Let us proceed with plans for industrialization and the value systems will take care of themselves.

SIMON ROTTENBERG: Messrs. Hoselitz and Moore have written perceptive papers which raise meaningful questions and deal with them meaningfully. In this discussion, I shall not speak of the majority parts with which I agree, but only of the minority parts with which I take issue.

As Mr. Hoselitz correctly says, growth can, and has, occurred in other ways than through industrialization. I think it is true that economies have grown almost everywhere in the world in the last half century. Almost everywhere, income per head of population is larger than it was fifty years ago, and this is true despite the increase in numbers that has also occurred almost everywhere. But industrialization—if by this we mean manufacturing industry—has not occurred everywhere. If people are, on an average, better off now in the Rhodesias, it is in large part because of copper and tobacco; in the Gold Coast, coca; in Assam, tea; in Malaya, rubber and tin; in Ecuador, bananas; in Venezuela, oil; in Colombia, coffee; in Cuba, sugar; in Iowa, corn and hogs; and in Florida, tourism and citrus fruits.

The number of cases in which per capita income increments have been achieved through agriculture, mining, and other nonmanufacturing forms of economic activity is very large.

It is significant, also, that that within the last several decades, in the United States and in Europe, some cities have become "too big." As diseconomies of scale were encountered, entrepreneurs making locational decisions have tended to choose metropolitan area fringes in preference to their centers and, to some extent, small places to large. In the process of locational trial and error, skills have been learned in the administration of decentralized manufacturing that may result in the long run in less congested industrial areas in the poor countries of the world. For both of these reasons, discussion of the functioning of cities in relationship to growth covers less than the whole growth story.

I turn now to the partial case in which growth does occur through the proliferation of factories and where the factories are concentrated in cities.

Mr. Hoselitz says that disproportionate numbers of rural people are attracted to these cities in the non-West, relative to the opportunities that await them there and that these excessive numbers consitute a *lumpenproletariat* but not a reserve (and presumably efficient) industrial army available to new factories.

We need to ask ourselves here: what is meant by opportunity? People move from place to place in any society for many and diverse reasons. The causes of movement are uniform neither among societies nor among individuals within given societies. But it is uniform among both societies and individuals

that movement occurs because people believe they will be better off if they do move. We are compelled to come to this judgment, if we reject the only two alternative propositions: that people make migrational decisions randomly (as by deciding by the turn of a coin) or that they locate where they believe they will be worse off. I think neither of these is a priori tenable, either for Nigeria or for Tennessee. I have said that people's decisions are consistent with the belief that they will be better off. This does not mean that mistakes are not made. They are; but in the end mistakes are found out and new decisions correct errors in old.

This suggests that, all things considered, if there is movement to the cities from rural areas in the non-West, it will not be larger than "warranted." The incidence of unemployment and mortality in the cities may be high and earnings may be low, but either, even at these levels, conditions are better than in the country or else conditions are worse, but people are willing to pay a price in less opportunity for the specialized services and "glamor" which are available in the cities but not in the country.

Multidimensional motivations for cityward migration characterize movement in both the West and non-West. In both, opportunity-for-income differentials and opportunity-for-nonincome differentials play a part. Can we say which of these two has the heavier influence in each, the West and the non-West?

Suppose we accept, as I think we can, the evidence of both papers that non-Western people are psychologically so thoroughly committed to kin, tribe, and village that they trek to the cities only with very great reluctance, while, in the West, the psychological impediments to movement are less formidable. Then we are led to the conclusion that differences in incomes between the cities and the country must be larger in the case of the non-West to induce movement to cities and, from this, to the corollary that numbers moving cityward there are, if anything, less rather than more than warranted, measured by the criteria of growth and efficiency.

Non-Western city slum dwellers may be poor raw material for the factories, but it is for other reasons than that too many have come to the cities. Relative marginal products of labor in city and country are such that the removal of a proportion of present city dwellers to the country can be expected to diminish total output, not increase it.

In one sense, Mr. Hoselitz is right to say that the learning of new skills by primitives and rural people is done quickly. In the absence of contrary evidence, we can, I think, accept the proposition that the genetic capacity to acquire knowledge is about equally distributed among countries. Where the extent of the market makes the division of labor consistent with cost-minimizing choicing within and among firms, the problem of skill learning even diminishes in magnitude, because simple operations are more quickly learned than complex ones and specialization tends to result in simplification.

I have said that the genetic capacity to acquire knowledge seems to be equally distributed in the world. What Simon Kuznets has called the "*social* capacity" to acquire knowledge, however, is unequally distributed. Some societies are characterized by the experimental acceptance of knowledge, by an

empirical outlook, by skepticism, and by change. Others are characterized by sanctification of knowledge, by a mystical outlook, by traditionalism, and by changelessness. In the former, new knowledge is easily accepted when it proves its worth; old knowledge is easily abandoned when it becomes obsolete. In the latter, new knowledge must struggle to make headway against customarily-verified old knowledge. It is this unequal distribution of the social capacity to acquire knowledge which explains, in large part, why new technology diffuses so much more rapidly among Iowans than among the Bantu. In this sense, adjustment to technology does seem to be something of a problem. Adjustment to technology has still another facet which magnifies the problem. If economies are to grow, knowledge must be used as well as acquired; but the use of new knowledge almost always disadvantages those who use old knowledge. The survival of old firms and old jobholders is threatened by new ones. In economies experiencing relatively rapid rates of growth, old techniques are willing, more or less, to let the market decide which shall pre-empt the field. In relatively stagnant economies, on the other hand, old techniques are often buttressed by public policies which protect old jobholders against new entrants, hold an umbrella over inefficiency, and encourage stagnation to feed upon itself.

In the end, looked at from these vantage points, I think that Mr. Hoselitz may have underestimated the seriousness of the problem of technological adjustment in the poor countries. I think, on the other hand, that he may have overestimated, on net balance, the knottiness of the problem created by the persistence of traditional values in the non-Western cities.

The transmutation of behavior governed by some other rule to behavior governed by the rule of efficiency is everywhere a problem when rural or preindustrial people are taken into the factories. But somehow this transmutation occurs. The economic history of the United States is studded with examples of it. It happened first to the rural women of New England who took up work in the early cotton mills and later to successive waves of immigrants from Europe who somehow accommodated imported values and standards to the requirements of competitive survival in a rational economy in which the market disciplined behavior.

In many respects, our cities were also aggregates of distinct clusters, simulating, in some ways, the cities of Africa and the East of which Mr. Hoselitz speaks. In these clusters, the old standards persisted even into the first generation of American-born of immigrant parents, but efficiency on the job seemed to be achieved, nonetheless. To discuss how this accommodation occurred takes us some distance away from economics and I am sure that I am not competent to pursue it. I venture the guess, however, that the immigrants found it possible to live diverse lives by diverse standards, so that in their homes and neighborhoods they obeyed (perhaps incompetent) elders and on their jobs they obeyed (perhaps young) competents.

I do not want to appear to question Mr. Hoselitz' central thesis that our cities are more efficient producers of rational human animals whose choices among alternatives serve the interests of output and individual gain. I should think, however, that patterns of diversity of values similar to ours can be

found to exist in the non-Western cities, too, and that the factories there breed, for themselves, individualization of contract, hierarchy oriented to merit, and the other structures and values of which Mr. Hoselitz speaks which are the conditions of efficiency. This has, at least, been the experience of the poor countries of the Western Hemisphere where the cities and towns have played a primary role in converting indigenous human raw material characterized by very profound adherence to tradition into a stock of human resources appropriate to economic growth.

I think that some of the detail of Mr. Moore's decription of employment choicing in the poor countries is arguable, but I do not propose to argue it here. His description is closely proximate to truth; in some average sense, in the poor countries people are less mobile and less responsive to price incentives than in the rich countries. Strong loyalties to kin and tribe, low aspiration levels, devaluation of new occupations, and so on cause people to be willing to pay a price for traditional structures and systems.

Certain propositions can be appropriately drawn from this evidence. Other things equal, a community which aspires to consume a small volume of goods and services will produce less than one which aspires to consume a larger volume; a community that wants to consume much leisure will produce less than one that wants to consume little leisure; a community of immobile workers will produce less than a community of mobile workers; a community of workers who are indifferent to income will produce less than one of workers who prefer high income to low. But Mr. Moore goes beyond judgments of this kind, to say that choice patterns in the poor countries make economic theory a useless instrument for understanding the processes by which labor is allocated to uses there. This, I think, is wrong and I propose to devote the remainder of my discussion to this question.

The economic theory of the allocation of labor is an enormously versatile analytical tool. Supply and demand schedules are drawn to show functional relationships between quantity and price. But this does not say that theory assumes that quantity is exclusively responsive to price, nor that response is instantaneous, nor that it is uniform among workers, nor that small price changes will necessarily induce change in quantity.

Textbooks in economics carry graphic illustrations of supply curves of various degrees of slope and there is a long history of literature in the journals on kinked curves and their analytical consequences. If the Bantu prefer to stay on their reserves and if they go into the mines only reluctantly, the supply curve of labor to the mines will be more inelastic than if this were not so. Similarly, the preference of Pennsylvania coal miners for staying in depressed towns after coal has been mined out will tilt the curve upwards.

The number of different degrees of slope of supply curves of labor from infinite elasticity to infinite inelasticity is infinite and the number of different complexes of circumstance which can determine any single degree of slope in this set is also infinite.

The analytical apparatus is broad enough to encompass them all. The extreme case of infinite inelasticity of supply is one in which quantity is precisely completely irresponsive to price; theory comprehends even this case.

The individual supply curve of labor does not even have a persistent slope, but turns and twists in its course. The number of different shapes which it may have is also infinite. The diversity of shapes and slopes which theory permits occurs precisely because of differences among men in aspiration levels and in the evaluation of the income and nonincome properties of different occupations.

The labor supply schedule will be more inelastic if a given increase in the supply of labor can occur only with a large increase in marginal cost and if a given increase in the price of labor makes it worth while for only a small number of new workers to make themselves available for work. The components of cost and of the calculus of what is worth while are many and diverse and they include all of the considerations that Mr. Moore has said—incorrectly, I think—are outside the system of the theory of economics.

Economic theorists never said that relative prices alone determine in which employments people will be disposed to place themselves. When price is measured on the *y*-axis in conventional graphic analysis, the other components of jobs are implicit but not omitted; and theory is versatile enough to admit some other variable than price, either alone or in combination with price, to be inserted instead. If such a substitution should give better predictive results than analysis which functionally relates prices and quantity, then the change can and should be made, but the theory will have been substantially unchanged. Much of what Mr. Moore says about the nature of economic theory is, in the light of what I have just said, not correct.

Economic theory does not assume that men make choices among alternatives which are appropriate to the maximization of satisfactions that are "economically defined and measured," if by this is meant income and only income; nor that "human wants are . . . unlimited," if this means that men are not thought to reckon the cost in leisure foregone, effort spent, and other values sacrificed in satisfying wants; nor that " 'human nature' [is] pretty much the same wherever encountered"; nor that "financial incentives are . . . effective in allocating labor," if this means they do so to the exclusion of other incentives or, again, that costs are not reckoned; nor that workers everywhere are equally willing to work; nor that the preference for lower income occupations over higher income occupations is necessarily " 'irrational' or 'nonrational' as judged on economic grounds"; nor that "Western experience can be used as a guide to behavior in Dahomey," if this means that behavior is the same in both.

I do not want to put this case so strongly that the cause of the criticism of theory will be put out of sight. Theory says that people will be distributed among classes of work such that their marginal products are proportional to their prices. If this is not true at some time or place, then it will pay for people to move from one employment to another until it is true. If it is true, the system is in equilibrium and there is no incentive to move.

Now this is a solution to the problem of the allocation of labor which has a quality of elegance. Everyone knows that competitive equilibria are logical ideals; that no system of natural phenomena ever purely achieves these results; and that the pure theory expresses tendencies rather than describing

precisely what transpires in real worlds and real markets, either in the West or in the non-West.

Elegant solutions are derived because it is the nature of theory, in order to simplify analysis, to be highly abstract, to introduce only a few explicit variables, and to hold others constant or to randomize their effects. The usefulness of theory derives from the capacity of abstract, simple, and unrealistic models to yield results which are highly predictive of real phenomenal experience.

Much of the criticism of theory derives from the belief—which I think false—that simple models cannot be used to explain complex phenomena or from the failure to understand that results that are drawn from these models are only tendential.

From all of this, you will have seen that I dissent a little from Mr. Moore's conviction that economic theory has very limited utility for analysis of preindustrial labor markets. I think, on the contrary, that its utility there is about as great as in industrial labor markets. The insights of economists on behavior in different societies are not different from those of the sociologists-anthropologists. The differences are only in the subject matter of the disciplines.

The economist says: my insight is that Peruvian Indian labor is relatively inelastic to price. He proceeds to examine the factor price and resource allocational consequences of inelasticity, but only some of its causes, such as the cost of training, the scarcity or abundance of natural talent, and the restraints on competition. The sociologist-anthropologists says: my insight is that Peruvian Indian labor is relatively inelastic in price. He proceeds to examine other causes of inelasticity and finds them in values held, kinship systems, family structures, and residential patterns. Their studies are not competitive but complementary.

In any case, I think that the price elasticity of labor in the poor countries is not as low as the reiteration of impediments to mobility would make it seem to be. In all of these countries, there is evidence of movement to cities, factories, plantations, and mines, and from other employments where income is lower. This movement is consistent with the expectation of theory that, other things equal, men will prefer higher income opportunity to lower, and its corollary, that because it occurs, average productivity and average output per head will rise.

Just the other day, Robert Oppenheimer, seeing with profound poetic perception what is going on about him, said: "What is new in the world is the massive character of the dissolution and corruption of authority, in belief, in ritual. . . ." For this discussion, this means, it seems to me, that men now, almost everywhere, pursue higher material levels of life more cheaply in psychic costs than they once did.

MODELS OF ECONOMIC GROWTH

CAPITAL INTENSITY AND ECONOMIC GROWTH

By JOHN H. POWER
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The purpose of this paper is to explore the theoretical possibility of equilibrating adjustments in the capital coefficient in a growing economy. Professor Fellner has recently called attention to the fact that in projections of economic growth this possibility has been largely ignored, leaving to fiscal policy the whole of the problem of correcting any unbalance. Fellner argues that, in an automatically and fully adjusting economy, "the capital-output ratio would always assume the value corresponding to the postulate that all voluntary savings (plus whatever tax payments there are) must be absorbed by the planned private capital formation (plus whatever government expenditures are made). . . . We all know that the economy is not fully adjusting in this sense. But it should not be assumed that the economy is fully nonadjusting in the same sense."[1]

The first section of this paper sets the problem in terms of a crude growth model. It should be emphasized at the outset that this model is not offered as a general model of economic growth, but as a device for concentrating on particular questions in growth theory. In the second section we turn to a reconsideration of the theory of capital intensity. The results therein obtained are applied in the final section to the issue raised by Fellner. Some concluding remarks follow the final section. The analysis throughout pertains to a period longer than a business cycle; and for simplicity we consider a closed, private economy with only two factors: labor and capital.

I

The problem of stable growth is ordinarily posed as that of maintaining balance in the rates of growth of a few aggregates: total demand, the capital stock, and the labor force. The condition for stable growth in this somewhat restricted sense can be stated as follows: potential total output given by the existing labor force and capital stock must be matched at all times by the total demand for output given by

[1] William Fellner, "Long-Term Tendencies in Private Capital Formation," in *Long-Range Economic Projection*, Studies in Income and Wealth, Vol. 16, Conference on Research in Income and Wealth, a report of the National Bureau of Economic Research (Princeton University Press, 1954), pp. 301-302.

the saving function and capital formation, where the latter is just sufficient to equip increments to the labor force in the optimum ratio.

This is shown graphically in Figure 1.[2] L and dL represent, respectively, the labor force and its annual increment. S, I, and Y are net saving, net capital formation, and total net output, all per annum. The slope

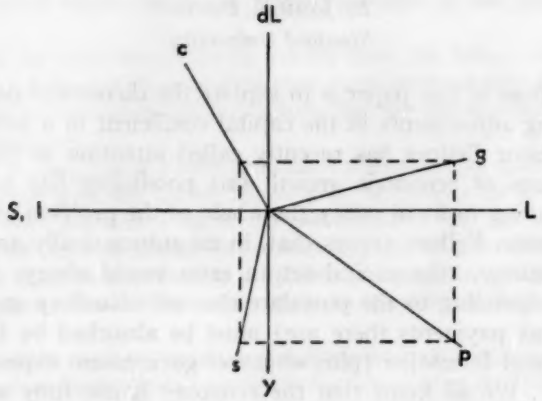

FIGURE 1

of the g function is simply the percentage growth of the labor force. The slope of the c function is the capital coefficient (here stated as a ratio of capital to labor instead of, as is usual, of capital to output). It is assumed that investment decisions are undertaken automatically to equip increments to the labor force in the optimum (most profitable) ratio of capital to labor as given by interest rates, wage rates, and the prices of current output. The stock of knowledge will initially be held constant, and the possibility that a changing pattern of demand will alter the optimum ratio of capital to labor for the economy as a whole will be ignored.

The s function is the saving-income relationship and, together with capital formation, gives the total demand for net output, Y_d. Finally, the labor productivity function, p, gives the relationship between the labor force and the total supply of net output, Y_s, for a given (optimum) capital stock. There is a balanced relationship among the aggregates if Y_d and Y_s meet on the Y axis (alternatively, if investment and saving meet on the S, I axis).

If, however, the rate of growth of the labor force were higher, Y_d would exceed Y_s, resulting in a chronic tendency toward inflation. At the same time there would be rising unemployment (though it might

[2] This diagrammatic representation was suggested to me by Paul J. Wells, of Stanford University.

be "disguised") since a part of each increment to the labor force would fail to be supplied with capital equipment (assuming that forced saving does not equal the total gap between Y_d and Y_s; i.e., that real investment suffers as well as real consumption as a result of the inadequacy of total output).

When, on the other hand, the rate of growth of the labor force is insufficient to require capital formation equal to the flow of saving, Y_d falls short of Y_s and part of the existing labor force is unemployed. If no adjustment occurs in the functions, unemployment grows over time. In the first case, we have chronic inflation with growing unemployment; in the second, we have chronic depression with growing unemployment.

In a general theory of economic growth, we could not simply take it for granted that the atomistic decisions of individual entrepreneurs would provide all increments to the labor force with equipment in the optimum ratio. Indeed, this might constitute a major part of the adjustment problem in an automatically adjusting economy. But an adjustment in the capital coefficient is not the appropriate remedy when unbalanced growth results from a failure of capital widening to proceed at the rate warranted by the growth of the labor force. If, for example, in an otherwise balanced situation, the actual rate of widening fell short of the warranted rate, resulting in a chronic deficiency of demand, a rise in the capital-labor ratio would reduce the deficiency of demand without eliminating growing unemployment. The problem is not solved, that is, by adopting production techniques which raise the ratio at which only a part of each increment to the labor force is being equipped with capital. Thus, the question of appropriate adjustments in the capital coefficient arises only when the adjustment of capital widening to the warranted rate has been accomplished (by automatic forces or government policies). But the question does arise in this case, for the warranted rate may exceed or fall short of saving.

Again, in order to concentrate on adjustments in the capital coefficient, we rule out Malthusian adjustments of population growth to wage changes and classical adjustments of saving to changes in interest rates. The rate of growth of population and thriftiness are the independent variables in the model.

Thus the issue comes down to this: will there occur automatically equilibrating adjustments in the capital coefficient when the growth of the labor force is not consistent with the flow of saving? A certain growth in the labor force will require a certain increase in equipment at the optimum ratio if the new labor is to be employed. This net investment may or may not equal the saving out of the income corresponding to the output produced by the existing (fully employed)

labor force and capital stock. If investment exceeds saving, a fall in the marginal capital-labor ratio would be equilibrating in the sense that it would reduce the excess investment. In the opposite case a rise in the ratio would be equilibrating.

There is a further complication, however. The relation between the labor force and potential output (the p function in Figure 1) is not independent of the optimum capital-labor ratio. Given time, a change in the optimum ratio will affect not only increments to the labor force, but all of the previously existing labor force as well.[3] This means that when the optimum capital-labor ratio adjusts in an equilibrating fashion, the labor productivity function moves in a direction which is disequilibrating! In the case of deficient demand, for example, a rise in the marginal capital-labor ratio will raise investment and total demand; but as the new optimum ratio is gradually adopted throughout the economy the potential output of any level of the (fully employed) labor force is also increased. Only if demand rises more than supply will the adjustment be equilibrating.

Fortunately, however, we can expect the change in demand ordinarily to exceed the change in full employment output, so that, if the shift in the optimum capital coefficient alters investment in the right way, the perverse movement of labor productivity will not prevent the gap between total supply and total demand from being narrowed. This can be demonstrated by considering the case where the optimum ratio of capital to labor is doubled. This will double investment and total demand, but labor productivity will rise in a smaller proportion, provided only that the marginal productivity of capital is diminishing. Furthermore, since we are concerned with net output, we must take note of the fact that the proportion of the labor force engaged in replacement of capital will be higher with a higher capital coefficient. Thus, the rise in net output per worker will be smaller than the rise in gross output per worker.

It is possible, of course, that with the rise in real income per capita the ratio of saving to income will rise and total demand will fail to increase in the same proportion as investment. Offsetting this influence on the saving function, however, will be the change in income distribution resulting from the market forces (e.g., a rise in wage rates or a fall in interest rates) required to adjust the capital coefficient in an equilibrating fashion. For the prospect that labor's share in income will rise is enhanced by the perverse movement in labor productivity, requiring, as it does, a greater rise in wage rates (or fall in interest rates) to eliminate a given amount of unemployment.

[3] It is assumed for simplicity that this adjustment takes place as old equipment is completely amortized. The problems arising from a lack of correspondence between amortization and replacement are ignored in this paper.

On balance, then, it does not seem likely that the shift in labor productivity could be great enough to upset a tendency for the total adjustment to be equilibrating if the shift in the capital coefficient alters investment in the appropriate direction. Hence we will proceed on this assumption. Nevertheless, it should be clear that the tendency will be considerably weakened by the perverse movement of labor productivity.

We have yet to consider, however, just what modifications in the optimum capital coefficient will result from the market forces accompanying unbalanced growth. We turn now to a reconsideration of the theory of capital intensity for the answer.

II

One important result of the capital theory discussions of the thirties was the recognition that the influence of interest rates, wage rates, and prices on capital intensity depends on the nature of the limit to the firm's size. (Wage rates and prices refer to those expected over the lifetime of the investment.) It had theretofore been generally assumed that either a rise in the rate of interest or a fall in real wages, or both, would always reduce capital intensity, while changes in the opposite direction would raise it.

Nicholas Kaldor was perhaps the first to point out that interest rates will be the sole determinant of the firm's capital intensity when its size is limited by the market for its output; but when the supply of capital funds is the limit to expansion, real wages determine its capital intensity and the rate of interest has no influence. ("Capital Intensity and the Trade Cycle," *Economica,* February, 1939.) Unfortunately, Kaldor's results depend on the assumption of complete vertical integration in processes of production. (The assumption of complete integration is a natural pitfall for those who think of capital intensity as an average period of production. This makes Kaldor's adoption of this assumption even more surprising, however, for he explicitly rejected the period of production as a measure of capital intensity.) Furthermore, he was concerned with the capital intensity of the firm, not that of the economy. Changes in the composition of firms can change the over-all capital intensity even when there is no incentive for individual firms to alter their techniques. We shall proceed on the assumption that firms are nonintegrated (i.e., that they buy equipment from other firms and in addition only current labor). It is assumed that the services of equipment and current labor are the only inputs. And we shall take into account the effects on the economy's capital intensity of changes in the composition of firms. Finally, investment is assumed to be a continuous, synchronized process. (Cf. Friedrick and Vera Lutz, *The*

Theory of Investment of the Firm [Princeton University Press, 1951], page 33.)

We shall consider three limits to the size of the firm: sales, labor supply, and capital funds.[4] These, together with four market forces—changes in sales prices, wage rates, interest rates, and equipment prices—give us twelve possible cases for the firm which can be analyzed according to the following three rules: (1) profit maximization implies maximizing profit per unit of the limiting factor ("factors" include labor, capital funds, equipment, and sales); (2) a change in the price of a limiting factor will alter profit per unit of that factor equally for all methods, and hence optimum capital intensity is unaffected; (3) in all other cases, a change in the price of a nonlimiting factor will alter profit per unit of the limiting factor more for those methods which use relatively more of the factor whose price has changed.

All twelve cases are shown in Figure 2, where a zero indicates no change in capital intensity and a plus or a minus indicates, respectively, a change in capital intensity in the same or opposite direction to the change in the price of the factor.

Changes	*Limits*		
	Sales	Labor	Capital Funds
Sales Prices	0	+	−
Wage Rates	+	0	+
Interest Rates	−	−	0
Equipment Prices	−	−	0

FIGURE 2

If, for example, sales are the limiting factor, a change in sales prices will affect profit per unit of sales on all methods equally and cannot change their order of profitability. A change in wages, however, will alter profit per unit of sales (inversely) by a greater amount for those methods using relatively more labor. A rise in wage rates will, then, raise the optimum capital intensity. A change in interest rates or in

[4] Entrepreneurship has sometimes been suggested as the limit to the firm's size. For an effective argument to the contrary, see Tibor Scitovsky, *Welfare and Competition* (Irwin, 1951), pp. 190-193.

equipment prices will affect profit per unit of sales (inversely) more for techniques using relatively more capital. Thus a rise in interest rates (or equipment prices) will reduce capital intensity. The other cases can be worked out in similar fashion.

The crucial question is, then, what limits the size of the firm? This is not necessarily the same thing as asking what limits the growth of the economy. And it is fortunate for the prospect of automatic adjustment that it is not. For if labor supply limited the growth of the economy, for example, the resulting relative rise in wage rates would not induce a change to laborsaving methods if labor were also the limit to the size of the individual firm. The limit to the size of the firm depends, instead, on the imperfections in its product, factor, and capital markets. With no market imperfections there would be no visible limit to the firm's size (assuming proportional returns to scale).

Conceivably the supply of labor could serve as the limiting factor, particularly if the firm required a very specialized kind of labor. These cases are not likely to be quantitatively important, however, in a developed economy. Moreover, both kinds of unbalanced growth in our model are characterized by growing unemployment, ruling out a general shortage of ordinary labor. Nevertheless, for the less developed economies, the growth of some forms of enterprise may be checked by a shortage of skilled labor. Since this kind of economy is also likely to exhibit a deficiency of saving relative to the requirements of population growth, the case of labor supply as a limit might be appropriate in considering the adjustment to the excess demand type of unbalance. Due to limitations of space, however, we can treat adequately only the adjustment in the deficiency of demand case. Here, it will be assumed, the labor limit plays no important role.

The argument for capital funds as the limit to expansion of the firm depends heavily on the existence of uncertainty and the accompanying borrower's and lender's risk. Risk of bankruptcy and of wide fluctuations in earnings limits the ratio of borrowed capital to entrepreneurial capital from the borrower's side. Fear of loss of control limits the offers of new shares also to some proportion of entrepreneurial capital. From the lender's side, the insistence on reasonable security against the loan again limits the ability to borrow to some proportion of the firm's net worth. The availability of capital funds, then, is limited ultimately by the entrepreneur's own capital and this limits the size of the firm.

Some objections can be raised to this argument. First, uncertainty may be more specifically oriented than is often assumed. Instead of a vague, general timidity, uncertainty may reflect doubt about some particular aspect of the calculus involved in the investment decision.

If the doubt concerns some aspects of the expected performance of one method of production only, the risk premium should be deducted from the expected profits associated with that technique instead of being considered as affecting the supply of capital funds. Or, if the uncertainty applied equally to all techniques but is specifically uncertainty about the future volume of sales, this is properly thought of as a sales limitation on the firm's size. For capital expenditures which reduce the cost of producing a given output will not be impeded by uncertainty of this kind. There will undoubtedly remain, however, an important element of general uncertainty which cannot be related to any particular aspect of the investment decision; and this will be reflected in a reluctance to expand total investment beyond the limit imposed by entrepreneurial capital.

Still, there are good reasons for believing that the sales limit is more important in our economy, at least insofar as the capital-intensity decision is concerned. The large firms (which weigh most heavily in total investment) are fully aware of the obstacles to expanding their market shares and will often simply consider their growth as given by the growth of the market as a whole. Furthermore, they are usually in advantageous positions in the capital markets due to the lower degree of risk which normally accompanies large size, to their relatively great accumulations of entrepreneurial capital, and to the relative ease with which the dominant group can maintain control when the number of equity shares is increased.

Even with small firms, market situations where retaliation is expected are probably more prevalent than is generally assumed. Among many small firms selling similar products there will be local ("local" in the sense of product similarity as well as in the geographical sense) concentrations of more intense rivalry among a few. The sales limit seems, then, to be quite general in our economy, and we shall assume in the analysis that follows that this limit predominates.

So far we have considered only the capital intensity of the firm. But the average capital intensity of the economy can be affected by changes in the composition of firms even when there is no incentive to change the capital intensity of any single firm.

Insofar as different industries have different capital intensities,[5] market forces will affect their profits in varying degrees—but profits per unit of what? It is usually assumed that it is capital which moves to where its profit per unit is greatest, but this is correct only when

[5] We assume that all firms in the same industry are employing the some methods. This assumption is not essential, however. We could simply redefine an industry to include all those firms employing techniques of the same capital intensity, whatever they are producing. Then, general market forces will affect profits alike for all firms in the industry. The argument would proceed from there in exactly the same way.

capital funds represent the limit to the scope of the enterprise (because capital funds are limited by entrepreneurial capital). In the general case, it is the entrepreneur who moves from industry to industry and the incentive is maximum profits per entrepreneur. Wage, price, and interest changes will affect profits more in industries which have, respectively, high ratios of labor per entrepreneur, output per entrepreneur, and capital per entrepreneur. But there is no necessary relationship between these ratios and the ratio of capital to labor. Relatively high ratios of capital per entrepreneur and labor per entrepreneur within the same firm are perfectly compatible. Therefore, there is no presumption that these shifts will affect capital intensity in any definite way.

This would not be true if the capital funds limit were predominant in the economy. Since the capital funds limit is related directly to the size of entrepreneurial capital, maximizing profit per entrepreneur is equivalent to maximizing profit per unit of capital. Thus, whereas a rise in interest rates (or equipment prices) will not influence the optimum capital intensity for existing firms, it will shift enterprise away from those industries which have high ratios of capital to labor. Likewise, a rise in prices will pull enterprise toward those industries with high output-capital ratios; while a rise in wage rates will shift enterprise away from industries with high ratios of labor to capital. The necessary amendments to the table in Figure 2 can be accomplished by adding a minus wherever a minus or zero occurs in the third (capital funds) column and a plus where the plus occurs.

III

We are now ready to apply our theory of capital intensity to the case of chronic deficiency of demand. The first step is to establish what changes in prices, wage rates, and interest rates will occur when the growth of the labor force is insufficient to warrant capital widening equal to the flow of saving.

The unemployment due to the deficiency of demand will initially be accompanied by a redundancy of part of the capital stock. This will tend to reduce widening below the warranted rate and initiate a business cycle downswing. When the cyclical movement is reversed (for any of a number of familiar reasons), the pool of unemployed will permit capital widening to proceed in excess of the warranted rate in the upswing. Provided that the functions in Figure 1 remain unchanged, however, the fluctuations will tend to occur about an average trend as given by our crude growth model.

The only persistent element of disequilibrium in this situation is the growing (average) excess supply of labor. This means that wage

rates are likely to fall in relation to prices. The net effect of this on optimum capital intensity is likely to be disequilibrating (assuming sales to be the predominant limit to the firm's size; see Figure 2).[6] Only if interest rates fall sufficiently to overcome the perverse movement of real wage rates will the tendency for the capital coefficient to fall be absent. The "natural" movement of interest rates in this situation depends on what assumptions are made about monetary policy, which means that, in effect, there is no "natural" tendency in a state of chronic underemployment. In any case there are familiar reasons for supposing that beyond a point interest rates can be forced down only very slowly (at least with orthodox central bank techniques).

The tendency for adjustment in the capital coefficient in this situation to be disequilibrating results simply from the fact that a failure of the labor force to grow rapidly enough results (paradoxically) not in a labor shortage but in unemployment. Instead of rising real wage rates and the adoption of laborsaving techniques when the rate of growth of the labor force is too low in relation to the saving function, real wage rates fall and (in the absence of a sufficient reduction in interest rates) the capital coefficient adjusts in such a way as to widen the gap between saving and investment.

Though space limitations preclude an adequate treatment of the excess demand type of unbalance, it should be clear that here, too, real wage rates will tend to fall (as a result of chronic inflation accompanied by rising unemployment). In this case, however, the effect on the capital coefficient will be equilibrating (and will probably be reinforced by a rise in interest rates).

Before concluding, we should consider very briefly how the argument would be affected by relaxing the assumption of a constant state of knowledge. With growth of knowledge, labor productivity will rise (except as offset by diminishing returns due to fixed natural resources) and the optimum capital intensity may change in either direction or not at all, depending on whether the new, more profitable techniques use relatively more or less capital per worker than the previous optimum ratio. If we assume no population growth, the force behind expansion will be the labor freed as productivity rises. That is, over any period of time the labor force required to produce the average output of the period will diminish with the rise in labor productivity. If this freed labor is equipped with capital in the optimum ratio, causing capital widening just equal to the saving forthcoming from the income corresponding to full employment, there is balanced growth. This

[6] If wages and prices move in the same direction and in the same proportion, the effects on capital intensity exactly offset in this case.

freeing of labor can be represented by dL in Figure 1, and the problem of adjustment to unbalance is formally the same.

IV

Our conclusions can be summarized briefly:

1. Equilibrating adjustments in the optimum capital coefficient will normally be partially offset by disequilibrating movements in the labor productivity function; hence the market forces requisite to induce such an adjustment must be considerably stronger than would otherwise be the case.

2. However, given our assumptions (see 4 below), there is no natural tendency for the capital coefficient to adjust in an equilibrating way in the case of chronic deficiency of demand.

3. A situation favorable to automatic adjustment of the equilibrating kind in the deficiency of demand case would be inflexibility of money wage rates for downward (but not upward) movements combined with an easy money policy. An alternative would be a fiscal and monetary policy designed to promote capital widening in excess of the warranted rate; i.e., an overfull employment policy. For then growing labor scarcity would tend to induce shifts to laborsaving techniques (provided that interest rates were held down).

4. Our results depend on two important assumptions relevant to the theory of capital intensity: that firms are not vertically integrated and that sales represent the general limit to the size of the firm. If firms are vertically integrated, changes in real wage rates will not affect the optimum capital intensity for the firm in the sales limit case. (See Kaldor, *op. cit.*, pages 49-51.) If the supply of capital funds is the general limit, the results depend on the direction of movement of the over-all wage-price level as well as on relative wage-price changes and interest rates.

AN ECONOMETRIC MODEL OF GROWTH
U. S. A. 1869-1953

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I

In the last generation we have seen theories of economic growth cast in mathematical form. Since the publication of the work of Professor Kuznets and Dr. Goldsmith, we are supplied with long-term statistics as well. I am referring to their contributions in the 1952 volume entitled, *Income and Wealth in the United States, Trends and Structure*. Mathematical theories plus statistical data invite econometric research.

This model adds to the short list of long-term estimating models (other such: Tinbergen, Kalecki). It is the first to employ simultaneous estimating techniques. It has been extrapolated one period with satisfactory results; it can be used to test certain economic hypotheses and to derive the dynamic properties of the American economy.

Summary of the Model. Table 1 lists the variables and definitional identities. Table 2 shows the estimated structural relations. There are twelve structural relations and eight identities and twenty endogenous and seven exogenous variables. A brief picture of the economy follows: (1) Consumers' (including government) and producers' demand outputs. (2) Manpower-producing families and capital accumulators supply the factors of production. (3) Labor, entrepreneurs, landlords, and the financial sector claim income shares. (4) The factors of production are transmuted into the outputs demanded by means of a production function. (5) The actual factor-blend used in production depends on relative costs in the manner shown by a "substitution function." (6) The distribution of income is attained by bargaining and held within limits by occupational mobility. New manpower flows so as to prevent too great a disparity of average per capita income from one occupation to another. (7) To the real flow corresponds a counterflow of money, whose quantity is controlled by the government and the financial sector. Money influences the price level and this in turn affects distribution. The degree of activity (or wealth) and the quantity of money together determine the interest rate, which in turn affects capital accumulation.

The model derives its dynamics from lags (see Table 2, equations 4 and 8), but also from the use of time as a variable (equation 6). Other dynamizing devices are found in the cumulative nature of the

variables K and J (they are defined in Table 1, bottom) and also in the generous use of relative variables; for instance, those with K or J in the denominator.

Problems Specific to Long-term Econometric Models. Over and above the usual difficulties of econometric investigations, the building of long-term models presents special difficulties.

TABLE 1

LIST OF VARIABLES AND IDENTITIES

(All units are in annual averages for the decade, unless otherwise indicated. Stock variables are as of the midpoint of the decade.)

Endogenous Variables		Units	Endogenous Variables		Units
b	Birth rate	Number per 1,000 of population	w	Money wage rate	1929¢ per hour
C	Consumption	b—1929—$	Y	Gross real output	b—1929—$
D	Depreciation	b—1929—$	e	Labor input	billions of man-hours per year
E	% of N employed	full employment = 100		**Exogenous Variables**	
F	Financial income	b—1929—$	d	Death rate	number per 1,000 of population
I	Gross investment	b—1929—$	h	Standard hours	hours per week
i	Long-term interest rate	% per year	L	Value of land	b—1929—$
J	Population	millions	M	Money	billions of $
J^*	Population over 15	millions	m	Net immigration	1,000s per year
K	Stock of capital	b—1929—$	t	Time	1869–78 = 1
N	Labor force	millions			1874–83 = 2 &c
N_E	Entrepreneurs	millions	U	% of N_W unionized	per cent
N_W	Wage earners	millions		**Error Terms**	
p	General price index	1929 = 100	u or $u_1, \cdots u_{11}$	random shocks	
Π	Entrepreneur income	b—1929—$			
R	Rental income	b—1929—$			
W	Real wage bill	b—1929—$			

Identities

(I 1)	Output	$Y = C + I + D$
(I 2)	Income	$Y - D = W + \Pi + R + F$
(I 3)	Capital	$K = K_{-2} + 10(I - D)_{-1}$
(I 4)	Population	$J = J_{-2} + .01(b - d)_{-1} \cdot J_{-2} + .001 m_{-1}$
(I 5)	Labor force	$N = N_W + N_E$
(I 6)	Money wage rate	$w = Wp/52EhN_W$
(I 7)	Labor input	$e = 52 \times 10^{-6} EhN$
(I 8)	Population 15+	$J^* = J_{-3}(1 - .01 \cdot d_{-3})(1 - .01 \cdot d_{-2})(1 - .01 \cdot d_{-1}) + .01 \cdot m_{-2}(1 - .01 \cdot d_{-2})(1 - .01 \cdot d_{-1}) + .01 \cdot m_{-1}(1 - .01 \cdot d_{-1})$

The present state of our historical statistics compels us to build long-term models of rather frugal proportions. A model should involve a small number of variables, because several of the available statistical series rely on one another—this being particularly true of the earlier years. Too many interdependently constructed series produce fictitious

TABLE 2

STRUCTURAL RELATIONS
PARAMETER ESTIMATES*

Production.....	1	$Y=0.788e+0.29K+u_1$ (0.013) (0.006)
(L.SQ).....	1′	$Y=.374e^{0.2488}K^{0.6995}u_1$
Substitution.....	2	$e=112.32+0.6396K-327(w/p)+u_2$ (21.84) (0.1508) (122)
Depreciation.....	3	$D=0.081+0.035K+u_3$ (0.18) (00.01)
Consumption.....	4	$C/J=-0.034+0.6997(Y-D)/J+0.311(C/J)_{-1}+u_4$ (0.011) (0.057) (0.069)
Investment.....	5	$100\frac{I}{K}=-1.6+0.71\left(100\frac{Y-W}{K}\right)-0.54_i+u_5$ (0.96) (0.08) (0.30)
(L.SQ).....	5′	$\frac{I}{K_{act}}=6.66+7.00\frac{K_{req}-K_{act}}{K_{act}}+u$
Birth rate.....	6	$b=33.67-1.175t+1.07\frac{(Y-D)/J_{-1}}{(Y-D)/J_{-2}}+u_6$ (3.65) (0.084) (2.51)
Labor force: size (L.SQ).....	12	$N/J^*=0.0794-0.000118(Y-D)/J+u$ (0.0078) (0.000015)
Labor force: allocation.....	7	$\frac{\Pi}{N*}=-0.59+1.64\frac{W}{N_W}+u_7$ (0.21) (0.2)
Wage bargain.....	8	$w=w_{-1}-40.72+0.438E+0.240(p-p_{-1})+0.57U+u_8$ (36) (0.10) (0.05) (0.06)
Financial income..	9	$F=0.014+0.0076iK+u_9$ (0.51) (0.0006)
Rental income....	10	$R=0.48+0.0061iL+u_{10}$ (0.37) (0.0007)
Liquidity preference.....	11	$Yp/M=-0.0938+0.1520i+0.1958(100\Pi/K)+u_{11}$ (0.27) (0.076) (0.046)
(L.SQ).....	11′	$M/p=5.893+0.104(K+L)+0.0000768(K+L)^2-1.77i+u$

* The figure in parentheses is the standard error of estimate of the corresponding coefficient. Limited information maximum likelihood, unless otherwise specified. L.SQ.=least squares regression.

correlations. Few variables per equation should be used, because over a period of growth all variables increase (or decrease) several times their initial values and thereby exhibit a superficial intercorrelation that overwhelms finer structural relationships. Several well-intercorrelated variables in a single equation yield regression parameters of shaky statistical significance. Example: In the production function of Table 2, capital, output, and labor all increase substantially with time. Time and capital are so well correlated that it is impossible to introduce both in the production function and still obtain reasonable sampling error. Finally, a long-term model should contain a small number of equations, because if the few variables it contains are brought again and again in a great many equations they may eventually produce lack of identification.

TABLE 3

PERCENTAGE RESIDUALS FOR SELECTED LIMITED INFORMATION MAXIMUM LIKELIHOOD ESTIMATES

Decade	Equation Number										
	1	2	3	4	5	6	7	8	9	10	11
					(Sample Period)						
1	14.8	−45.4	29.0	−10.2	− 5.0	6.2	18.1	32	− 3.2	−88.2	16.0
2	22.4	− 0.6	18.3	8.3	− 3.2	0.3	−38.3	34	21.0	−27.3	10.3
3	18.1	16.5	10.0	1.6	− 0.2	0.7	−41.7	− 3	9.3	−13.8	11.6
4	12.6	18.4	10.0	− 1.3	10.1	− 6.0	−30.0	− 8	− 6.9	−17.7	1.4
5	34.2	6.5	0.4	− 2.4	10.4	1.0	− 5.2	− 5	−13.2	− 8.2	− 4.5
6	0.9	8.3	− 5.2	− 0.2	− 3.1	0.6	7.1	− 8	1.1	8.7	−12.2
7	− 2.2	5.0	−11.8	0.5	−10.5	− 3.0	10.1	−23	9.1	24.2	−11.5
8	− 0.5	4.2	− 6.0	− 0.7	−14.0	− 4.7	16.7	−21	12.7	21.2	− 7.7
9	− 6.3	− 6.5	− 5.6	− 2.4	− 4.6	− 1.9	23.2	−16	− 3.1	13.5	2.4
10	− 6.3	− 6.3	− 1.5	− 1.1	10.6	3.3	11.1	12	−30.4	−10.3	5.1
11	− 1.7	0.1	0.5	0.3	12.9	0.1	− 6.1	7	−13.5	3.8	4.2
12	− 9.2	−13.3	− 4.4	1.6	− 1.0	− 7.1	−19.9	− 5	4.5	− 6.8	9.1
13	−12.8	−15.8	− 4.7	3.5	−25.5	−11.2	−17.2	− 5	14.3	−24.1	5.6
14	4.7	12.1	8.6	1.6	2.0	− 1.4	− 2.8	2	14.7	3.0	5.7
15	10.7	6.3	2.4	− 2.5	10.4	17.2	14.1	1	− 8.4	13.1	− 6.4
					(Extrapolation)						
16	3.1	− 8.1	− 6.0	− 0.5	63.5	8.0	4.2	9	−10.2	25.9	1.0

Lags and other similar devices (integrals, derivatives, accelerators, maxima, and minima) are almost indispensable for self-powering dynamic systems and are used very extensively in ordinary econometric models. But in long-term models it is often not legitimate to use these devices. We have more changing sets of people and firms, a dimmer memory of lagged variables, and a shorter inertia in behavior if we pass from decade to decade than from year to year. Naturally the optimum lag may itself be among the unknowns to be estimated. This would be possible if annual data were available though at the cost of lengthy computations.

We are cautioned, therefore, to use sparingly derivatives, lags, and other traditional devices that give dynamic patterns. But then how

can we describe the exponential, logistic, and other dynamic patterns typical of most long-term statistics?

First, it happens that certain relationships materialize if allowed enough time. For instance, the accelerator's constraint (spurious in the short run where you have excess capacity, overtime, and double shift as good alternatives to new investment) is the more likely to become felt the longer the period considered. I find it much easier to believe in a long-run accelerator than in its frantic short-run version. Second, to dynamize a model we may use "floors," "ceilings," targets, and other critical values obtained from technology or psychology. When a uniformly developing system (growing or decaying) hits a floor or ceiling, it is forced to alter its velocity of development, thereby causing a realignment of other variables by accelerator-type effects. Another dynamizing device is the building into the system of several potentially conflicting trends, like the simultaneous decrease in both the birth rate and in the death rate, or the simultaneous tendency for the labor force to increase and for weekly hours to decrease. Critical points when crossed alter the direction of growth in some part of the system—say in employment or distribution—and soon propagate disturbances throughout the system.

Like any econometric model, a growth model can also be dynamized by means of exogenous shocks. Exogenous variables are of two kinds: economic and noneconomic. The more noneconomic exogenous variables, the better. The fewer economic exogenous variables, the better. Shock-type changes occur in particular times and not in others for good reasons, some of which are to found in the process of development itself.

So much for the difficulties of long-term econometric model building in general. Now there are some special ones due to the nature of my data and the extent of my computing resources. The Kuznets and Goldsmith data, used in this study, come smoothed out in the form of decade averages overlapping five years. The first decade is 1869-78, the second is 1874-83, and so on. For consistency, the data derived from other sources were reduced to the same pattern of fifteen overlapping decades from 1869 to 1948. Averaged series are generally fictitious—meaning by this term that the variable in question seldom assumes a particular average value. Now it can happen that a particular value is a critical value; for instance, a "warranted" rate, or an equilibrium point, which, had it been hit, would have had special effects. The data used in long-term models are as a rule more drastically averaged than data used in short-term models, and therefore the long-term model is more likely to miss essential information from our deliberately ignoring the critical points. Suppose that we did possess annual, quarterly,

weekly, or daily data. We could proceed in two ways: average the data and then estimate the equations or estimate the equation and average the result. Unless all the series and functions are extremely well behaved, there is no guarantee that the two methods will give outcomes that agree. Further, in disregarding these possibilities we are postulating something akin to a "law of continuity" of behavior and brushing aside the whole question of whether cycles are organically connected with growth. So both the fact that averaged data are used and the fact that these averages are decade (and not some other) averages qualify the results and restrict the uses of my model.

Estimating Procedure. The parameters of this model were estimated by the limited-information–maximum-likelihood method. Those of you acquainted with the vicious algebra of the method may ask: why now use such an elaborate and refined method for crude data, rough approximate relations, and with so few degrees of freedom? The answer is that this method captures the spirit of simultaneity underlying the economic system that the model is supposed to describe: it treats all the variables on equal footing, avoiding cause-effect decisions between endogenous variables. In this sense the method is free of theoretical bias. The method is unbiased in the statistical sense as well (for large samples). However, I did not use the maximum-likelihood method in the full glory of its general form. Certain assumptions were made in order to simplify the computations.

1. The disturbances u were assumed to be non-autocorrelated in each equation. This overstates the efficiency of our estimates by pretending that the fifteen observations are independent. To see this, take a linear equation of the form $\beta y_t + \gamma z_t = u_t$ where β, γ are vectors of the unknown (constant) parameters, y and z vectors of the endogenous and of the predetermined variables and u_t the random disturbance at time t. It follows that $\sigma^2_{\Delta u} = \mathrm{E}(u_t, u_{t-1})^2 = \beta \mathrm{E}(y_t, y_{t-1})^2 + \gamma \mathrm{E}(z_t, z_{t-1})$. Where the y's and z's are period averages overlapping half a period, they have a serial correlation of $\frac{1}{2}$. Therefore $\sigma^2_{\Delta u} = 0.5(\beta + \gamma)$, a fact which should be taken into account, except that β and γ are unknown, considerably burdening the computations. The first simplification then was to set $\sigma^2_{\Delta u}$ equal to zero.

2. *Linearity.* The numerous nonlinear variables such as, in Table 2, $e = 52 \times 10^{-5} EhN \frac{I}{K}, \frac{Yp}{M}$ contained in the model were treated equation by equation as if each were a simple variable, no correction being made in the estimating procedure to take account of these nonlinearities. Though it violates the strict requirements, this disregard of nonlinear variables leaves unaffected the statistical efficiency of my limited

information estimates. The alternative was to make the model strictly linear. Two reasons decided against it. First, the particular compound forms used are supposed to have economic and behavioral significance; e.g., e is the "labor inputs," $\frac{\Pi}{K}$ is the "rate of profit," and $\frac{Yp}{M}$ is "income velocity of money." Second, linear approximations generally necessitate a larger number of parameters to be estimated and I felt that I ought to economize the few degrees of freedom I dispose of. The model is linear in the parameters; and there are no side restrictions on the parameters.

3. An essential feature of the maximum-likelihood methods (with full or limited information) is to let each predetermined variable affect not only the parameter estimates of that equation in which it enters explicitly but of all other equations in the model as well. The commonsense equivalent of this property is that an exogenous shock or a lagged endogenous value permeates from end to end the simultaneous system of the total economy; computationally this is done by bringing into the estimation of each equation the matrix M_{zz} of moments of all the predetermined variables (z_i) in the entire system. But in fact the capacity of my computing equipment forced me to use only an arbitrary subset of the predetermined variables. This is the third simplifying assumption.

4. The fourth is the use of limited information. The method does not allow the parameter estimates of one equation to affect those of another. Adding, dropping, substituting, linearizing, or delinearizing a variable in one equation leaves unaffected the parameter estimates of another. Adding or removing an entire equation has no effect on the balance of the model. This means that if we wish to experiment cheaply with alternative formulations of given structural relations, we can do so under the limited-information method. In exploratory studies, the flexibility advantage of the limited-information method overweighs the disadvantage of suppressing some information. Since I think we know a little, a priori, concerning the form and number of independent long-term structural relationships, I am not so urgently interested in observing the modifications in one function brought about by some change in another or those brought about by dropping or adding an equation.

II

Evaluation of the Estimates. Though theory states that a certain relation exists, the particular econometric form of it that we adopt is reached by "cut-and-try" methods. A version is satisfactory if it combines a reasonable value of a parameter, small variance, and accuracy

in prediction. The first two qualities can be observed directly from Table 2.

In adopting this rather than that formulation, I have sometimes used a combination of arguments based on theoretical principles, on statistical necessity, and on expediency. This I frankly admit and, if you will, I may justify it in jargon by saying that with scarce and crude data, limited computing resources, and as yet undeveloped statistical techniques, what I did was to equalize at the margin the benefits from arithmetical minuteness, statistical subtlety, and theoretical rigor. You do have to sell your soul in the econometric business of today unless you have at your disposal tailor-made data and vast computing resources.

The results sometimes confirm and sometimes contradict accepted economic hypotheses. In view of all the inadequacies, the ability to confirm or contradict an economic hypothesis does constitute a step forward. In fact, I can do a little more; namely, predict in a limited sense.

Extrapolation of the Model. Estimating the model was begun in 1951, when only fifteen decades of data were available. At present, however, we have some data for a sixteenth decade (1944-53) and by means of them we can evaluate the predictive power of the model. The system of Table 2, if solved simultaneously, reduces to a ninth degree equation in Y. Inserting values for the predetermined variables, Y can be forecast, and working backwards all the other endogenous variables can be forecast too. To avoid this laborious task and yet be in a position to say something about the predictive ability of the model, I have proceeded in the opposite manner. I have obtained rough estimates for the years 1944-53 for all the variables, both endogenous and exogenous. These were inserted in the structural equations and they yielded certain residual errors for the decade 1944-53. These residual errors, in percentage form (Table 3), can be compared with the residual errors of the sample period. They should not be compared from column to column.

Actual investment (Column 5) is 64 per cent over the estimated but this is not surprising in view of the intervening world war and the investment boom it brought with it. Financial income was 10 per cent *larger,* and rental income 26 per cent *less* than "forecast"—probably due to wartime speculation and rent controls.

In the remaining equations, the model extrapolates to 1944-53 at least as well as it fits the sample period 1869-1948.

Implications of Our Parameter Estimates. In Table 2, I exhibit a linear homogeneous version 1 and a Cobb-Douglas version 1′ of the production function. The latter reverses the relative size of the contribution of labor and capital found by Douglas. We must remember,

however, that Douglas' study is for manufacturing only while mine is for the whole economy, and there is relatively less capital in the sum total of productive activities than in manufacturing industries. Secondly, Douglas' figures for labor input include wage earners only, while mine include the entire labor force; so my labor inputs, the variable e, are always greater than Douglas' N (in absolute terms) and my K is always less than Douglas' K (in relation to the labor inputs). Thirdly, there is no reason why the long-run marginal productivities of labor and capital should not differ from the short-run.

It is interesting to note that the sum of the exponents of 1′ approximately equals unity, something akin to the Cobb-Douglas homogeneity hypothesis, only now for the long-run global production function. This implies that if income were distributed according to marginal productivity, the product would be exactly exhausted; if distribution does not in fact agree with the marginal principle, we ought not to blame the production function. Let us look at the marginal productivities in Table 4. Those of labor and capital have been computed from the production function and in Table 4 have been listed along with the real wage and the profit rate. In both cases the marginal hypothesis seems contradicted. Labor appears to be 1½ to 2 times "overpaid" while capital is persistently 3 to 5 times "underpaid." Before concluding too much from this we should remind ourselves that: the labor inputs in the production function relate to the entire labor force, while the wage rate relates merely to the wage earners; our statistics do not record the increasing skill, health, and education as additions to the labor inputs but as forms of capital (schools, hospitals, churches) and thereby understate the productivity of labor; many things can happen in aggregating over firms and industries to obtain a global production function. The fact that the latter turns out to be homogeneous of degree 1 is no proof that the individual production functions are too. It is possible that each industry or firm equates wages to the marginal product of labor, but that monopolies or fixed input-output requirements of one industry from another prevent the marginal product of labor from reaching a uniform level in the entire economy. Therefore the statistics carry no implication that in any one firm or industry labor gets more than its marginal product. For the underpayment of capital, there are a number of fancy explanations: British predilection for investment in America; cheap money agitation; managers maximize size of business; they make consistently overoptimistic estimates, and others which do not diminish my astonishment at the unexpected results of Table 4.

Professor Arnold C. Harberger offers the following pragmatic solution: We know that the various income shares show remarkable stabil-

ity over a long period of time; that, at least in the long run, there is competition both in the product and in the factor markets; that there has been technological progress. Therefore we should utilize this knowledge to fix the exponents of our Cobb-Douglas production function at some reasonable level and from this estimate the remaining unknown; namely, the contribution of technology.

If we use as exponents the average value of the factor shares, as in Table 4, $av\ (W/Y) = .540$, $av\ (\Pi/Y) = .177$, $av\ (R/Y) = .074$ we

TABLE 4

MARGINAL PRODUCTIVITIES, REMUNERATION OF LABOR AND CAPITAL, AND INCOME SHARES

	LABOR (In 1929 cents per hour)		CAPITAL (In percentage)		INCOME SHARES (Percentage of GNP)		
	Marginal Product	Real Wage	Marginal Product	Profit Rate	Wages	Profits	Rent
1. From equation 1′							
Decade 1	7	17	41%	11%	53%	19%	8.3%
2	8	24	41	9	55	16	9.5
3	9	27	35	8	56	17	8.8
4	10	27	31	8	56	18	8.1
5	10	27	26	7	53	20	8.3
6	11	29	25	8	51	22	8.5
7	12	30	24	7	50	21	9.1
8	12	32	24	7	50	21	8.8
9	13	33	22	6	50	20	8.8
10	15	40	22	6	52	18	7.9
11	17	48	22	5	54	16	7.5
12	18	51	20	4	55	14	6.2
13	18	52	19	4	56	13	4.4
14	20	60	24	5	60	15	3.9
15	21	63	25	5	59	16	3.4
Average					54	17.7	7.4
2. From equation 1 Average for 1869–1948	8		29				

Equation 1″: $Y = .72K^{.177}(52\times10^{-6}EhN_W)^{.844}L^{.074}(1.08)^t$

obtain by least squares the formula 1″ (Table 4, bottom), where .72 and 1.08 are the estimated constant factor and the technological growth factor, respectively.

Eight per cent per decade is equivalent to about ¾ of 1 per cent (cumulative) per year. This rate is not unreasonable for that part of growth not due to increase in labor force or to increase in capital.

Elasticities. Table 5 shows point estimates of the various elasticities connected with the production function: They are defined as ratios

between the logarithmic derivatives of Y, e, and K. The three columns present the output elasticity of labor inputs (capital held constant), the output elasticity of capital inputs (labor held constant), the elasticity of substitution of labor for capital (output constant). The ratio of two values of $\eta_e K$ for two decades is identically equal to the ratio of the values K/e for the same two decades. Similarly, the trends of $\eta_Y e$, $\eta_Y K$ are average productivity trends Y/e, Y/K.

We notice that the elasticity $\eta_e K$ decreases quite rapidly with time and this indicates that the economy produces a given output in more

TABLE 5

ELASTICITIES OF TRANSFORMATION AND OF SUBSTITUTION

	Decade	η_{Ye}	η_{YK}	η_{eK}
1. From the production function 1....				
	1	.35	.50	−1.44
	2	.27	.50	−1.86
	3	.24	.58	−2.40
	4	.23	.66	−2.81
	5	.22	.78	−3.54
	6	.20	.80	−4.04
	7	.18	.84	−4.59
	8	.17	.84	−5.01
	9	.16	.90	−5.88
	10	.13	.93	−7.07
	11	.11	.90	−7.99
	12	.11	.99	−9.22
	13	.11	1.01	−9.76
	14	.09	.87	−8.95
	15	.09	.81	−9.21
2. From the production function 1'....				
Average for 1869–1948.....		.2488	.6991	−2.81

and more capital-intensive ways. In the decade 1939-48, for instance, a 1 per cent increase in capital would have (for the same output) displaced 9 per cent labor, while in the decade 1869-78 it would have displaced only 1½ per cent labor. These comparisons are significant only to the extent that the linear production function 1 (Table 2) fits the data well. As a matter of fact the exponential form 1′ fits them better and its elasticities are constant over time.

The "substitution function" (equation 2 in Table 2) has two aspects. First, it is a technological relation. Though particular firms or particular industries may operate under fixed coefficients of production, by substituting process for process or industry for industry, we can in the aggregate technology speak of substituting labor for capital. As for any firm or industry, so, too, for the aggregate technology, there are limits to substitution. Second, the function describes an economic

relation. It determines which factor-mix out of the many possible will be actually employed, and (equation 2) tells us that more capital will be used the higher the real wage rate. For instance, if the level of capital is 360 billions of 1929 dollars and wages are 63 real cents per hour, then a 10 per cent increase in real wages will lead to a 4 per cent contraction in labor inputs; at the same level of real wages and capital a 10 per cent increase in the capital stock would require a 4 per cent increase in labor inputs.

Turn to the first version of the investment function (equation 5). It implies that a one-point rise in interest brings the rate of investment down by roughly one-half of a percentage point. As our equation is linear, we cannot use it to test the interesting speculations whether investment exceeds all bounds at near zero interest rates. The alternative version (5′) is an accelerator type. From the substitution function, we can compute the amount of capital that is best for any given level of output, real wages, and labor. Investment is determined by the attempt to adjust capital to the appropriate level K_{req}. Equation 5′ says that the rate of investment is more than seven times the rate of capital shortage.

The dynamics of the birth rate (equation 6) are straightforward, with the variable t (time) summarizing all the sociological and ecological trends that pull down the birth rate. Shorter run fluctuations in the birth rate are related to prosperity and depression. Other things being equal, one more baby per year is added to each 1,000 of population for every 9 per cent annual increase in per capita real income. To maintain the birth rate at any realized level requires an average increase in per capita real income of 11 per cent annually.

Equation 12 confirms the hypothesis that people choose to take part of their progress in leisure for more individuals, in addition to more leisure for a given number of individuals which is reflected in the decline in working hours.

Equation 7 shows how the total labor force divides itself into its two main occupational categories: the wage earners and the self-employed. It is not necessary for every worker to have occupational choice but that there be a marginal group having mobility. If mobility were perfect and income the only consideration, the constant term of the equation would be zero and the second parameter of equation 7 equal to one.

We have discovered that the global production function cannot explain the factor shares by means of the marginal productivity theory. The wage bargain equation 8 formulates the common-sense view that wages depend on prices, on the power of the unions, and on the degree of prosperity.

The Monetary Equation. In equation 11, I have expressed Kalecki's theory of long-term liquidity preference, while equation 11′ is an asset preference ratio theory of money balances. M is monetary assets of the nongovernment, nonbanks public. The curvature is as expected. The richer the community the greater the proportion of its assets it can afford to hold in liquid form.

III

Dynamic Properties of the Model. To show the derivation of the model's dynamic properties is a laborious task. I shall limit myself to telling what the results are.

The Long-run Multiplier and the Domar Problem. In solving for equilibrium conditions, we find the long-term marginal propensity to consume very close to unity. At most, 23 billion 1929 dollars of autonomous, unproductive, net investment are required annually to maintain employment equilibrium. If, however, the required investment is all productive, since it raises output in the future, it must increase at the rate of 3 per cent per year, assuming constant population and constant labor force. If, as is predicted by population experts and sociologists, the labor force percentage should continue to increase for two or three decades more and then become stabilized while population continues to rise at the present rate, the rate of growth in the required investment is considerably smaller than 3 per cent in the long run, and perhaps the savings gap will close by itself.

The Inflationary Bias. The income distribution and the monetary equations solved simultaneously as a subsystem reveal that if other things remain equal an autonomous rise in the price level leads to a sharper rise in the equilibrium money wage.

The financial income equation (9), disregarding the constant, says that roughly three-quarters of the value of capital yield financial income. This is in agreement with the fact that approximately 20 per cent of total capital is social capital (roads, monetary silver, and gold) which does not yield financial income.

Similarly, not all land yields "rental income." Public land must be subtracted: it is about one-fourth of the total in acreage, though it is hard to say what percentage it is in value terms. About 40 per cent of homes and farms are owner-occupied. The rest are rented but their yield is not counted in rental income unless the proprietor engages primarily in real estate. As a rough guess about one-third to one-fifth of the leased land belongs to this category. All in all, rental income yielding land should be in the neighborhood of two-thirds of the total. This, too, agrees with equation 10.

Conclusion. This simplified account of my study shows that for broad

purposes, one set of a dozen or fewer equations captures the outline of the American economy over a long period of time. In spite of laborious compilation of data and refined manipulations of time series, I have admittedly contrived only a rough sketch of the American economy. But there is some satisfaction in the very roughness of the task. By tapping only the more accessible surface riches of our new statistical series and by testing econometrically only for the bolder generalizations of "magnificent dynamics," one is rewarded with easy victories. As you see, it is possible to get some stable estimates with reasonable implications and with predictive ability. If I am to evaluate the general significance of my work myself, the most modest conclusion I can draw is this: Pessimists, cheer up! It *can* be done!

DISCUSSION

EVSEY D. DOMAR: Mr. Power's paper is in two parts: the first is concerned with the relationship between the growth of the labor force and general economic growth and stability; the second is devoted to the study of the effects on capital intensity (that is, the ratio between capital and labor) of changes in the interest rate and in the prices of labor and of capital equipment. An analysis of these effects is required for the solution of the problem raised in the first part.

The first part presents a growth model consisting of four simple equations:

$$(1) \quad I = c\,\frac{dL}{dt};$$

$$(2) \quad S = sY;$$

$$(3) \quad Y = pL;$$

$$(4) \quad I = S,$$

where c stands for capital intensity, s for the fraction of income saved, p for average labor productivity, and I, L, S, and Y indicate investment, labor force, saving, and income, respectively.

The solution of this system takes the form of

$$(5) \quad \frac{\frac{dL}{dt}}{L} = s \cdot \frac{p}{c}.$$

In other words, under the assumed conditions, there exists an optimum rate of growth of the labor force which will just assure sufficient investment for the maintenance of full employment; and a deviation, up or down, from this rate will result in a cumulative process of inflation or deflation respectively.

The structure and solution of Mr. Power's model are so similar to those of the investment models of Harrod, Fellner, and others (and it can be easily shown that the capital coefficient used in the latter is but c/p), but one may wonder what special purposes it serves. I believe there are several.

It allows the builders of other growth models to see their creation, so to speak, from another side, which may give rise to some second thoughts. It is obvious that constancy of the capital coefficient implies constancy of the ratio between capital intensity and labor productivity (c/p); yet this had not occurred to me.

Mr. Power's model shows how an attempt to equip a rapidly growing labor force with sufficient capital equipment can simultaneously create inflation and unemployment—a phenomenon not unfamiliar to students of undeveloped countries. But the fact that the remedy lies in a higher propensity to save may be of interest to some of our more devout Keynesians.

Finally, the model makes explicit the conditions which give rise to a most interesting paradox in the theory of growth; namely, that an insufficient

growth of the labor force can cause the unemployment of labor—a paradox mentioned by a number of writers but usually without the naked simplicity of Mr. Power's model. (See the discussion of this subject by D. Hamberg, H. Pilvin, R. F. Harrod, and E. D. Domar in the *Quarterly Journal of Economics*, August, 1952, pages 444-449; November, 1953, pages 545-563; and November, 1954, pages 641-648.) They have been concerned that an attempt to invest all the savings which the public desires to make would cause an over-accumulation of capital, presumably relative to available labor, and that the capital thus made idle would lead to a fall in investment and hence in national income and employment. This presupposes that the ratio between capital and labor (capital intensity) is fairly rigid, and I cannot help wondering if all those who have used this paradox as an explanation, or at least one of the explanations, of the downswing are also willing to subscribe to the assumption on which it is founded.

To raise a question of this type may well be the main purpose of simple mathematical models: they hardly describe reality, nor can they predict, but they may induce the general economist and the man of affairs to give a second thought to a position previously taken.

The second part of the paper deals with capital intensity; that is, with the ratio of capital to labor. This is indeed a most interesting subject the importance of which transcends its use in this particular model. It is not a new subject in economic literature but unfortunately it has been mostly used (or may I say misused) in business cycle theories. (See the discussion and bibliography in Friedrich and Vera Lutz, *The Theory of Investment of the Firm* [Princeton, 1951], pages 137-142.) Let me put cyclical considerations aside and restate the problem in the following manner: why does it pay to use American machinery produced with expensive American labor to save other expensive American labor, while it frequently does not pay to use, say, Japanese machinery produced with cheap Japanese labor to save other cheap Japanese labor? If Japanese machinery were used to replace American labor, the answer would be obvious; but the shoe is on the other foot, and quite often it pays to use American machinery produced with expensive American labor to replace cheap Japanese labor. My question undoubtedly sounds naïve, and the answer may be perfectly obvious; but I must confess that it escapes me, at least in the more or less static setting of Mr. Power's model, which does not include an analysis of technological progress as such.

Abstracting from the latter, we can reduce the question to two simpler ones: will a fall in the interest rate and a rise in wage rates increase capital intensity; that is, bring about the substitution of capital equipment for labor? If wages rise relatively to prices of capital equipment, the answer is obvious (I hope). This is the assumption that Mr. Power uses, but I have doubts whether it is fully justified on empirical grounds if sufficient time is allowed, as it should be in a secular problem, for the completion of all adjustments. From an analytical point of view, the other possibility—that prices of labor and of capital equipment rise at the same (relative) rate—is of course much more interesting.

For the solution of this problem, we are referred to Kaldor's article on "Capital Intensity and the Trade Cycle." (*Economica*, "New Series," 1939, pages 40-66.) It is a most interesting paper (though I do wish that its author, having shown his well-known analytical virtuosity in the body of the paper, would then have added an appendix for the general reader). Two cases are considered: (1) when the size of the firm is limited by capital funds, and (2) when it is limited by the extent of the market for its products. In the first case, Kaldor reaches the surprising—at least at first glance—conclusion that capital intensity will not be affected by changes in the interest rate, because, by assumption, every project considered by the firm requires the same amount of capital. The latter is really rationed, and hence its price is of no importance, unless it rises high enough to make this assumption ineffective. On the other hand, the amount of capital being given, a more capital-intensive project uses less labor and has a smaller output and hence a larger profit margin per unit of output than a less capital-intensive one of equal profitability. Therefore an equal proportionate rise in prices and in wages will discriminate in favor of a more capital-intensive project.

Kaldor's second case assumes an unlimited supply of capital funds but a limited demand for the product. The conclusions of the first case are now reversed: the interest rate is effective because the capital-intensive project uses more capital; but an equal (relative) change in the prices of both labor and capital equipment is of no importance because two equally profitable projects have identical cost curves. Kaldor's whole discussion is based on the assumption of constant returns to scale.

Kaldor's two special cases (and they are quite special) only whet one's appetite rather than satisfy it, but an excursion to *The Theory of Investment of the Firm*, by Friedrich and Vera Lutz, where a number of possibilities are analyzed in great detail, does not resolve the problem either, particularly as far as the change in prices of labor and of capital equipment is concerned (the authors are somewhat more positive regarding the effect of the change in the interest rate); and we are told that depending on the exact nature of the profit-maximization process and on the shapes of demand and cost curves and other conditions, the effect on capital intensity can be in either direction. And this is odd, because the greater capital intensity in the American economy as compared with others appears to be so clearly established that a reasonably unequivocal explanation of this phenomenon should be forthcoming.

The problem surely deserves further study, and I hope Mr. Power will continue working on it. May I make a few very simple suggestions:

1. It is about time to start distinguishing between a firm and an industrial process or a project. The amount of capital funds available to a firm may be given, but not necessarily to each project. On the other hand, the market for a particular product may be limited, but a firm can create new products. (On this see Edith T. Penrose, "Limits to the Growth and Size of Firms," in this volume.)

2. Similarly, it is about time to stop the identification of funds available to a firm with its investment in capital equipment. Funds are obtained not only for such investment but also to buy raw materials, pay wages and taxes,

and make all sorts of expenditures. After all, capital equipment is not always bought; it can be rented.

Finally, I suspect that we shall make little progress in this field without a closer analysis of technological change and without an explicit recognition of uncertainty.

T. C. SCHELLING: My comments relate, first, to the structure and, second, to the perspective in time of Mr. Valavanis' model.

In making the acquaintance of a twelve-equation model it is sometimes helpful to look for familiar, simpler models embedded in it, being on guard against the dangers in taking a submodel out of context.

We can ask, for example, what in the model makes income grow. A complete answer depends on the whole system of equations and even to describe how it grows requires a ninth-degree equation. Still, the main engine must be simpler. There are only two demand equations for final goods—consumption and investment—and they must be central to the growth process.

In the consumption equation, population explains a rising level of absolute consumption but not the rise per capita. The high long-run propensity to consume, however, of about 100 per cent, needs only a little investment to become explosive; and the lag, by holding the "current" propensity to about 70 per cent, strongly damps the explosion. (If investment followed a simple relationship to income, for example, the growth formula would be approximately $Y = [30/(30\text{-}v)]Y(t\text{-}1)$, with v denoting the propensity to invest and the lag being a complicated average of about five years.)

The Valavanis investment equation can partly be construed as a "propensity" related to profits; if we multiply through by $K/100$, one term is "income minus wages." But then it also has a term equal to the capital stock times a constant plus the interest rate. While the interest rate component could be interpreted here as a refinement of profits to a net "after-interest" basis, the negative effect of the capital stock on investment strongly suggests the acceleration principle rather than a propensity to invest out of profits.

The acceleration principle emerges quite clearly if we treat the interest rate as a constant, and consider $(Y\text{-}W)$ as a constant percentage of Y. The whole investment equation than translates into

$$I = a[kY\text{-}K]$$

where k can be interpreted as the "normal" capital/output ratio, and the parameter, a, reflects the rate over time at which investment tends to bring capital stock into normal relation to output. This is pure acceleration principle.

The interest rate is not in fact constant; but its variation over the period exerts little leverage in the Valavanis model. The constancy of $(Y\text{-}W)$ in relation to Y can be improved by inclusion of a time trend that would not be hard to rationalize in the acceleration relationship.

Whether investment is really acceleration-motivated is not proven by the fact that this translation can be made; and I doubt whether the "correct" structure of an investment equation will ever be discerned in such highly aggregative time series data. What the translation shows is that investment,

income, and capital grow, in the Valavanis model, as if a familiar multiplier and accelerator yielded nicely damped explosive growth over the eighty-year period.

Probably any pattern of actual growth is bound to look like a multiplier-accelerator model over the long run, and we must not deduce too much motivational significance from the appearance. But it does help acquaint us with the mechanics of the present model to see its close quantitative analogy with the familiar machine. Incidentally, the strong consumption lag in the Valavanis model yields an unusually stable mechanism of accelerator growth.

We did not ask above whether the parameters of this accelerator model yielded growth rather than cycles. They do, but since they were fitted to a period of actual growth, their values were nearly bound to. But we are naturally tempted to inquire how close they are to yielding cycles, and to manipulate the model in various ways to see whether shocks can lead to cycles, or how price inflation works through the system, etc. Unfortunately, most such interesting questions have to be ruled out of order, for reasons that lead to the second part of my comments—on the statistical timing of the model.

The data from which the model is derived are decade averages, spaced five years apart. There can be no business cycles in such data, except very long ones or such greatly damped and distorted ones as emerge from the averaging process. Similarly, the model cannot generate any cycles but very long ones. Even ignoring the averaging process, the shortest time interval is five years. And the only inflationary process that can be traced through the model is, consequently, one that emerges from long-run wage trends.

But what good is a model that cannot generate business cycles? This question is asked not because we are interested exclusively in cyles but because business fluctuations in the period covered by the model have been of such importance that we must question any analytical scheme that is inherently incapable even of recognizing those that occurred.

The above question is not purely rhetorical. Whether the model is valid depends a good deal on whether long-run economic development in this country has in fact been essentially long run in its behavior dynamics or has been instead the continuous compounding of essentially short-term behavior. If we knew—on the basis of knowledge other than long-run time series—that investment decisions were mainly related to current or short-term factors, then a long-run model might reflect some kind of average behavior; but it would probably be fairly unreliable for analysis or prediction, it could differ greatly from the "true" structure of the system, and it would probably yield no information about the motivation of investment at the level of the individual firm.

I would emphasize that this question about the Valavanis model is not the usual one of long- versus short-run determinants of growth. The model is not just long run; it is long-run and highly endogenous in its mechanics of growth. It does not incorporate trends, with the main exception of population. It is a self-propelled machine that ignores the intradecade bends in the road and navigates from a minimum of secular benchmarks. So our full question should read: can we abstract both from the endogenous short-term dynamics

of the American economy and from the long-term exogenous trends, and find a substantial body of long-term endogenous relationships that are not only valid but so influential that they govern, decade by decade, the long-run pattern of growth?

Mr. Valavanis' hypothesis is that we can; and, within the limitations of aggregative time series correlation, his results are some test of that hypothesis. Unfortunately, until some annual data are reconstructed for the same period, a competing hypothesis relying on shorter term behavior dynamics cannot be fitted for comparison.

CURRENT PROBLEMS IN AGRICULTURAL ECONOMICS

THE SUPPLY OF U.S. FARM PRODUCTS UNDER CONDITIONS OF FULL EMPLOYMENT*

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Appropriate quantitative estimates of the agricultural supply function under full employment do not exist. However, some writers have suggested that either the elasticity is zero or that the variables underlying the supply functions are entirely exogenous to the economic system.[1] While agricultural production does not vary greatly between years, the elasticity coefficient is certainly greater than zero.

Full interpretation of the agricultural supply function in an environment of full employment can be made only with several years of complex empirical research including: (1) specification of the appropriate analytical model (i.e., the relevant variables and their possible algebraic forms); (2) measurement of the relevant observations; and (3) prediction of the coefficients which make up the system of equations involved. These empirical predictions are difficult over a period which encompasses the full range of variations in employment; perhaps they are impossible for the smaller number of time-series observations which represent the relevant span of full employment. The period of years which provides the framework for observing the agricultural supply function under full employment is both short in years and heavily confounded by other phenomena. The only two periods of full employment which are meaningful for our purposes are 1910-29 and 1942-54. Even then, half of the first period represents one in which the agricultural plant was still being expanded by the addition of cropland and total farm acreage. Also, a war emergency existed in both of these periods; nonprice restrictions and incentives cause these years, as well as some of those immediately following wars' end, to be somewhat inappropriate for predicting the nature of the agricultural supply function. In reference to the future, the 1942-54 period alone may

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[1] Wilcox and Cochrane (*Economics of American Agriculture* [Prentice-Hall, 1951], pp. 413-418) suggest that the supply function is a vertical line. Brewster and Parsons ("Can Prices Allocate Resources in Agriculture," *Jour. Farm Econ.*, Vol. 28) state that farmers do not respond to price because they are motivated by "mechanistic" rather than "economic" forces.

be meaningful for predictions. The 1910-29 period represented a completely different complex of techniques, capital structure, and firm-household bonds. Because of the dearth of appropriate time-series observations and because of the complexity of the phenomena, we make no attempt to provide quantitative statements. At best, our discussion represents hypotheses about the nature of "selected variables" which enter into and affect the elasticity of the supply function. Time prohibits even a "description" of all the relevant variables.

Supply Functions for Individual Products

If the elasticity coefficient is great relevant to consumer demand, it results in wide swings in the output of particular commodities: there is a relative scarcity in some periods and a relative surplus in others. If it is too low, a relative surplus of particular products will persist over time. (We define a surplus or deficit as marginal value products which are lower or higher respectively than those which might be earned by the same resources in alternative employment opportunities, considering existing barriers to transfer of resources.) The short-run supply functions for individual farm products have relatively high elasticity under conditions of full employment. Elasticities are great enough to cause large diversions of resources between agricultural products over the span of a single year. Over the full employment periods 1920-29 and 1946-54, acreages of crops such as potatoes, flax, soybeans, sugar beets, barley, cotton, onions, pimentos, and other vegetables quite often differed between years by as much as 25-50 per cent. These are, of course, crops where the slope of the production possibility curve (or important segments of it) for individual farms and regions normally does not differ greatly from the slope of the isorevenue curve (i.e., the substitution ratio between crops differs relatively little from the "normal" price ratio). Even for major crops such as corn and wheat where substitution ratios and price ratios between products do differ considerably and a large portion of the farm plant is devoted to their production, annual changes in acreage sometimes have approached 10 per cent in full employment periods. Cattle fed and slaughter lambs fed and cattle and sheep on farms have recently changed between years by as much as 10-20 per cent. These changes in inputs of particular classes of resources (acres and animal numbers) fairly well denote changes in production planned by farmers; other classes of inputs change similarly, to the extent that they are close technical complements of land and animal units.

The interproduct transfer of resources within agriculture takes place as readily as the interproduct transfer of resources within major industrial sectors. In one sense, this transfer within agriculture takes

place with greater ease in agriculture than in industry. Most of the interproduct transfer of agricultural resources between years takes place on individual farms at one geographical location. Very little of it results from the transfer of labor and capital resources between such distinct products and locations as cotton in Alabama and oranges in California. The supply function for individual farm products is highly elastic because of these reasons:

1. The production function, particularly for annual crops, approaches an elasticity of 1.0 on individual farms. We are speaking, of course, about the crop production function characterized by a transfer of more of all resources to a particular commodity. Over a considerable acreage range on all farms, increasing land acreage in fixed proportions with its technical complements results (aside from weather fluctuations) in about a proportional increase in output of an individual crop. For example, a corn belt farmer growing twenty acres of soybeans may double his output if he pushes soybean acreage up to forty acres and at the same time doubles the input of labor and capital services; if the acreage for soybeans is diverted from corn, decreases in the output of the latter generally will be proportional to the amount of resources withdrawn. Of course, as we aggregate farms, a given percentage increase in inputs for a particular crop will result in successively smaller additions to output as the added acreage must spread to soil types less adapted to the crop. While this phenomenon cannot be looked upon as causing the elasticity of the individual farm production function to depart from unity, it does increase the slope of the supply function and causes its elasticity to decline for all farms considered together.

2. Large changes can be made in the quantity of resources used for particular products without causing important changes in the total quantity of resources employed within agriculture and causing large shifts in the geographical location of capital and labor resources.

3. The skills of labor and the services of capital resources have great adaptability between some products. For example, labor and machinery adapted to corn production are generally adapted to soybean production.

4. The production possibility curve is one with great flexibility for transfer of resources between products. On many farms, it is composed of linear (or near-linear) segments specified by the production limits of each individual resource. In Figure 1, for example, the possibility curve becomes *abcd*. If the slope of the *bc* segment falls within the range of price ratios experienced, the individual farm may readily shift product combinations back and forth between *b* and *c*. Of course, since these "between corner" segments are of different length (take on varying slopes for different farms and may even be slightly concave on

some), the aggregate supply curve for one individual product is not linear, although it has a high elasticity coefficient. (Because of uncertainty considerations, the combination of products may fall within a segment such as *bc*.)

5. Shift of resources between products often does not entail any great change in uncertainty. Resources may be on hand to produce either radishes or carrots, corn or soybeans, barley or potatoes, etc. Transfer of resources between two such products does not entail the same degree of risk as transfer of labor to a manufacturing enterprise or in adding large amounts of capital to the farm enterprise (and, consequently, in reducing the firm's equity).

The Aggregate Supply Function

The short-run supply function (i.e., one extending over a single year or two) for all farm products aggregated together presents a different picture. Either because the price flexibility of major resources parallels that for agricultural products in general or the phenomena which underly it cause the supply function to decline or to have low elasticity, output does not respond as readily to changes in factor/product price ratios. To be certain, aggregate output of farm products does respond, under conditions of full employment, to price changes. However, the aggregate output is much less sensitive to price change than is the output of individual commodities. Using 1910-29 and 1942-54 as our full employment periods, we find that gross farm production has never increased by more than 8 per cent between years; it has never decreased by more than 10 per cent. Changes this large in output result mainly from weather variations rather than from planned changes in resource inputs.[2] Changes in inputs, although slightly affected by exogenous variables such as weather, better suggest changes which farmers plan. Total inputs of agriculture increased by 4 per cent from 1919 to 1920. While most of this maximum increase was due to price, some of it also was due to above average yields. The largest decrease in inputs over a single year was 3 per cent from 1915 to 1916; some of this decline also was due to weather. Perhaps the greatest change planned between years by farmers, as measured by inputs, has never been over 2.5 per cent. A longer period of contraction was 1920-24 when both absolute and relative prices of farm products declined; total inputs decreased by a total of 5 per cent. Major expansion periods, accompanying a rise in absolute and relative price levels, were 1917-20 and 1942-49; aggregate inputs increased by totals of 8 and 9 per cent, respectively. Changes over a few years do not, of course, represent

[2] T. W. Schultz *(The Economic Organization of Agriculture* [McGraw-Hill, 1953], Chaps. 7 and 13) gives a detailed account of these quantitative changes.

a long enough time period to truly indicate the kinds of plans which are made in response to price changes. Most changes in the farm plant which affect longer run supply function rest on expectations over a period of years. Periods of full employment as long as twenty years, with factor/product ratios for agriculture drifting continuously in one direction, are probably needed before we can predict the elasticity coefficients for particular periods.

Adjustments in inputs—those facets of the supply function over which farmers do have control—have been more sensitive than examination of aggregate inputs would indicate, however. Technical innovation and relative prices have placed different premiums on resources. They have called for increases in some resource inputs and decreases in others. These changes partially cancel each other in the aggregate index of inputs. For example, total labor inputs decreased by about 10 per cent in the period 1942-49 while fertilizer increased by 74 per cent and annual inputs of building and machine services increased by 60 per cent. In the twenties period of contraction in agriculture, fertilizer and lime inputs decreased by 30 per cent in a single year from 1919 to 1920; power and machinery inputs declined by 18 per cent from 1920 to 1925, even though the "technical revolution" in agriculture was beginning to press (even in the face of declining price relatives) for use of more of these resources. We also must recognize that some sectors of agriculture have an increasing role while others have a declining role in a developing economy. Economic development occurs largely in periods of full employment. With a growth in national product and per family income, the income elasticities of demand call for a withdrawal of resources for some agricultural products and an expansion for others. These several changes within agriculture tend to cancel each other out and "obscure" the magnitude of the elasticity coefficient. While representing only small segments of agriculture, numerous farming areas can be used to show remarkable changes in product output and resource input. Negative or very low income elasticities of demand and contraction in foreign sales and other changes have provided price forces resulting in withdrawl of 40-50 per cent of the nonland resources in some areas. In other selected farming regions, growth in the national income, relatively high income elasticities of demand, and other forces giving rise to increases in relative price for particular products have resulted in large expansions in both outputs and inputs. While other forces were also at work, one need only turn to the first century and a half of American agriculture to view an extremely elastic supply function. At a lower stage of economic development and with a set of income elasticities of demand calling for increased output, agriculture

expanded in a manner which has seldom been surpassed by other industries.

Because of these many "offsetting" trends affecting resource inputs and because of the "joint variables" involved, refined prediction of supply elasticities might be obtained only through simultaneous estimation of the several structural relationships which "make up the system." Empirical predictions, with the proper degree of refinement, would undoubtedly prove the supply elasticity to be greater than a superficial examination of aggregate input and output suggests it to be. A simple plotting of aggregate farm output (or, for that matter, aggregate inputs) against agricultural price relatives, or a simple least squares prediction of the regression coefficient, probably tells us little about the elasticity of the agricultural supply function. This procedure can only provide a "mongrel relationship" which is neither the fish nor fowl of the supply function. First, there is not a supply function over a period even as short as five years, but rather a whole series of supply functions. There is a different supply function for each change in techniques, for each change in exogenous variables such as weather, for each change in the absolute or relative price of a particular resource and for each change in certainty of farmers' price expectation (both in respect to farm prices and nonfarm prices, such as industrial wage rates). Second, supply and demand functions jointly affect prices and quantity produced; where both are subject to change, an estimation of one function apart from the other may be quite meaningless. (A supply function can be estimated by least squares regression techniques only if the supply function remains constant and the demand function goes through a series of changes and price and quantities are the only variables.) Finally, the supply function for agriculture products cannot be predicted accurately apart from the demand and supply function for various categories of agricultural resources.

Aggregate supply responses may not be rapid when compared to those of individual agricultural products or to particular industrial products. However, certainly a sustained twenty-year period of full employment, with relative agricultural prices continuously moving either down or up, would give some very sizable adjustments. We are not, of course, saying that the elasticity coefficient is as great as 1.0. We are only saying that (1) in the short run it is considerably greater than zero and (2) over a period of years a succession of long-run supply curves, which consider changes in commitments of labor skills and preferences as well as in physical plant, would become operative to show agriculture as a price-responsive industry. Accordingly, we wish to summarize, first, conditions which cause short-run supply functions to

be relatively inelastic but which become greatly relaxed over longer periods and, second, phenomena which surround supply functions and cause them to appear, particularly when measured as a simple plotting of output against price relatives, to have greater slope than is actually true.

Forces Encouraging Low Elasticities in the Short Run

Supply elasticities tend to be low in the short run because of the following reasons.

1. The reservation price of agricultural labor is low for a portion of the farm population. (This phenomenon is simply one facet of flexible resource prices outlined later.) We suspect that income from agriculture relative to income from use of the same resources in other opportunities may drop by a considerable amount before many farm owners, upwards from middle age, will transfer their resources out. Not so many of their sons, however, will go into farming if incomes are relatively low. Other segments of labor also have low mobility in the short run, because of uncertainty and lack of knowledge, but would not remain in agriculture for decades under low resource returns.

2. Capital limitations place a damper on the rate at which assets are added in the short run. With a high degree of risk aversion built into their psychological make-up during depression, farmers in general may be willing to add assets only at about the rate which they can build up equities at the outset of full employment periods. Over a longer period of time, however, as the farmer's equity becomes high (as it is now), he may make large added investments if relative price ratios remain favorable and certain over a long period.

3. Because of uncertainty and capital limitations, farmers typically use so few resources that transformation ratios are considerably greater than factor/product price ratios. An increase in the price ratio brought about by a decrease in product price need not cause the amount of resources used to decline proportionately; the price ratio can increase considerably before it approaches the transformation ratio (although uncertainty may cause some contraction). As factor/product ratios decrease through a relative increase in product prices, farmers are still reluctant to drive transformation ratios lower (through the use of more resources) until they approach price ratios. However, over longer periods of favorable prices, their risk discounts may decline, allowing transformation and price ratios to be more nearly equated.

4. The production function is "more fixed" and less elastic in the short run than in the long run. The aggregate production function is limited, on the side of expansion, to a nearly-fixed plant in acreage; for single products, land can be increased in about the same proportion as all other categories of inputs. (Similarly, "space" limitations allow

particular industries to expand their plant in a "scale fashion"; production functions can have elasticities approaching 1.0.) The elasticity of production (and hence that of supply) is lower as the number of fixed resources is increased. With a fixed plant, the elasticity of the aggregate production should be less than 1.0, although it also may be composed of linear segments approaching 1.0. We doubt, however, that the aggregate production function has a "corner" separating ranges of high and low elasticity as sometimes proposed.[3] In the first century and a half of this nation, agriculture was faced with a long-run production function wherein land inputs could be increased freely and in proportion to the increased capital and labor inputs (i.e., in the sense of a "scale expansion"). We need only look back into history for indications that agricultural supply is not a variable exogenous to the economic system. No industry has responded more readily to the national economic development factors (particularly a "high" income elasticity of demand for food in a rapidly growing consumer economy). Land has, however, become largely a fixed factor (on the side of expanding the agricultural plant) in the aggregate production function. Technical innovation now acts over the long run to provide the elasticity of production formerly allowed by land. Technical innovation as it relates to the agricultural production function can be viewed in this manner: The production function is of the form $Y = f\ (X_1, X_2, X_3, X_4, \mid X_5 \ldots . X_n)$. Here, the vertical line separates resources which are variable or fixed in quantity. As the bar is moved to the left, the elasticity of the variable inputs declines; as it is moved to the right, their elasticity increases. The input of a particular resource, such as hybrid seed corn or stibestrol, is fixed at zero before the innovation. Upon innovation, the bar moves to the right of this resource (i.e., the input of the particular resource is no longer "fixed at zero" in the production function).

[3] Wilcox, "Effect of Farm Price Changes on Efficiency in Farming," *Jour. Farm Econ.*, Vol. 33, proposes this possibility on the basis of a particular fertilizer experiment. A "corner" on the production function (i.e., one segment of a marginal cost function forming a near-angle with a more vertical segment of the marginal cost curve) would allow price ratios to teeter over wide ranges across this fulcrum without specifying a different quantity of resources. However, research on fertilizer and feed with fixed production units as small as a single acre or a single animal does not show distinct corners on the production function (and hence on the marginal cost function). These "fixed plant" production functions generally show the elasticities to decline by degree rather than abruptly. Hence, the aggregate production, defined by the addition of fertilizer, feed, and their technical complements to a fixed acreage defining the agricultural plant also should have an elasticity which declines only by degrees. In fact, many farmers use resources in ranges of the production function which still have very high elasticity. While resources in the Great Plains, for example, may need to be combined nearly as "fixed technical complements" (C. Gislason, "Nature of the Aggregate Supply Function of Agricultural Products, *Jour. Farm Econ.*, Vol. 34) and may have low elasticities beyond this "sequence of tasks," the aggregate supply function need not be "sloped" accordingly. Even if every farmer has a "distinct point of production," gradations in soil productivity and adaptability would still cause the supply function, aggregated over all soils and acreage, to slope "more gently and continuously."

Hence, innovation and the increase in input of this resource (allowing it to become a variable in the production function) causes the elasticity of production for the remaining resources to increase. The longer the time period and the greater the number of innovations, the more elastic the production function becomes. (Space does not permit us to go into the details as to which resources under innovation become technical complements and "factor using" or substitutes and "factor saving.")

5. Miscellaneous forces such as the close bonds between the firm and the household, low reservation prices on particular resources and a greater degree of short-run fixed costs, also interact to give a supply elasticity which is relatively low at one point in time but which certainly increases under a long span of full employment. The forces mentioned above cause supply to be relatively inelastic in the short run but allow the elasticity to increase in the long run; they "hold output in check" over the short run of increasing price relatives, although they need not do so over longer periods. The forces mentioned below tend to encourage a given level of output even in the face of declining price relatives; but they need not support an extremely low short-run elasticity coefficient.

Variables Causing Supply Elasticity to Appear Low

As suggested by Elmer Working three decades back, any phenomena which causes demand and supply functions to change simultaneously may result in "simple empirical" observations such as those shown in Figure 2.[4] Here quantities are determined by the supply and demand pairs a and z, b and y, and c and x. If one plots the quantities against price, he obtains a "mongrel" line such as M. Using it to denote the supply functions, he would certainly conclude that it has an extremely low elasticity. However, the "true" supply function (x, y, or z) in any period has a much greater elasticity. We offer the following to explain why supply elasticity, under full employment, may be greater than sometimes predicted:

1. Flexibility in factor prices.[5] Factor prices may parallel product prices in flexibility. Supply and demand functions then move together in the fashion of Figure 2. While the supply function may be relatively elastic, the sequence of annual outputs give a "mongrel production curve" *(M)* with great slope. D. G. Johnson has illustrated the importance of flexible factor prices in explaining maintenance of agricultural output during depression ("The Nature of the Supply Function for Agricultural Products," *American Economic Review,* September, 1950). While applying to fewer resources, fixed factor supplies and flex-

[4] Elmer Working, "What Do Statistical Supply Functions Show?"

[5] This is the case of Marshall's stones receiving quasi-rents and can be discussed almost synonymously as inelastic factor supplies or high fixed costs.

ible factor prices also apply during relative price changes in full employment periods. Prices are relatively flexible, over short time spans, for all resource services which have their origin in agriculture. While short lags exist, prices of breeding or feeder stock and feeds generally move in the same direction as prices of livestock products. Feed and breeding or feeder stock normally make up 80 to 90 per cent of the resources used in meat production. Accordingly, with a fall in the demand schedule from *b* to *a* in Figure 2, the supply function also falls, as from *y* to *z*, because of lower prices of feed and livestock resources. In crop production, the price of land services, as denoted by share rents, also parallels crop prices in flexibility even over the short span of a

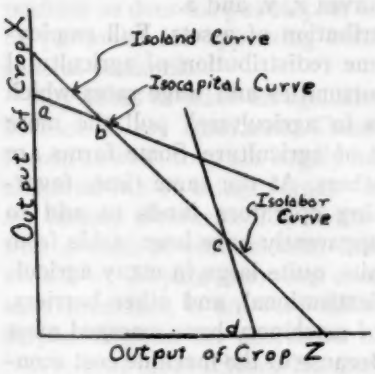

FIGURE 1

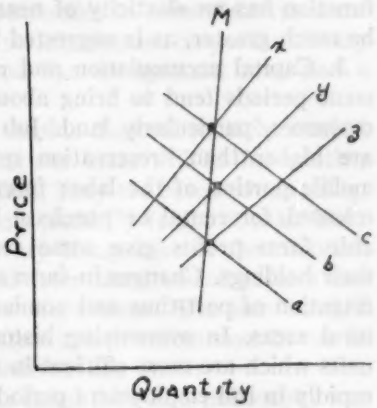

FIGURE 2

year. The reservation price of most land is near zero. Because of its limited supply in the agricultural production function, the price of its services parallels the price of its products. We have already indicated that in the short run a segment of the labor force in agriculture has low mobility and a low reservation price. Within limits, many established and older farm operators will take a lower labor return and stay on the farm rather than move to industrial employment where returns are higher. A similar situation holds true for a portion of the capital embodied in farm buildings and machinery. Over a short period of five to ten years, the major supply of building and machine services comes from stocks of these resources located on farms rather than from new stocks moved in from the machinery and lumber industries. Under declining prices, these "quasi-rent resources" are willing, because of their specialized nature, to take low returns. On the side of farm expansion (i.e., increasing price relatives), however, new machinery and building prices tend to be less flexible.[6]

[6] Numerous resources such as fertilizer and miscellaneous supplies have relatively inflexible

2. Technical change. Most technical changes, especially biological innovations, have the effect of lowering per unit costs of production; otherwise they would not be adopted. (Some techniques will be favored more in one price situation than in another; e.g., a machine technique will be favored in periods of high labor/machine service price ratios.) Hence, with declining price relative to agricultural products, output can be maintained even if the supply function is elastic. Starting from demand function c and supply function x in Figure 2 and with two successive declines in demand, the supply function may decrease to z because of technical innovation. Again, examination of changes in output will lead one to view "mongrel curve" M, and suppose the supply function has an elasticity of nearly zero. Actually, the elasticity may be much greater, as is suggested by curves x, y, and z.

3. Capital accumulation and redistribution of assets. Full employment periods tend to bring about some redistribution of agricultural resources, particularly land. Job opportunities and wage rates which are higher than "reservation returns in agriculture" pull the more mobile portion of the labor force out of agriculture. Some farms are released for rental or purchase by others. At the same time, favorable farm profits give some remaining operators funds to add to their holdings. Changes in farm size apparently have been, aside from retention of part-time and nominal units, quite large in many agricultural areas. In overcoming historic, institutional, and other barriers, units which are more efficient in use of machinery have emerged most rapidly in full employment periods.[7] Because of the machine cost economies associated particularly with crops, cost functions have tended downward. With declining costs, the pattern of Figure 2 may again be traced out to suggest "mongrel curve" M.

This paper has provided hypotheses about major variables affecting the elasticity of the supply function. Time has prevented discussion of other variables. Unfortunately, the paper has the limitations of many other journal articles. It is mainly a "set of literary hypotheses." Urgently needed is an empirical study which predicts the many coefficients in the set of structural relationships and which are necessary for meaningful statements about supply elasticity.

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prices, as compared to farm products. Whether or not the price is flexible makes little difference for a resource which is a technical complement to another resource which does have a flexible price and makes up a small portion of total inputs. For example, inflexibility in the price of livestock minerals, small tools, and similar items has little bearing on the use of such resources.

[7] Although there are, in the terms of Gislason ("The Nature of the Aggregate Supply Function of Agricultural Products, *Jour. Farm Econ.*, Vol. 34), still "a sequence of tasks directly related to cultural practices to be done," these can be accomplished as well by the consolidating farm as by the previous small owner.

A MODEST PROPOSAL FOR SURPLUS DISPOSAL

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The philosopher, T. V. Smith, when discussing matters outside his professional ken, takes care to speak "as an ignorant man and a philosopher." I do not yield to T. V. Smith in ignorance, but I am not a philosopher. The general economist, however, stands to the expert in specialisms like agricultural economics in somewhat the same position as does the philosopher to the social scientist. So perhaps I may be pardoned for appearing before you "as an ignorant man and a general economist."

Any ideas of mine on agricultural surplus disposal must be modest because of my ignorance. These particular ideas will be for another reason as well: they are largely unoriginal. They are based not on things unknown but on things forgot—and only partially forgot at that. They are largely a refurbishing of the McNary-Haugenism of the twenties, with a dash of econometrics borrowed from the forward pricers and a dash of Keynes borrowed from the Keynesians. The sum total, I have been told, is not very different from the practices now in use in surplus disposal for perishable commodities like butter.

A relapse into McNary-Haugenism is defensible. This philosophy was too radical and revolutionary for the Era of Wonderful Nonsense in which it originated, the nonsense then including federal budgets balanced at about 3 billion dollars. It has become conservative and old-hat in the present Era of Wonderful Nonsense, which includes support of everyone's prices and wages and letting inflation take the hindmost. Now Nonsense Can Be Wonderful and Life Can Be Beautiful, without excluding occasional deviations into sense, or into death.

Preconceptions of an Ignorant Man. An easy first step in solving the surplus disposal problem would be stopping all future surplus accumulation. This could be done, as we all know, by free markets, by forward pricing (in periods of high employment), by stringent acreage and output controls, or by crop destruction, etc. But the political strength of organized agriculture, the log-rolling skill of its spokesmen, the positive appeal of the parity idea, the negative appeals of subsidies, output limitation, and "killing little pigs" combine to preclude elimination of surpluses through lower prices or reduced output. Therefore I take for granted that we shall have with us a problem of disposing of increments to surpluses, as well as of existing or holdover surpluses,

substantially more than half the time for most of the major storable farm crops at going prices.

By a surplus I mean explicitly an amount larger than required as a disaster reserve for the United States or as the American share in a disaster reserve for the world at large. I mean, also, an amount larger than can be distributed feasibly in school lunches or poor relief anywhere in the world without distorting the normal markets for the goods we are considering. I recognize, also, sufficient limitations on farm bloc power to rule out as not only dictatorial but impracticable all suggestions to eliminate food surpluses by setting up compulsory "adequate" diets as liens on the disposable incomes of nonfarmers. In short, I am trying to render the surplus disposal problem nearly as difficult in my model as it is in real life rather than defining important aspects of the problem out of existence at the start.

The subject of our panel relates particularly to the international aspects of surplus disposal. Here again it would be easy to assume as the goal of our international economic relations a set of free-competition and free-trade policies which would simplify the issues tremendously. Free competition and free trade would restrict our surplus accumulations and decumulations within the dimensions of disaster insurance funds. They would outlaw as monopolistic any accumulations and disposals, at least by individual countries, intended primarily to influence either world or domestic prices. They would equate the domestic prices of all our agricultural imports and exports with world prices, adjusted for transport costs. I cannot disagree with Professor Johnson's position, for example,[1] that such are the logical consequences of free trade as applied to agriculture. But once again, I propose to assume instead that the farm bloc, having not only tasted blood but gorged upon it for the better part of a generation, will be able to veto many of these corollaries of free trade. Instead of free trade, then, I am asking you to assume with me that our international economic policy be limited to the less exciting goal of multilateralism—meaning effective competition by all exporting countries in the markets of all importing countries but normally not effective competition between foreigners and domestic interests. In this connection, the term country should be defined broadly enough to include customs unions like Benelux, temporary customs-union arrangements like the US-Philippine Trade Pact, and preferential areas like the British Commonwealth and French Union. But on one other point I must ask your indulgence for evading a problem which may be more significant than I

[1] D. Gale Johnson, *Trade and Agriculture, A Study of Inconsistent Policies* (Wiley, 1950), Chaps. 1, 2, 11; "Agricultural Price Policy and International Trade" (Princeton: *Essays in International Finance*, 1954), pp. 8-11.

believe it is. I wish to speak mainly in terms of a single abstract "agricultural product" and ignore special problems of individual crops and of complementary or competitive relations between them. I am thinking of this abstract agricultural product as a storable item, with low transport costs relative to its normal value. It is normally an American export item, which may, however, be imported if the American price is above the world price. It is a raw material for other products, agricultural or industrial, in which Americans compete with foreigners in both domestic and foreign markets. Wheat, corn, and cotton are three important feet which this shoe seems to fit quite well. I hope we do not lose too much by neglecting other feet of the agricultural centipede, which the shoe fits rather badly.

The Proposal. The leading features of the plan I wish to lay before you can be outlined under a few main heads.

Instead of supporting domestic prices by no-recourse loans, let a public corporation like the present Commodity Credit Corporation or the Hoover Farm Board purchase agricultural surpluses for cash at support prices. (I suppose that there will be such support prices, whether based on "parity" or not, and whether rigid or flexible in character. Among these various methods of supporting farm prices, I refrain deliberately from any choice. The surplus disposal problem is difficult enough "for an ignorant man and a general economist" without throwing in the entire "farm problem.") Title will rest clearly in this public corporation—let us call it Federal Agricultural Surplus Corporation or FASC, if we can stomach the fascist implications of the initials. FASC will then have full authority and full responsibility for disposal of surpluses.

At the end of every crop year, let the FASC set up a monthly schedule for the next twelve months of the sales to be made on the world market at world prices, whatever these may be. Let this international sales schedule be public knowledge. Let the FASC actually make these sales, either directly or through private exporters. (If sales are made to private exporters, they should be made by competitive biddings; i.e., at whatever prices exporters can afford at current and prospective world market prices. The roundabout procedure of selling to exporters at domestic prices and subsidizing their losses when they sell at world prices should be replaced by surveillance to prevent diversion of export purchases back to the domestic market. If the United States adheres to an international agreement governing the commodity, deliveries of American export quotas to the international commodity authority should not be included in the international sales schedule.) In years of disaster or the abatement of disaster at home and abroad, the FASC must of course be permitted to revise its international sales schedule

downward or upward for the remainder of the crop year, but only after the maximum practicable amount of public notice.

The aggregate sales during any normal crop year should exceed the increment to surplus in the preceding crop year, and therefore reduce the accumulated surplus on hand. The FASC should not, however, wreck the world market (as the Farm Board did in 1931-32) by dumping its entire accumulation at one fell swoop. It need never lower its normal holdings below a reasonable disaster reserve or "ever normal granary," as estimated by competent commodity specialists with a minimum of political interference. This reserve, of course, should not be held idle when disaster actually strikes, like gold reserves under some versions of the gold standard. They should be drawn upon to meet the drought, flood, war, or other emergency and replenished later.

Some arbitrary figures may illustrate how this scheme might work, how far the FASC can be guided by fixed rules, and how much discretion it must apparently be awarded by the nature of the problem. Let the accumulated surplus prior to the initial year of this plan (year 1) be 10 million units (bushels), including 2 million bushels from the crop of the year immediately prior (year 0). Let the safe disaster reserve be estimated (discretionarily) at 5 million bushels. Then the amount to be sold on world markets during year 1 might be from 2 to 7 million bushels, distributed by months according to the ordinary seasonal pattern of world demand. Suppose the figure chosen is 3 million bushels. This would leave the surplus at the beginning of the next year (year 2) at 7 million bushels plus the increment of surplus from crop year 1. If this increment of surplus exceeded 2 million bushels, annual sales by FASC in year 2 would presumably exceed the 3 million bushels of year 1; if the increment fell short of 2 million bushels, annual sales would presumably be less than 3 million bushels. But these are discretionary decisions, subject only to the general rule that sales in each normal crop year should exceed the increment to surplus from the last crop year whenever the total surplus exceeds the disaster reserve. (This simple illustration presumes a static situation, in which the disaster reserve remains constant over time. In actual practice, there will be increases and decreases in the reserve, depending on such factors as increasing population, changing foreign production, development of substitutes, improvement of transportation, revised probability assumptions, etc.) When the surplus falls, in year t, to 5 million bushels plus the increment of year t-1, the program of year t would be limited to disposal of the last year's surplus.

If, through any reason (disaster or miscalculation), the surplus at the beginning of year t should fall short of 5 million bushels plus the in-

crement from the previous year's crop, part or all of that increment would be retained unsold.

Should a disaster (crop failure, flood, drought, war, etc.) occur or be in prospect, the disaster reserves would of course be mobilized and not held idle. At the same time, at FASC discretion, the international sales schedule for the remainder of the crop year might be reduced or canceled. FASC should also be given discretion to increase or reinstate an international sales schedule when a shortage ends during a crop year or when a threatened crisis fails to materialize.

The heart of this proposal lies in the public announcement each August or September (say) of the FASC's monthly international sales schedule for the next year. If the disaster reserve has been dangerously depleted in prior years, there may be a purchase instead of a sales schedule. This procedure allows for dribbling, but not in most years for dumping,[2] the American surplus upon the world market at prices below American domestic ones. It withdraws the looming threat that the United States may wreck the world market entirely by disposing of its entire accumulation. Normally it exerts a constant downward pressure on world prices. This should not be belittled if world demand is inelastic, but on the other hand, when we consider the inelasticity of American domestic demand, this downward pressure may be little greater than it would have been in a completely free American market with the domestic price lowered to the world level. The added depressive element may also be offset in part by the added element of certainty implied by the scheduling of sales by the FASC. This certainty is itself not complete, covering only a year at best and subject at worst to drastic and sometimes destabilizing revisions[3] of the international sales schedule, in response to the rumors, presences, and abatements of droughts, floods, locusts, and wars.

Financing. Buying at high domestic prices and selling at lower world prices, the FASC is doomed to financial losses in normal years, which

[2] Our distinction between dribbling and dumping loses much of its validity in years of unusually large surplus accumulation. In the case of wheat, for example, the Commodity Credit Corporation's holdings amounted on June 1, 1954, to 878.6 million bushels as against 514.4 million on the preceding July 1. Disposal in the single year of 364.2 million bushels, or 70.8 per cent of the previous total, is hardly a dribble.

[3] As an example of destabilization, assume a failure of the Indian wheat crop, met in part by depletion of the American disaster reserve. The depletion is accompanied by a reduction of the American international sales schedule for the remainder of the crop year. The world wheat price, raised by the short crop in India, may be raised further by the reduction of the American international sales schedule. (The lowering of the American disaster reserve will be an offset, but usually not a complete one.) A similar argument applies on the downward side of the market when a crisis situation is concluded or avoided. Here the combination of American disaster-reserve and international-sales-schedule changes may lower world prices by more than they would have fallen with no revision of American policies.

will probably exceed its expected gains from operations with the disaster reserve. Under the present system, losses can be concealed for long periods by carrying surplus stocks, or nonrecourse loans based on them, upon the books of public corporations as assets valued at purchase prices. (During the political campaign of 1954, the total cost of the existing farm price support program since 1933 was often stated at less than 1.5 billion dollars, at a time when the Commodity Credit Corporation was holding over 6.1 billion of such questionable "assets.") The losses would be revealed almost immediately by any plan requiring, as this one does, drawing down of surpluses above a disaster reserve. (The disclosure feature is, in my judgment, a point in this plan's favor; many will doubtless judge otherwise.) At any rate, the problem of how the FASC deficit will be financed arises sooner and more continuously under this plan than under the existing system.

Prior to the Keynesian revolution of 1936, it was taken for granted that such deficits would be financed completely by farmers, or if not by farmers, by the processors of agricultural products.[4] This would avoid drain on the general funds of the federal government and conform to the period's concept of sound finance; namely, a balanced or slightly overbalanced budget of minimum size. A number of proposals satisfying this condition were made during the period 1921-32.[5] Of the few proposals which I have read, the most impressive appears to be the McNary-Haugen "equalization fee." Under this plan, federal marketing losses on each commodity would be financed by a specific levy[6] on each acre of land sown to that commodity by a farmer participating in the plan. The levy, which we may call an equalization fee, would be assessed whether the crop was marketed or used on the grower's farm, except that no farmer should be required to pay more in equalization fees than the difference between domestic and world prices on the quantity which he actually sells.

Participation in the plan and payment of equalization fees should both be voluntary. Nonparticipants, however, should not be permitted to sell on the domestic market at the supported prices. They should be restricted to foreign sales or to the use of their output on their own

[4] A typical statement of the underlying ethics is from George N. Peek and Hugh S. Johnson, *Equality for Agriculture* (Moline, Ill.: Moline Plow Co., 1922), p. 15, reproduced by Murray R. Benedict, *Farm Policies of the United States, 1790-1950* (Twentieth Century Fund, 1953), p. 210: ". . . agriculture ought not to produce a surplus which cannot be sold above cost of production, and if it does so produce it must stand the loss. Every farmer who produces contributes to that loss and ought to stand his share of it."

[5] Benedict, *op. cit.*, Chaps. 10-11. In addition to the equalization fee plan discussed immediately below, the "export debenture" and "domestic allotment" plans achieved wider support during this period than did any other proposals dealing at all seriously with financing problems.

[6] In conformity with some recent Farmers Union proposals, progression might be introduced into the levy as a measure to encourage family over corporate farming.

farms. Such a restriction as this was a feature of the domestic allotment plans of Professor Black and others during the late twenties.[7]

The equalization fee mechanism has a definite political advantage over financing from the general fund. It reduces somewhat the farmer's stake in high domestic prices, which he must now pay himself in part. It therefore reduces the conflict between the farmer on the one hand and the consumer or taxpayer on the other. This mechanism has also a corollary economic advantage over financing from the general fund in that it reduces the farmer's incentive to allocate his resources to the "wrong" crops,[8] whose prices are being supported above the level justified by the world demand for them. (This text has been criticized for failure to consider the export debenture plan, which would accomplish the same purposes as the equalization fee by a reverse mechanism. Under the export debenture system, the farmer receives the export or world price plus certificates whose value varies with the domestic price and the losses of the price supporting agency. The export debenture plan achieves greater simplicity than the equalization fee but is more difficult to adjust to modern fiscal insights, as indicated below. It also forces farmers to become speculators against their will and inconsistently with their abilities if they must sell certificates for cash before the value of the certificates is determined.)

There are also sound economic arguments in favor of general-fund financing of any FASC deficit. Modern fiscal policy teaches that a budget deficit may be a desirable means of increasing total expenditure when the national income is not keeping pace with productive capacity. General-fund financing of an FASC deficit would increase the flow of income at such times and should not be renounced in advance by exclusive reliance upon equalization fees or other schemes of self-financing. And even in normally prosperous times, a general-fund contribution to the normal FASC deficit may be useful to induce participation by small farmers who market relatively little of their produce.

The advantages of the equalization fee can be combined with the insights of modern fiscal policy, at the cost of some complications of administrative procedure. Each year's FASC deficit may be financed in the following year, partially from the general fund and partially by the

[7] John D. Black, *Agricultural Reform in the United States* (McGraw-Hill, 1929), Chap. 10. Also W. J. Spillman, *Balancing the Farm Output* (New York: Orange Judd, 1927), *passim*. Cited by Benedict, *op. cit.*, p. 267.

[8] Doubt has arisen regarding the quantitative importance of misallocation of resources between crops under the existing programs, once these resources have been committed to some form of agricultural activity. It is difficult to find farm products which are being "underproduced" because of the overproduction of those whose prices are being supported. Cf. J. K. Galbraith, "Economic Preconceptions and the Farm Policy," *Amer. Econ. Rev.*, March, 1954, pp. 46-48. Professor Heady's paper represents a rebuttal to these skeptical doubts, valid (in my opinion) for sufficiently long periods, less valid for the short term.

proceeds of commodity equalization fees. The proportion between the two parts, however, should vary with the business cycle in a sliding scale. The government should foot most or all of the bill when conditions are generally depressed; the farmer should foot most or all of the bill when inflation threatens the general economy. Farm income from price supported commodities may be expected to correlate with the national income more closely than farm income in general. It will be most unusual to find equalization fees major burdens on, say, wheat farmers in inflationary periods, even though income from wheat might

FINANCING THE FASC DEFICIT

IF GNP IN YEAR t EXCEEDS GNP IN YEAR $t-1$ BY	THE FASC DEFICIT OF YEAR $t-1$ SHOULD BE FINANCED IN YEAR t BY	
	Equalization Fee	General Fund
Less than 0%	0%	100%
0–2	20	80
2–4	40	60
4–6	60	40
6–8	80	20
More than 8	100	0

have moved quite inconsistently with the national income in the absence of support for wheat prices. The variable equalization fee and the variable government contribution would then operate as a small "built-in stabilizer" for the control of economic oscillations.

A possible basis for division of the deficit is indicated in the table. It depends upon the year-to-year rate of growth of the gross national product in current prices. Percentages above and below the long-term upward trend of national income or product would be more desirable, given a substantial measure of agreement upon the trend line itself. Variability is provided, but changes are to be made according to rules with little or no administrative discretion.

These figures are included purely as illustrations. Experts in agricultural finance may devise other schedules using the same principles and arriving at more equitable results.

In some years FASC operations will show surpluses. These will be years of scarcity in which disaster reserves are being liquidated at high prices. The surpluses should be held in special funds against subsequent deficits, in the manner of the Social Security Trust Funds. Accounts for each crop should be separated insofar as possible, so that the equalization fees paid on each crop will contribute to FASC deficits in the purchase and sale of that crop. Suppose that, in year t-1, FASC operations show a profit in cotton and a loss in wheat. Wheat farmers should then pay equalization fees, in year t, even though the over-all

FASC operations of year *t*-1 show a surplus, and cotton farmers should pay none even though the over-all operations of the FASC show a deficit.

Tariffs and Quotas. Free trade in agricultural products and in products largely manufactured from them is impractical so long as government policy holds the domestic and the world prices of agricultural products apart. It would be pointless, for example, to permit free imports of corn and pork while holding the domestic price of corn above the world price. The result of free trade in corn would be that the FASC would buy the entire American corn crop, while foreign corn would take over the entire American market for reasons completely unrelated to comparative advantage, relative factor endowments, or other determinants of the normal flow of free international trade. If foreign corn were excluded from the American market, free trade in pork products would drive the American hog raiser out of business because of higher feed costs, again without justification in the theory of international trade. Professor Galbraith (*op. cit.*, page 49) puts the same conclusion in reverse: after domestic prices have been raised, departure from free trade "restricts imports that have first been made artificially attractive."

This much is elementary. It is equally elementary that no fixed tariff rates, either specific or ad valorem, can serve as offsets for the differential between domestic and international prices when the differential itself is a variable. My suggestion, therefore, is that fixed tariff rates both specific and ad valorem, on both agricultural products themselves and their principal products, be replaced by schedules of flexible rates. The flexible rates should vary automatically as often as is practicable—every month, quarter, or year—with the differentials between the domestic and the world prices of agricultural products. The differentials should be computed as absolute amounts in connection with specific duties, and as percentages in connection with ad valorem duties.

This sliding-scale system is preferable to the alternative system of supplementing tariffs with import quotas and with tariff quotas. This alternative system, while more arbitrary, is simpler and is more favored in present American practice. A sliding-scale duty system has the advantage, however, of leaving the American market attainable to foreigners who lower their production costs by increased efficiency not connected with the differential between American and world prices, while the quota system makes no subtle distinctions among the causes of increased foreign imports. Furthermore, the sliding-scale duty system has the advantage of reducing discrimination between individual importers and exporters. The windfall rewards for getting particular consignments in under quotas will be reduced, and also the windfall

losses for getting them in late. A certain amount of discrimination, however, will remain as necessary consequence of periodical rate changes

International Agreements. We have been speaking of a two-price system, in which there are only world and domestic prices. This is an oversimplification—sometimes a dangerous one. If there is an international commodity agreement covering our agricultural product, exporting and importing countries will meet annual quotas and an international agency will accumulate disaster reserves. Export and import quotas will be met at an agreement price which, being fixed for a reasonably long period, will normally differ somewhat from the actual deal. The "agreement price" is itself usually double. There is a buying price for export quotas and a selling price for import quotas. The selling price is normally somewhat higher than the buying price. The administrative and storage expenses under the international agreement are wholly or partially paid out of the differential between the two prices.

In normal years, we are safe in supposing an agreement price lying somewhere between the high American domestic price and the low unregulated world price. In scarcity years, the agreement price will be the lowest of the three. In considering whether the United States should participate in such an agreement, attention should probably be concentrated upon the normal or surplus years. If circumstances are as we have supposed them, the agreement will then offer an outlet for part of the American surplus on terms more favorable than the world market. On the other hand, the United States will be required to finance a share of the administrative expenses and operating losses of the agreement. I can see no better way to determine American policy toward the agreement than the cold-blooded one: to weigh the probable gain in lower FASC losses against the probable loss in administrative contributions, to join if the former exceeds the latter and to abstain in the reverse event. In the interests of international comity, we should in no case join in years of export surplus and then withdraw or welsh on our delivery quotas when there is a shortage and the world price rises above the agreement one. Such double-dealing is currently ascribed (1954) to some importing countries (e.g., France), which joined the International Wheat Agreement when world wheat prices were high and refuse to accept their quotas when the Agreement price exceeds the free world price.

Bilateral Arrangements.[9] Pressure to dispose of farm surpluses by

[9] The writer has received substantial aid in preparing this section, particularly with regard to recent legislation, from a mimeographed paper by Professor J. S. Hillman which formed the basis of his essay, "Impact and Problems in Administration of Export Programs," *Jour. Farm Econ.*, February, 1955, pp. 96-109.

bilateral arrangements with importing countries is at least as old as the early twenties. For example, Senator George W. Norris introduced a bill into the 67th Congress (1921-23) establishing a 100 million dollar government corporation to buy farm products for cash in the United States, sell them abroad for foreign bonds, and then market the bonds in the United States. (Benedict, *op. cit.*, page 207.) A wide variety of such arrangements has been attempted. Unlike Nazi Germany and Soviet Russia, the United States has abstained thus far from large-scale direct barter agreements in specific commodities, but postwar legislation has been edging closer to the Nazi-Soviet model. During the Hoover administration, however, the Federal Farm Board not only sold wheat (to Germany and China) below world prices, but also bartered 25 million bushels for Brazilian coffee (*ibid.*, page 334). (This occurred prior to the Nazi elevation to power in Germany!)

The American bilateral arrangements have been of three general types:

1. "Tied loans" by the Export-Import Bank and other institutions, to finance purchases of American agricultural products (particularly cotton) for dollars at domestic prices. These have been part and parcel of American trade policy since the Export-Import Bank was founded in 1934. Even prior to the founding of the Export-Import Bank, the Reconstruction Finance Corporation made a tied cotton loan to the Soviet Union (*ibid.*). Foreign aid legislation since the first Economic Co-operation Act of 1948 has arrived at the same tying principle by indirection, the key provision being that no funds be used to finance purchase from third countries of agricultural products in surplus in the United States.

2. Earmarking of portions of American economic grants-in-aid, first to Europe and later to underdeveloped countries elsewhere, for purchase of surplus American farm commodities for dollars. Until 1953, purchases were at the domestic American price or the Commodity Credit Corportion acquisition price, whichever was the lower, but Section 550 of the Mutual Security Act of 1953 permitted sales at lower prices under bilateral agreements with the approval of the Department of Agriculture.

3. Sales of surplus American agricultural commodities for foreign currencies, which are then devoted to off-shore procurement of military goods and for purchase of strategic materials. The goods are ostensibly sold at domestic price equivalents in the absence of specific agreements authorizing lower prices, but the foreign-currency equivalents are often computed at unrealistic "official" exchange rates in such wise that a substantial discount enters the transaction in fact even when it is concealed on the surface. The most recent legislation on foreign surplus disposal, the Agricultural Trade Development and Assistance Act of

1954 (Public Law 480, 83rd Congress; approved July 10, 1954; 68 U. S. Stat. 454-59), is of this type.

Pedantically speaking, products sold abroad under these schemes include more than actual surpluses in public hands. They include, also, stocks which would have entered into surplus if not sold abroad. They include commodities like dried fruit, for which no price support or surplus accumulation program is in effect. It seems nevertheless permissible as a simplification to include all these categories as elements of surplus. Even in the most questionable category—products not under the price support umbrella—inclusion in bilateral arrangements seems to have been intended as a substitute for more direct government support of domestic prices.

Under all these programs, the enabling legislation includes a proviso against interference with normal American exports, with world market prices, and with the marketing of the agricultural produce of "friendly nations," including presumably the importing countries themselves. A friendly nation is defined in Section 107 of the Agricultural Trade Development and Assistance Act of 1954 as "any country other than (1) the U.S.S.R., or (2) any nation or area dominated or controlled by the foreign government or foreign organizations controlling the world Communist movement." If these provisos were never ambiguous and always taken seriously, they would limit the scope of these programs almost entirely to short-term emergency aids. Such, if I understand his essay correctly, is one of Hillman's criticisms of these programs: except under emergency conditions, they are so hedged in by these limitations as to be of insignificant quantitative importance. Hillman (*op. cit.*, Table 2) presents Department of Agriculture figures indicating a total of 245.1 million dollars of authorizations under Section 550 of the Mutual Security Act of 1953 for thirteen countries through July 8, 1954. During the same period approximately, total surplus holdings of the Commodity Credit Corporation rose by 2,633.0 millions, or ten times as much!

On the other hand, these limiting provisos are ambiguous and may in fact be disregarded at administrative discretion. They have been disregarded in the past, particularly as they protect foreign and not American agriculture. They have been disregarded either through ignorance of administrators or through political pressures upon them. It is safe to assume occasional disregard in the future as well. If the protective provisos are disregarded, more substantial surpluses may move under these programs at the price of increased international ill will and possible international retaliation.

The present (1954-55) rice situation in Asia illustrates the ambiguity involved and the leeway for administrative discretion. Since 1945,

American rice exports have risen from a prewar average of 80 thousand tons a year to 800 thousand tons in 1953. Much of the increase went for relief purposes; Japan was the principal recipient under bilateral arrangements. The program did not prevent a steady rise in world rice prices. The three leading prewar exporters had been Burma, Thailand, and Indo-China. For seven years following 1945, exports from both Burma and Indo-China were hampered by war damage and civil conflict. Beginning in 1953, however, Burman recovery turned rice prices downward. Continued pre-emption of the Japanese import market by the United States through 1955 may mean not only a break in world rice prices but also years of delay in the development projects of Burma and Thailand, which are financed largely by profits on rice exports, and a shift of Burman "neutralism" in an anti-American direction. At the same time, rice is one of the six "basic" commodities of American farm price policy. About 1.4 million bushels are expected to pass into surplus from the record 1954 crop. (Cf. "Too Much Rice in Hungry Asia," *Business Week*, November 6, 1954, pages 142-145.) Domestic political pressure has been strong for requiring Japan to import primarily from American surplus stocks as a condition for receiving American economic aid, since such action appears a passive matter of maintaining an American rice export market against Burman competition rather than as active matter of expanding American exports to areas traditionally served by Burma. Burma has previously never been an important source of rice for Japan except when the Japanese or Korean crops failed. In the decade following 1930, Japan obtained nearly two-thirds of her necessary imports from Korea, and approximately one-third from Formosa. (Cf. Jerome B. Cohen, *Japan's Economy in War and Reconstruction* [University of Minnesota Press, 1948], pages 368 f.)

Since the above paragraph was written, a draft Japanese-American aid treaty has been released to the press, according to which American pre-emption of the Japanese grain import market will be concentrated in rice substitutes (wheat and barley) rather than rice itself. This solution will dispose of American wheat and barley rather than rice surpluses. It will avoid economic conflict with Burma but will be resented by producers of rice substitutes (chiefly sweet potatoes) within Japan's domestic economy.

It is conventional to denounce bilateral arrangements in the interests of multilateral international trade. I follow this convention to the extent of omitting bilateral arrangements from the general surplus disposal program outlined here. There are, however, two sets of circumstances under which bilateral arrangements seem to have some legitimate place in a surplus-disposal program, however multilateral may be

our final or even our immediate ends. These circumstances can be spelled out explicity as "reservations."

1. A "distress" reservation. We agree on the permissibility of gratuitous gifts of agricultural surpluses to meet major, acute emergencies abroad. If the greater includes the less, it seems equally permissible to sell the same agricultural products at cut rates, to sell them for overvalued currencies, or to barter them for overvalued exports of the distressed countries, if the emergencies are less acute than those calling for international charity. But such sales should be permitted only with the agreement of other major exporters of the agricultural products concerned. In the Japanese rice deficit, no bilateral arrangement between the United States and Japan should go into effect without the approval of Burma, Thailand, Viet-Nam, and Cambodia, the principal rice exporters of the Far East.

2. An "inconvertibility" reservation, which can be illustrated from the recent history of Japanese cotton imports. Suppose American cotton cheaper than Pakistan cotton in Japan at existing official exchange rates, but suppose the Japanese pressured by sterling bloc regulations to purchase the more expensive Pakistan product. (Japanese sterling balances are freely available to purchase cotton from Pakistan, a sterling bloc member, but not to purchase cotton from America, which is not a member.) A dollar loan to Japan, even if tied to purchase of American cotton, is then less an interference with multilateral trade than it is a restoration of multilateral trade in retaliation against an interference by the sterling bloc.

Summary. While a candidate for his first presidential term in 1932, Franklin D. Roosevelt delivered his major agricultural policy address at Topeka, Kansas. He set forth in this address the requisites of an ideal farm program—which were not always to be met during his own administration. But the ideal is stated clearly, and we can use it as a kind of yardstick against which to measure the proposal just outlined.[10]

Roosevelt said that the ideal plan should provide for the American farmer the benefit of the domestic market without causing him to increase production. This proposal comes close to meeting this specification with regard to export commodities during periods of prosperity, when equalization fees are high. It falls short during depressed periods, when the fees will usually be lower and FASC losses met out of the general fund. While not positively encouraging farmers to leave the farm when there are ample opportunities outside, it does not encourage

[10] Franklin D. Roosevelt, *Public Papers and Addresses* (Random House, 1938), Vol. I, pp. 693-711. Cited by Benedict (*op. cit.*, p. 273), who suggests that Roosevelt may have framed his ideal with special attention to the domestic allotment plan.

them to stay there; it encourages them to stay where they are when unemployment is reducing alternative opportunities in industry.

Roosevelt said that the ideal plan should be self-financing. To current ways of thinking, self-financing is less obviously a virtue than it seemed in 1932. Our proposal, however, seems self-financing when it is desirable on fiscal grounds for it to be self-financing. It is not self-financing in periods when fiscal theory suggests monetary expansion through federal deficits.

Roosevelt said that the ideal plan should not use any mechanism that would cause retaliation on grounds of dumping. This proposal does involve the name of dumping, as does any other proposal to hold the domestic above the world price. On the other hand, it avoids one of the most undesirable features of dumping; namely, erratic and unforecastable onslaughts upon the world price level. Also, in view of the inelastic domestic demand for most farm products, the depressive effect of this plan upon world prices seems little more than would result from American exports in a free market with no protection for the domestic American price. As an additional feature, the plan encourages American participation in international agreements including both exporting and importing countries, and designed to insure both groups against the consequences of price fluctuations. Roosevelt said that the ideal plan should be voluntary. This plan is voluntary, at least in form, which may have been all Roosevelt had in mind.

Roosevelt also said that the ideal plan should make use of existing agencies and should operate on a co-operative basis. The restriction to existing agencies beats strangely upon ears attuned to Roosevelt's subsequent alphabetical extravaganzas, but I find it not unreasonable; my proposed FASC is little more than the existing CCC (and many a predecessor) reinitialled. "Operation on a co-operative basis" means that operating farmers should have a leading voice in the administration of the plan. Participation by farmers is good politics; it is apparently as easy to erect a window-dressing of dirt and dirt farmers for one plan as for another.

I cannot claim an "A" rating in agricultural economics for these random ideas by the rigorous Roosevelt standards of 1932, even ruling out such standards as I cannot accept. But I think I can hope for a "B," or at the very least a "graduate B minus." Even that would not be too bad, "for an ignorant man and a general economist."

DISCUSSION

D. GALE JOHNSON: In his "modest proposal," Bronfenbrenner has specified two conditions for the appropriate behavior of the United States if and when it dumps or dribbles its agricultural surpluses. One condition is that we should never sneak up on our adversaries, i.e., competing producers, but should let them know in advance that we are going to clobber them, what we are going to clobber them with, and how hard we are going to clobber them. The other condition is that Uncle Sam should play as small a role as possible in financing the dumping; US farmers should, under conditions of expanding GNP, pay part or all of the cost of the dribbles. In other words, in this competitive battle for export markets, the US should not allow her farmers to bribe the referee unless it is necessary to do so. Thus in this new set of Marquis of Queensbury rules for the dumping arena, it is not cricket to sneak up behind one's opponent and bash his head in without letting him know you are going to do it nor should the fantastically large resources of the US Treasury be used to make it certain that there cannot be a fair battle between US farmers and those of competing nations.

In my opinion, if we are to continue our foreign surplus disposal operations, there is no question that a set of rules or guides to appropriate conduct should be followed. On the whole—and not arguing that there have been no exceptions—it would appear that the administrators of our surplus disposal programs since the end of World War II have followed a code of behavior that has tended to minimize the undesirable side effects of our dumping operations. It has generally been fairly well known what our export price will be for wheat—the major commodity subject to export subsidization. And administrators have shown considerable restraint in using the omnibus authorizations such as contained in Section 550 of the Mutual Security Act or the more recent Agricultural Trade Development and Assistance Act of 1954.

Despite the past restraint and general good sense that has been shown in the administration of export disposal programs, I agree with Bronfenbrenner that a procedure should be adopted which minimizes the degree of uncertainty attendant upon such operations.

As a transition to my brief comments on Heady's paper, let me twit Bronfenbrenner for accepting a Galbraith economic preconception which turns out to be a misconception. Our "ignorant man and general economist" seems to accept the idea that relative prices have little or no effect upon the allocation of resources in agriculture since it is difficult to find farm products which are being "underproduced" because of the "overproduction" of those whose prices are being supported. What, pray tell, is a sensible definition of underproduction? Unless the government intervenes, in a competitive industry the amount produced or supplied is approximately equal to the amount demanded at some price. The price of beef cattle is not supported nor controlled by government action. How could one detect whether beef is being underpro-

duced? Since one cannot define underproduction in any sensible manner, it is perhaps not surprising that it is difficult to find it. After all, who can find the black cat in the dark room when the black cat isn't there anyway?

What is important here is the elasticity of the supply of agricultural output as a whole and of the various individual farm products. The implication of Galbraith's preconception is that all such elasticities are zero or so near to zero that the difference is unimportant. Heady would not accept such a view and the reasons he gives are convincing to me, which is hardly surprising since I was already convinced.

Heady's paper does much, in my opinion, to dispel the notion that agricultural output is so inflexible—the elasticity of supply so low—that disaster would strike agriculture if an attempt were made to relate agricultural prices to market demand and supply relationships, such as is encouraged in a forward price proposal or even actual free markets. But it is pertinent to point out that a low price elasticity of supply, either for one or all agricultural products, is not necessarily a calamity so great as to justify any measure designed to stabilize farm income. The responsiveness of farm prices—and thus of farm income assuming a low price elasticity of supply—to changes in the level of national economic activity is a function jointly of price and income elasticities of demand and price elasticity of supply. Thus the elasticity of farm prices with respect to changes in national income is equal (assuming constant elasticity functions) to the income elasticity of demand divided by the sum of the price elasticities of demand and supply. Since the numerator is generally quite small and in any case not much larger than the price elasticity of demand, the low supply elasticity does not necessarily imply that farm incomes will be more unstable over the business cycle than the incomes of other producers, even producers of those products with a perfectly elastic supply function.

It is true, of course, that when demand declines for reasons other than a decline in national income, the low price elasticity of supply is a curse if it is accompanied by a low price elasticity of supply. A similar statement may be made when the supply function shifts due to variables beyond the control of man.

DON KALDOR: Most American economists have been well nutured in the virtues of international specialization and trade. Among agricultural economists few have felt comfortable about farm programs that drive a wedge between internal and external prices. Consequently, it is a rare occasion when we hear one of our professional colleagues purpose a program of export dumping as a long-range solution to the farm surplus problem.

Professor Bronfenbrenner has presented a streamlined version of the old McNary-Haugen plan with its two-price system and equalization fee. The new version incorporates several additional features, however. It requires advance public announcement of specific export sale schedules. It lays down a general rule for determining the quantity to be offered. Losses on export sales would be financed by public funds and an equalization fee levied on producers. The proportions would be adjusted according to some measure of the deficiency in

aggregate demand. It provides for flexible tariff rates, and for certain restrictions on the use of bilateral agreements.

On the whole, these new features make a substantial improvement in the original plan. The provisions covering export sale schedules would help to reduce uncertainty and add some order to the disposal process. The suggestions on financing recognize the role of expenditure in generating income and the opportunity to induce a countercyclical effect. The restrictions imposed on the use of bilateral agreements recognize some, but not all, of the effects which export dumping can have on our international relations. But why impose these restrictions only on bilateral arrangements for export dumping? Since the objectives of the proposal are not made explicit, some question can be raised about the alleged advantages of flexible tariff rates. Under the assumptions made, any addition to domestic supply resulting from imports would necessitate an equal increase in the quantity dumped abroad. Therefore, encouraging low-cost production in exporting countries could not produce any direct gain for the US economy. Any gains made possible by lower costs would accrue to other importing countries. Nevertheless, as export dumping programs go, the proposal has considerable merit.

In setting the stage for his recommendations, Professor Bronfenbrenner warns against simplifying the issues by assuming as our goal of international economic relations a set of free competition and free-trade policies. But is the critical question really one of free competition. I suspect most economists are more concerned about the effects of export dumping on allocative efficiency than on free competition and free trade per se. The important question seems to be whether we are going to sacrifice some of the gains from a better allocation of resources implicit in a long-range dumping program.

Competitive markets are an institutional device for co-ordinating and integrating economic activities. When competition is perfect, the conditions for allocative efficiency are automatically satisfied. However, if competition is imperfect, a closer approximation to allocative efficiency might be achieved under arrangements involving a greater degree of government influence over the administration of resources. The condition that internal and external commodity values be related is a requirement of allocative efficiency, not free competition. Perhaps Professor Bronfenbrenner is saying that we should not be too concerned about over-all allocative efficiency.

He suggests that we focus on a new kind of multilateralism, one that involves competition between countries (which apparently means governments), but not international competition between producers and consumers. Does he mean that we ought to forget about the gains from a more efficient allocation of resources in world production and concentrate on the strictly short-run gains from exchange? Is it not true, however, that the gains from international specialization in production are really the significant ones?

The point is made that world prices would be much the same with export dumping as with free domestic markets. In the short run, this is likely to be true with commodities such as wheat and cotton. Under export dumping, however, there is likely to be a smaller long-run effect on domestic production.

When we are concerned with allocative efficiency the output effects become important. But free market pricing may not be sufficient to induce the amount of adjustment needed to establish a long-run balance. Export dumping, like other beggar-my-neighbor policies, tends to shift the burden of adjustment to other countries. It provides a powerful incentive to retaliation and economic warfare. Moreover, it is inconsistent with our postwar efforts to strengthen the free world.

Why has Professor Bronfenbrenner cast his solution to the farm surplus problem in an export dumping mold? The answer he gives is that other solutions fail to meet the test of political acceptability. Accordingly, he has excluded from consideration all solutions involving changes in support prices, adjustments in production, subsidies, and nutritional programs.

These techniques, as well as export dumping, all have been used to deal with problems of excess market supply. The emphasis placed on each has varied with changes in Congressional restrictions, the size and nature of the problem, technical suitability, and other factors.

What role does Professor Bronfenbrenner assign to his proposal? Is it conceived as an exclusive surplus control program? If the answer is no, is it considered a marginal program—one to be used only when other programs fail to eliminate a surplus? The answers to these questions have a bearing on its quantitative effects.

The proposal is designed with an eye to the storable products. Wheat and cotton are two of the three commodities mentioned. They are also the commodities that have been in most trouble under the support programs. Between 1951 and 1954, wheat stocks increased 250 per cent and cotton stocks rose 240 per cent. A decline in export demand was largely responsible. But during the past year important production adjustments have been made in wheat and cotton.

Under the marketing quota programs, almost 19 million acres have been diverted to other crops. Further diversions are planned or in effect for the coming year. With average weather conditions in 1955, wheat production is expected to fall short of meeting foreign and domestic demand at near current prices. These adjustments in production will go a long way toward correcting the recent short-run disequilibrium in the markets for these products. What is more, they were politically acceptable.

Obviously these programs are shifting the burden of adjustment to other crops. Since a large part of the diverted acreage has gone into feed grains and pasture, the effects ultimately will be transmitted to livestock. However, most of the commodities involved are not subject to mandatory price support. The Secretary of Agriculture already has announced reductions in support levels for a number of the commodities affected. Will lower prices prompt new Congressional action? Will it mean a general control program, a program to subsidize consumption, use of direct payments, greater reliance on export dumping, or some combination?

Recent passage of the Agricultural Trade Development and Assistance Act of 1954 might suggest that the emphasis would be on export dumping. This

was enacted by a Republican controlled Congress, and the Republicans traditionally have not been very international-trade-minded. On the other hand, the same Congress passed the Agricultural Act of 1954 which provides some flexibility in support levels for basic commodities beginning in 1955. What the new Democratic Congress will do to this legislation remains to be seen. But it would not be too surprising if it placed less emphasis on export dumping and more on production control.

At the present time, general use of direct payments is on the politically unacceptable list. However, their popularity seems to be on the increase. The recent Republican Congress authorized direct payments as a method of support for wool. This is an interesting development in view of the close association of this method with the plan proposed by Mr. Brannan. Currently, nutritional programs, except for school lunch and relief donations, do not have widespread support. But who can say with assurance what methods will be politically acceptable in the future. If broadly conceived and effectively carried out, the new farm and home planning program may prove to become an important adjustment mechanism.

Designing an optimum dumping program is a legitimate analytical problem, given the objective to be maximized and the restrictions to be imposed. But it would be unfortunate indeed if we were to confine our analyses and discussions of farm policy to what momentarily might appear to be politically expedient. As long as there are gaps in people's knowledge and imperfections in our system of representative government, we should not be overly concerned about immediate political acceptability.

LONG-TERM TRENDS IN INTERNATIONAL TRADE

SECULAR MOVEMENTS IN THE TERMS OF TRADE

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Interest in the problem of economic development has stimulated the study of long-run shifts in international terms of trade. The major purpose of these investigations, of course, is to analyze the economic forces which have caused the observed movements in these trading terms. However, before this analysis can be pursued with clarity and accuracy, two questions must be answered. First, what is the significance in a real income sense of the various measures proposed as indicators of the concept, "terms of trade," and what are the comparative merits of these measures? Secondly, how well do actual trade statistics measure these various analytical concepts? This paper is concerned with these two questions.

Concerning the matter of the real income or "welfare" implications of shifts in the terms of trade, the general position of major writers from at least the classical period to the time of Professor Taussig has been that a change in the terms of trade is presumptive evidence of a change in the amount of gain from trade.[1] Indeed, a rise in the ratio of export prices to import prices for a country has long been designated as a "favorable" shift in this price ratio. However, it should be noted that these earlier economists generally added that the presumption is subject to the absence of certain countervailing factors.

Although modern authors in international trade theory such as Professors Viner and Haberler are considerably more cautious concerning the welfare significance of changes in the terms of trade, there is still a large group of writers who are prepared to attach significant welfare connotations to movements in the usual measures of the terms of trade. The literature is full of references to the real income hardships which industrial nations will face if the ratio of their export to their import prices declines in the next twenty-five or fifty years. And the representatives of the underdeveloped countries also have been ready to proclaim the welfare implications of the past trend in the terms of trade. The following remark by the Chilean delegate to the Economic and Social Council of the United Nations (as reported on page 329 of the *Official Records,* Fourth Year, Session 8, 1949) typifies one posi-

[1] Jacob Viner, *Studies in the Theory of International Trade* (New York, 1937), p. 555.

tion: "But the deterioration, over a long period, of the relation between prices of raw materials and those of manufactured goods must be borne in mind. The result was a constant decrease in purchasing power and capacity for development in the countries producing raw materials."

In view of this wide association of the terms of trade with the concept of economic welfare and also the warnings of most outstanding theorists in international trade that there are some limitations on the employment of this measure for such purposes, it may be useful to analyze the welfare meaning of changes in the terms of trade a little more carefully. The early classical writers refer to the terms on which a nation trades by comparing the number of units of productive services of a foreign country whose product exchanges for the product of one unit of the productive services of the home country. Being dominated by a real-cost theory of domestic value (although asserting its inapplicability in the international sector), it was only natural for the classical writers to express the terms of trade in this fashion. Professor Viner called this measure of the terms of trade the "double factorial trading terms" (*op. cit.*, page 561), while Professor Robertson has labeled them the "true" terms of trade ("The Terms of Trade," *International Social Science Bulletin,* Spring, 1951). However, since these early writers generally assume constant and historically stable costs, they frequently use the commodity terms of trade, i.e., the ratio of export to import prices, as a shorthand representation of changes in this more fundamental concept. Today this is the measure which almost invariably is utilized in statistical analyses of international trading terms.

Early writers relate changes in the terms of trade to changes in real income in only a general way. Ricardo, for example, argues that the gains from trade consist of an increase in the mass of commodities and therefore in the sum of enjoyments, and he merely implies that an increase in the ratio of export to import prices leads to an increase in these enjoyments. However, J. S. Mill, who has exerted a powerful influence on subsequent study in this field, goes much further (*Principles of Political Economy,* Book III, Chapter 18). He wishes to measure the gains from trade exactly, in order to explain the manner in which these gains are divided among the trading parties. Taking a simple two-commodity–two-country model, Mill asserts that the differences in the opportunity cost ratios between two countries is an index of the total gains from trade. Therefore, the relation of the commodity terms of trade with these cost ratios shows the manner in which this total gain is divided between the two countries.

It should be noted that Mill is concerned with changes in the division of trading gains, not changes in the absolute gains to each party.

Only if the cost conditions remained the same in each country would he be willing to correlate the two. For example, when he assumes an increase in productivity in one of the countries and discusses the possible resultant deterioration in this nation's trading terms, he refrains from asserting that the country has suffered an absolute loss. Mill seems to recognize quite clearly here the inadequacy of a change in the commodity terms of trade as an index of a change in the absolute gains from trade.

Mill, of course, realizes that in order to connect the terms of trade and real income, one first of all needs a method of measuring changes in a nation's welfare. Moreover, for Mill the task is made especially difficult, since he wishes to explain how the total gains from trade are divided among the trading parties. Therefore, he needs an absolute measure of trading gains.

His particular measure of these gains, i.e., differences in opportunity cost ratios, is entirely unacceptable to present-day economists. It inadequately accounts for both the total utility derived from consuming various combinations of the two commodities and the total disutility involved in producing the items.

Moreover, almost all modern economists feel that there is no completely satisfactory manner of accounting for these factors and thus for measuring changes in real income or welfare in an absolute sense. Today, it is usually assumed that utility can only be measured in an ordinal sense. Thus, modern welfare economists only make statements concerning the direction of a welfare change, and then only under rigid assumptions. For example, as far as a country is concerned, it is said that, if tastes remain constant and if there are more of all commodities (or more of some and no less of others) in one position than in another, this indicates a potential increase in real income. The word potential should be stressed, since the question of the distribution of income bars any outright statement unless assumptions concerning interpersonal utility comparisons are made. Of course, if tastes change, they would not make even this weak statement.

In view of these objections, Mill's use of opportunity cost ratios as an index of the gains from trade and the commodity terms of trade as an indication of the way gains are divided must be rejected. Furthermore, the whole theoretical concept of the gains from trade is of little practical significance. All agree that the concept deals with the difference in some welfare sense between the collection of goods a nation actually produces and consumes with the existence of trade and what it would produce and consume in the absence of trade. Setting aside the problem of finding a measure of absolute welfare, it still would be impossible to determine the no-trade position in actual practice. The only

concept that one can discuss in the real world is a small change in a country's real income as the actual trading situation varies. Such changes can be quite different from a change in the theoretical meaning of the gains from trade.

The basic classical concept of the terms of trade, i.e., the double factorial terms, makes intercountry comparisons of these actual real income changes in the international sector. If one country's double factorial terms improve and thus its trading partner's (assuming there is only one) deteriorate, the interpretation is that the first country has gained more (or lost less) than the second country. This, of course, rests on the assumption that the welfare change of one country can be compared with that of another. But if we assume for the moment that utility is measurable in a cardinal sense and comparable among countries, this measure could give the wrong answer. For example, taking two countries (treating them as individuals), two commodities, and assuming that both countries have the same tastes, it is quite possible that if one of the two countries suffers a deterioration in its double factorial terms of trade—resulting, say, when there is a proportionate increase in productivity in the export industry of each country—this country could increase its utility more in a cardinal sense than the country reaping the better terms. For, while the country with the improved trading terms increases its imports (in physical terms) proportionately more than the other nation, this does not mean its utility for these imports increases more. One should not assume cardinal utility changes are proportionate to quantity change.

Professor Prebisch, for example, appears to illustrate this misleading type of conclusion when he complains about the secular fall in the commodity terms of trade of the underdeveloped countries. (United Nations, Department of Economic Affairs, *The Economic Development of Latin America and Its Principal Problems* [Lake Success, 1950].) He argues that, even though productivity in the export industries of developed nations has increased faster than that in underdeveloped countries, the commodity terms of trade have moved against the peripheral nations. Therefore, he concludes, "while the centers kept the whole benefit of the technological development of their industries, the peripheral countries transferred to them a share of the fruits of their own technical progress." Quite clearly he means that the real income gains through trade in the peripheral countries have not been as great as in more developed nations. But, accepting his assertion on the actual movement of the commodity terms of trade, his conclusion by no means necessarily follows. A major reason for the declining export prices of the peripheral countries relative to the export prices of the center would seem to be a high price and income demand elasticity for manufactured

products and a low price and income demand elasticity for primary commodities. The peripheral countries together with the industrial nations helped to keep up the prices of industrial commodities by their comparatively large increases in demand for these goods as real income rose. Although the quantity of imports of industrial nations rose relatively more than those of underdeveloped countries, perhaps the result would be that the latter group received a greater increase in utility for their imports than the developed nations, if these utilities could be measured in a cardinal sense and could be compared.

While most economists are reluctant to employ terms-of-trade measures such as the double factorial as indications of the relative trading gains among nations, many seem willing to give meaning to shifts in the terms of trade merely as an index of changes in the direction of a particular country's real income position. The commodity terms of trade, the income terms of trade, and the single factorial trading terms are examples of measures utilized for this purpose. But there are severe limitations on the range of welfare statements that can be made on this level without selecting some social welfare function for the country. Moreover, even with such a function, none of the above measures always indicates the direction of change of a nation's real income as affected by foreign trade. Nevertheless, there are differences in the merits of these various terms-of-trade concepts for long-run studies, given some judgment concerning the distribution of income within the country and thus a well-defined social welfare function.

Changes in the commodity terms of trade can be completely misleading. Frequently, a country, such as Great Britain during the first half of the nineteenth century, has been carried into international prominence on the wave of a secular decline in its commodity terms of trade. And the history of many a successful business is one of striving for more unfavorable terms of trade. The point, of course, is that the commodity terms of trade fail to take explicit account of changes in export productivity and changes in the volume of exports.

The influence of classical thinking is too great in regard to this matter. In their international trade analysis these writers generally assume, first, a constant supply of resources operating within a given state of technology and, secondly, a purely competitive, fully employed, domestic economy where all the optimum marginal conditions are satisfied. Under these conditions, a change in the export-import price ratio has some claim to be considered as an indication of a change in a country's real income in the international sector. But these assumptions are completely inappropriate for long-run analysis and, for that matter, even for most short-run analysis. Take the matter of the optimum conditions. Obviously, every factor does not earn the same return in all

of its possible different uses in both the developed and, particularly, the underdeveloped nations. A country may expand its exports partly by drawing on factors in less productive employments, incur an unfavorable movement in its commodity terms of trade, and still raise its real income significantly. This has been a frequent pattern of economic development. To study the long run with a series which can give a rough idea of what is happening only when population, capital stock, and the state of technology remain the same is equally misleading.

Another aspect of the static classical model is that it ignores the dynamic repercussions of the international sector on the domestic economy. An increase in the importation of some item essential to domestic development may force import prices up and cause a temporary decline in real income. But the long-run effect of these imports may be to add greatly to the real income in the domestic sector of the economy. In brief, a change in the commodity terms of trade is a useful indication of a change in real income as affected by foreign trade only if all other things remain the same. But in the long run, changes in the commodity terms of trade frequently are accompanied by other changes in the international sector which counter the real income effects due merely to changes in the commodity terms of trade. To concentrate upon the terms-of-trade element in real income and to exclude other relevant variables is highly inappropriate in analyzing long-run shifts in real income.

The widely expressed view on the part of the representatives of the underdeveloped countries that measures must be taken to stop the secular decline in their commodity terms of trade may well lead to disastrous policy decisions. And the same applies to those in the developed nations who seem obsessed by a fear of long-run deterioration of their commodity trading terms.

The obvious limitations of the welfare conclusions to be drawn from changes in the commodity terms of trade have led to a number of alternative measures designed to rectify these drawbacks. One measure, which can be computed quite as easily from the available statistics on trade as the commodity terms, is an index of the total value of exports divided by an index of import prices. This indicates changes in the volume of imports which can be purchased through the export trade. It has been termed by Viner as the total gain index (*op. cit.*, page 563); by G. S. Dorrance as the income terms of trade ("The Income Terms of Trade," *Review of Economic Studies*, 1948-49, No. 39); and (perhaps most appropriately) by the Economic Commission for Latin America as the capacity to import (Secretariat of the Economic Commission for Latin America, *Economic Survey of Latin America, 1949*). However, while it must be granted that a recognition of the volume of trade

is important for an appraisal of trading gains, the attempt to combine this element with the price element in a single composite measure can lead to quite incorrect conclusions as a measure of the direction of real income charge for a country. For example, if the export price level remains unchanged while the quantity of exports increases and the price of imports also increases proportionately, the index will not change. Yet simple, static indifference curve analysis tells us that these are precisely the conditions under which an individual would be worse off. *Ceteris paribus,* a rise in this index could mean either an improvement or worsening of a country's real income position. For this reason it is a poor measure of real income shifts.

Another measure, which attempts to include the influence of productivity in determining the direction of gains, is the single factorial terms of trade: the commodity terms of trade multiplied by an index of the productivity of the factors of production in the export industries. Professor Robertson *(op. cit.)* feels that this measure is the one best suited for a discussion of changes in a country's real income as affected by foreign trade. One problem with the concept, as has been stressed recently by Ely Devons, who has statistically measured the single factorial trading terms for the United Kingdom over the last twenty years ("Statistics of the United Kingdom Terms of Trade," *The Manchester School,* September, 1954), is the ambiguity associated with the term, "the productivity of a unit of the factors of production." It is difficult to define. This obstacle, of course, also is present with the double factorial terms of trade. Efforts in this field have been confined to measuring the productivity of labor—the measure Devons employs—and are not entirely satisfactory for the purpose. However, even if this measure is used in computing the single factorial terms of trade, an investigator is faced with a lack of adequate data in this area for anything but recent years. Furthermore, like the other measures, this one cannot always reflect the true direction of welfare change, even treating a country as an individual with well-defined indifference curves. It should be noted, however, that the last two measures of the terms of trade can rise and fall for everyone simultaneously.

Do all of these objections force one to the conclusion that all efforts to discuss the effects of long-run changes in foreign trade on real income should be abandoned? I would say it does not. Modern welfare economics does not conclude that almost all efforts to measure changes in real income are futile. It merely points out that on the basis of very weak value judgments, which are almost universally accepted, one cannot go very far in making definite statements. But there are many stronger value judgments involving assumptions concerning interpersonal comparisons of utility which are acceptable to the large majority

of policy-makers and affected parties and under which many definite welfare statements can be made. The economist must employ these judgments, if he wishes to have a significant role in policy making or in interpreting history in anything but a rather trivial sense. (Judgments concerning the welfare significance of new commodities and changes in tastes are also necessary, if any definite statements are to be made.) However, he should explicitly state these judgments and also point out the welfare conclusions which follow from different sets of these interpersonal utility value judgments. Then the reader can make a conscious choice in deciding what real income conclusions he wishes to accept. Too often both the writer and the reader are not consciously aware of the value judgments upon which the conclusions entirely depend.

These remarks, of course, are relevant to all studies of changes in real income. But, as has been noted, the investigator in the terms-of-trade area is faced with additional difficulties associated with the narrowness of his topic. It would seem to me that what little information can be gleaned from the foreign trade sector should not be further constricted by merely studying the ratio of export to import prices. It does make a difference to a nation whether its import or its export prices are changing; to withhold the knowledge of this information from the reader by presenting only a ratio (as is often done) appears unwarranted in view of the limited significance of changes in the ratio. Furthermore, a study in this field should be implemented by the use of export and import volume indices, productivity changes in the export and internal sectors of the concerned economies, and the host of other economic indicators which help to make up a comprehensive study of the total economy. In other words, we should not concentrate merely on the international sector if we wish to make meaningful statements about real income changes which are caused by foreign trade. The total economy should be studied in the context of a dynamic, non-optimum view of the development process. Nor should volume and productivity figures be combined with price data into single ratios. They can all be misleading. Certainly every bit of what can be seen by these ratios can be discovered without them, plus a good deal more.

It should be noted that these criticisms relate to the employment of the various terms-of-trade measures as indicators of long-run gains in foreign trade. There are, of course, other purposes for which these ratios are used—such as in studying balance-of-payments problems and capital movements. For such purposes these measures may serve as useful tools. However, in such analyses, the limitations of these concepts as real-income measures should be explicitly stated.

In addition to the many theoretical limitations involved in relating

changes in trading terms to changes in real income, there are a host of statistical problems connected with long-run studies of the terms of trade—particularly those dealing with pre-World War I figures. A major difficulty, of course, is the lack of comprehensive and accurate data. For example, Werner Schlote in his study of the foreign trade of Great Britain *(British Overseas Trade from 1700 to the 1930's* [Oxford, 1952]) employs Jevon's commodity index—an unweighted series of only forty commodities—in his computation of current import values from 1814 to 1854. This import value series, and the import price index he derives from it with the aid of a volume series, must be regarded as a very crude trade measure. Professor A. H. Imlah ("Real Values in British Foreign Trade, 1789-1853," *Journal of Economic History,* November, 1948, and "The Terms of Trade of the United Kingdom," *Journal of Economic History,* November, 1950) has pointed out some of the inadequacies of these estimates, while also doing much to improve them. But, as he notes, price and value series for these years can only be regarded as very rough measures of the true relationships.

However, the drawbacks of series used to depict trading terms for later and even quite recent years are not quite so obvious. H. Staehle ("Some Notes on the Terms of Trade," *International Social Science Bulletin,* Spring, 1951) notes that in the case of the foreign trade of the Belgium-Luxembourg Customs Union during 1948 where complete records (representing about 25,000 punched cards) were kept, import and export price indices varied significantly between a sample based on all commodities accounting for at least 1/1,000th of the total value and one based on 1/10,000th of total value. In other words, these price series seem to be highly sensitive to variations in coverage.

Furthermore, a price series based on a limited sample of similar commodities throughout a fairly long period, such as A. G. Silverman's index of British export and import prices from 1880 to 1913 ("Monthly Index Numbers of British Export Prices, 1880-1913," *Review of Economic Statistics,* August, 1950), tends to be biased in an upward direction because of the omission of new commodities introduced during the period examined. It is reasonably well established that the price history of a new product, relative to previously existing commodities, is one of rapid decline in its early stages of production. While new items probably do not enter international trade as soon as they do domestic trade, it would seem that the exclusion of these products from a commodity sample underestimates the relative price decline of the total commodity list. This factor, of course, applies to both import and export indices. An intensive study would be needed to determine whether the upward bias was greater on the import or the export side for a particular country in a particular period; e.g., Great Britain from

1880 to 1913. But since the composition of Britain's exports underwent a greater change than its import trade in this period, the suspicion is that this factor tends to cause a greater upward bias to export prices than to import prices.

A related point is the choice of weights in the computation of a price index. For a given array of commodities, price indices based on beginning-year weights tend to have an upward bias compared to price indices based on end-year weights if the changes in the prices and the changes in the quantities are negatively correlated, i.e., price changes result from changes in cost conditions, and a downward bias if the changes in the prices and the changes in the quantities are positively correlated, i.e., price changes are due to relative changes in demand. As R. G. D. Allen observes (*International Trade Statistics* [New York, 1953], page 195), the usual case with price indices of actual time series is that the index employing beginning-year weights shows an upward bias compared to one using end-year weights.[2] However, a study of the direction of the bias and its impact on export prices compared to import prices should be made on the widely used British figures.

Similar problems to those mentioned above also arise with the widely used procedure of computing price indices on a unit value basis. Although new commodities automatically enter the broad category groups in both the volume and actual value series, in this measure of price change, they are not weighted by their correct prices. Analogous inaccuracies also occur when there are compositional shifts within the promiscuously broad unit value categories—a frequent occurrence in any long-run series. And, of course, this index number is also subject to the relative bias problem associated with the choice of weights.

The matter of accounting for quality changes is still another obstacle to be faced in a study of the terms of trade as in almost all studies of price and quantity series. While there is no satisfactory method of handling the problem, all agree that a correct picture cannot be obtained without recognizing their great importance. In a terms-of-trade study the significance of this point lies in the generally accepted view that quality improvements have been greater in manufactured products than in primary commodities. If this is true, it tends to impart to the typical statistical measure a systematic bias which makes changes in the terms of trade of nations exporting manufactured goods and importing primary goods more favorable than they actually are. Certainly, this point applies to the example of the much-cited British data from 1870 to 1913.

[2] Viner in his study, *Canada's Balance of International Indebtedness, 1900-1913* (Cambridge, 1924, pp. 231-232), found that Canada's export price index based on beginning-year weights rose to 135.6 in the end year compared to 120.2 for the price index based on end-year weights.

A final point related to this British data is that the United Kingdom values imports c.i.f., i.e., inclusive of transportation charges, while it values exports f.o.b., i.e., exclusive of these charges. Therefore, a change in British import prices need not entirely reflect a change in the prices which foreigners received, since shipping costs on imports were in part paid by British importers to British shippers. Furthermore, on the export side there is no provision for the inclusion of British shipping services purchased by foreigners. A terms-of-trade index which included both merchandise and shipping services would give a more accurate picture of Britain's commodity terms of trade. An estimate of the importance of this point in the period 1870 to 1913 is as follows. Professor Imlah's index of net merchandise import prices fell 32 per cent and his index of domestic export prices 18 per cent between these two years. Thus Britain's commodity terms of trade rose 19 per cent (Imlah, *op. cit.*). However, net shipping earnings during this period were equal to about 22 per cent of domestic exports. Furthermore, freight rates fell about 50 per cent between these two years. (A. K. Cairncross, *Home and Foreign Investment, 1870-1913* [1953], page 176.) A rough estimate of merchandise and shipping terms of trade can be made if the import index is left as it is but shipping services are introduced in the export index. This new index increases only about 12 per cent between the two years rather than 19 per cent.

These statistical difficulties reinforce the analytical arguments urging a cautious use of long-run export and import price series. It is bad enough that the real income significance of changes in the commodity terms of trade is frequently misinterpreted, but it is even worse that the data on which these conclusions are drawn may not even indicate the true directional change. For example, it has been generally asserted that Great Britain's commodity terms of trade improved between 1870 and 1913. But an appraisal of the limitations of the statistics on which this conclusion is based leads one to doubt whether this statement is really true.

THE LONG-TERM FUTURE OF UNITED STATES IMPORTS AND ITS IMPLICATIONS FOR PRIMARY-PRODUCING COUNTRIES

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I. *Introduction*

Recent discussions of future United States imports have been largely focused on specific policy aspects: our requirements of strategic materials; "trade, not aid" as a solution—or a palliative—for the world's dollar problems; tariff policy and its relation to domestic production; etc. The present paper is not concerned with problems of United States import policy. It discusses, rather, the significance of certain prospective or inferred changes in the volume and the real value of our imports. Summarizing a series of projections made by the writer in a study prepared at the Federal Reserve Bank of New York,[1] this paper first discusses a method of estimation that is largely based on individual commodity studies rather than on aggregative trend projections. Next, the actual estimates and their implications for the United States' propensity to import and for the future of the terms of trade are taken up in turn. It will be found that both the downward trend of our import-income ratio and the deterioration of primary-product prices relative to those of manufactures have been reversed. Finally, the implications of a large increase in United States imports for the economic development of primary-producing countries will be discussed. The argument suggests that the marginal importance to developing countries of increased exports is much greater than is usually realized; moreover, the operation of a number of "multipliers" tends to raise the cumulative benefits of increases in exports far above the initial value increments.

II. *A New Attempt at Projecting the Value of United States Imports*

Methods of estimating the future demand for imports have been the subject of much professional discussion. A session devoted to this topic at the 1952 AEA meetings (*Papers and Proceedings*, May, 1953, pages 135-166) once more exemplified the difficulty of deriving elasticities and import functions from historical time series. Conceptual and statistical problems, however, are not the only obstacles. No long-range trend projection from any given historical experience can yield satis-

[1] "The Secular Increase of United States Imports and World Trade" (Balance of Payments Division, May 13, 1954, mimeographed).

factory results if the relationship between any of the basic variables is subject to significant change.

Recent investigations of our raw material base reveal that the last decade marked the beginning of a structural change that is characterized by a widening gap between the growth rates of demand and of domestic supply of several major commodities. Resulting shifts in the supply elasticities of domestic and foreign products tend to reduce or void the usefulness of rigid projections of aggregate import propensities derived from structurally different periods. The purpose of this paper is not to argue this view at length, although the traditional preoccupation with short-term elasticities has tended to divert attention from the factors involved in long-term estimation.[2] This point has had to be made, however, in order to explain the writer's tendency to rely only to the smallest possible extent on aggregative trend projections in his attempt to estimate the value of future imports.

As a partial substitute, the import values of about thirty major commodities, which account for approximately two-thirds of our imports, were individually projected. The estimation of import volumes followed closely the course adopted by the Paley Commission (*Resources for Freedom*, June, 1952) which, it will be recalled, estimated imports as the difference of projected demand over prospective domestic supply. In this respect, an important underlying assumption should perhaps be made quite explicit; namely, that import demand is in fact a residual. This implies that imports are drawn upon—even in the long run—only to the extent necessary to supplement domestic supply; in other words that, on the whole, imports are marginal to the domestic production of competing products.

This tacit assumption has found support in recent publications concerned with United States demand for imports.[3] In addition to the statistical evidence mentioned therein, this proposition actually has a solid foundation in American import policies. The interplay between protec-

[2] We really know little about long-term elasticities except that they are probably greater than in the short run. It is surprising that Professor Harberger should claim in his discussion below that demand elasticity is "well below unity." He was prominent among the economists who showed that all known methods lead to underestimation. (See Arnold C. Harberger, "Index Number Problems in Measuring the Elasticity of Demand for Imports," *Econometrica*, July, 1950, p. 275, and Fritz Machlup, "Elasticity Pessimism in International Trade," *Economia Internazionale*, February, 1950 pp. 127 ff.) Moreover, the most elaborate study of US imports yet made points to elasticities very close to or even exceeding unity, not .5 as Professor Harberger claims. (See J. H. Adler, E. R. Schlesinger, and E. Van Westerborg, *The Pattern of United States Import Trade since 1923*, pp. 70 ff.)

[3] See G. Lovasy and H. K. Zassenhaus, "Short-Run Fluctuations in United States Imports of Raw Materials, 1928-39 and 1947-52," International Monetary Fund, *Staff Papers*, October, 1953, p. 281; Arnold C. Harberger, "A Structural Approach to the Problem of Import Demand," and Hans P. Neisser, "The United States Demand for Imports," both in AEA *Papers and Proceedings*, May, 1954, pp. 146 and 154 f. Also discussion by J. H. Adler and H. K. Zassenhaus, *ibid.*, pp. 160 and 164.

tionist forces and countervailing interests and considerations has brought about a precarious, yet curiously enduring, balance. As a result, domestic producers of raw materials can count on a market for most of what they can produce within a fairly narrow range of average costs, while the economy is spared the cost of extreme protection. The dust of battle frequently seems to obscure the long-range operation of such a system of checks and balances in United States postwar policy. Sometimes, though rarely, this principle manifests itself in as explicit a fashion as the well-known recommendation of the National Petroleum Council that imports should supplement but not supplant domestic production. It appears realistic and not at all unreasonable to incorporate such a sociopolitical stabilizer as a datum into a system of foreign trade, in the form of an assumption that imports are marginal to domestic production.

Yet this relationship between domestic output and imports cannot operate independently of the price mechanism. If demand increases faster than domestic output and prices tend to rise, imports become more attractive within a given tariff and freight rate structure. Conversely, if prices fall, a specific tariff or a rigid freight component imposes a relatively greater burden on the foreign product and tends to shift the competitive advantage in favor of the domestic supplier. Similarly, the forces that make for substitution of one commodity for another can only be appraised in conjunction with price considerations. It therefore seems strange that the Paley Commission excluded relative price changes from its model, by adhering to the price structure of 1950. As the Commission was only concerned with import volumes, this assumption was perhaps not too harmful, especially since in some of the Commission's commodity studies the substitution effects of relative price changes were not entirely ignored. In projections of import values, however, relative price shifts can hardly be neglected without impairing the usefulness of the results from a realistic angle. This deficiency becomes apparent in an earlier attempt at translating the Paley estimates into value terms. (See Eugene R. Schlesinger, "The Long-Run Outlook for United States Merchandise Imports," International Monetary Fund, *Staff Papers*, February, 1954.) The choice of 1950 as a base year makes the assumption of unchanged price relations particularly unrealistic, since the Korean emergency had distorted the price structure in a singularly arbitrary manner. Whenever Schlesinger uses 1950 import values as the basis for projections, the distortions of trade volumes in that year tend to add to the disparity because the second half was already affected by the scramble for imports that began after the Korean invasion. Moreover, apart from certain recent important commodity developments, Schlesinger ignores sizable revisions in the population and

labor force estimates that have been made since the Paley report was published. As a result of such discrepancies and some differences in assumptions, my value projections range from 10 to 35 per cent higher than Schlesinger's interpretation of the volume projections in the Paley report.

On the other hand, an attempt to project commodity prices over long periods of time appears, at first sight, to face formidable obstacles.[4] Upon closer scrutiny, however, a number of specific factors tend to narrow the range of contingencies within the empirical economic framework that characterizes individual commodities. While it is not possible to summarize here the considerations set forth at great length in my earlier study, it can be said that the key to price estimation was found in the matching of the prospective domestic supply with estimated total demand, with due consideration to long-term technological trends.[5] Through numerous discussions with commodity specialists in industry and government, a fairly broad range of prices, not a single price, was established for each commodity in accordance with a fair consensus of experts, based on a partly intuitive, but to them quite tangible, appraisal of long-term cost trends in their industries. In line with the assumed marginality of imports, the lower limit of the range is determined by real domestic production costs, since the projected domestic output would not be forthcoming below a figure satisfactory to a requisite number of producers. Conversely, the upper limit of the range is reached when only a small increase of domestic output could be expected from a price rise to a point at which foreign imports would become available in sufficient quantity. Professor E. S. Mason, a member of the Paley Commission, has expressed the opinion that foreign reserves of metals are large enough to eliminate the likelihood

[4] It is essential to distinguish—as Professors Kindleberger and Morgan fail to do in their discussion below—between price estimation in the short and the long run. Daily commodity price changes are based on expectations that arise from imperfect knowledge of actual demand and supply. In the long run, supply elasticity is high and cost relationships are the prime determinant. It is therefore easier, not more difficult, to project price trends over the long term rather than for the next six months or so, especially if a reasonably broad range, not a single price, is assumed.

[5] Technological change can be projected with suitable precautions, since it is neither as erratic as Professor Harberger implies, nor as unpredictable as Professor Morgan believes. Industry experts are well aware of the "crucial technological elements in economic progress" to which Professor Harberger refers in his discussion below. The time table of technological change, especially in extractive industry, is not fortuitous but planned many years in advance. The technology of years hence has been on the drawing board or in experimental stages for a long time, and more remote possibilities for coming decades are continually explored. On the whole, very little that can become a sizable economic factor within the next twenty years is not already known in outline and thus accessible to appraisal by the experts. As Professor Harberger points out, the extent of change and its economic effects vary from industry to industry. This is why disaggregation is essential for realistic projections. The greater degree of aggregation that Professor Harberger seems to prefer would fail, precisely because we cannot expect that differential change can be "averaged" realistically and consistently.

of price increases (in real terms) during the next twenty to twenty-five years. (See "Raw Materials, Rearmament and Economic Development," *Quarterly Journal of Economics*, August, 1952, page 332.)

In the case of commodities not manufactured in the United States, similar factors that shape world supply and demand had to be substituted. For foods, the consumption of individually-projected import items was based on long-term trends of per capita consumption and on a population projection somewhat below the highest recent estimate of the Census Bureau.[6] Finally, in order to project the remaining one-third of United States imports not covered by individual commodity estimates, a modified system of trend projections was adopted, partly based on unpublished postwar coefficients computed in the Federal Reserve Bank of New York.

III. *The Outlook for United States Imports*

The Gist of the Projections. Table 1 summarizes the projections for a reasonably normal year in the 1970's when the United States gross national product would, by assumption, have reached a level of 615 billion in 1953 dollars, twice the level of 1950[7] and a modest figure in the light of recent official and unofficial estimates and targets for 1965. The value of United States imports in constant dollars about twenty years hence is estimated at from 2.5 to nearly 3 times the value of imports in 1948—the year of largest postwar imports before the Korean emergency. Using for comparison the values that are based on prices considered as "most likely" by the experts, this corresponds to an annual rate of increase of imports of 3½ to 4 per cent from 1948 to 1975. If petroleum, which accounts for 37 per cent of the projected increase during that period, is tentatively eliminated from the com-

[6] *Current Population Reports, Population Estimates*, Bureau of the Census, Series P-25, August 21, 1953, p. 5. Population in 1975 was assumed to reach 215 million. The census estimates range from 199 to 221 million, the lower part of the range being unlikely except in the case of relatively low economic activity. The highest estimate implies the maintenance of about the predepression age-specific fertility rate rather than the postwar birth rate to which Professor Mason refers in his comment on Colin Clark's population estimate of no less than 234 million in 1975. (See Colin Clark, "Afterthoughts on Paley," and Edward S. Mason, "Comment," in the *Rev. Econ. Stat.*, August, 1954, pp. 270 and 276.) According to the Census Bureau, all of its latest estimates may be considered conservative, and it will be recalled that all earlier census estimates had erred on the low side.

[7] The original table included three columns under each price assumption. These alternative estimates had to be omitted for reasons of space. (While the lower volume projections reduce the import total but very little, the higher assumptions would increase total imports for 1975 in the three columns of Table 1, respectively, by 2.0, 1.6, and .9 billion of 1953 dollars.) The three price columns refer to the range of prices established by the commodity specialists; the lower and upper ends of the range are perceived by the experts as possible but not very likely, while the price designated as "most likely" carries considerably greater probability weight. It should be noted that this range represents, as it were, the significant section of a probability range, in place of a single long-run price estimate; it is not meant to delimit the amplitude of short-term fluctuations, which might well be much greater.

TABLE 1
UNITED STATES IMPORTS—1975, 1948

	1975 (In Millions of 1953 Dollars)			1948 (In Millions of 1953 Dollars*)
	Low Price	"Most Likely"	High Price	
Crude Materials and Semimanufactures†				
Aluminum and bauxite	235	256	273	62
Copper	496	453	494	214
Iron ore	400	438	508	29
Jute and burlap	143	156	168	168
Lead	149	171	193	112
Lumber	141	151	161	193
Newsprint	778	812	846	449
Nickel	186	226	266	65
Oils and oilseeds and waxes	202	217	233	174
Petroleum and products	4,969	4,989	4,730	444
Rubber	515	560	605	332
Tin	130	140	149	144
Woodpulp	288	313	325	327
Wool	465	531	605	349
Zinc	126	136	160	43
Other crude materials and semimanufactures	2,423	2,762	3,042	1,484
Total	11,646	12,311	12,758	4,589
Foodstuffs				
Cocoa	204	259	316	209
Coffee	2,064	2,279	2,494	741
Sugar	386	432	478	377
Other crude and manufactured foodstuffs	2,006	2,169	2,384	799
Total	4,660	5,139	5,672	2,126
Manufactures	2,126	2,339	2,658	754
Total imports‡	18,432	19,789	21,088	7,469

* Adjusted by US wholesale price index (for 1953 estimated at 110.5 basis 1947-49).
† Includes in 1948 manufactures of aluminum, asbestos, as well as mica, unmanufactured and manufactured. It was assumed that these imports will have been replaced by domestic items in 1975.
‡ Figures may not add to totals due to rounding.
SOURCE: 1948: US Department of Commerce, *Monthly Summary of Foreign Commerce of the United States*, January–December, 1948, page 32.

parison, the annual rate of increase of imports still remains above 3 per cent. Such a prospect appears of sufficient magnitude to encourage the following three main lines of speculation.

Growth Trends. The rapid increase of imports since 1949 for which the post-Korea import boom at first seemed to be alone responsible now appears merely to be the forerunner of sizable future growth. In fact, after the passing of the Korean emergency and in spite of a reduction of commodity prices, our imports were well maintained in 1953 and are

likely to show no more than a slight drop in 1954, our recent recession notwithstanding. In the perspective of our projections a further doubling of the recent import level by 1975 is well within the realm of possibility.

The postwar period appears to mark a distinct change in the rhythm of our imports, from a period of near stagnation to one of rapid growth. From 1926 through 1948 our imports barely grew at all in terms of constant dollars,[8] but on the basis of our projections they might increase by 3½ to 4 per cent annually from 1949 through 1975. This change of pace can be best, though only partially, understood against the background of our greater dependence on certain raw-material imports. Steeply growing imports of petroleum, iron ore, nonferrous metals, and many others are clearly in store. It is, however, noteworthy that offsetting factors are always present, so that—perhaps surprisingly—in our projections for 1975 the combined share of crude materials and semimanufactures in the total just barely rises above the 62 per cent mark reached in 1925-29. (Newsprint and burlap, classified as finished manufactures in United States statistics, were included in semimanufactures in this calculation, which was derived in current dollars from the source mentioned in footnote 8.)

The "Propensity to Import." While the Paley report foretold a rapid increase of raw material imports, it did not imply that the volume of imports would grow as fast as the gross national product. The inclusion of a greater variety of imports and the present method of projecting values instead of volumes afford an altogether different perspective. It now appears that the succession of declines of the import-income ratio that used to be considered a historic trend has been arrested and a further slight upward tendency may be expected for the future.

Since in the past much has been made of our "declining propensity to import," a few additional remarks on this issue may be in order. Table 2 presents the percentage ratio of total United States imports to gross national product in current dollars for the last eighty years and adds the ratio for 1975 implied by our projections. It appears that the historical decline took place in somewhat discontinuous steps, interrupted by lengthy periods of stability like that from the turn of the century to the Great Depression. In view of this progression, one might well wonder why this phenomenon should have been so commonly interpreted as a "secular" trend without adequate investigation into the specific factors which have been responsible for the decline in each individual historical period.

[8] Current dollars adjusted by United States wholesale price index, calculated on the basis of John H. Adler, Eugene R. Schlesinger, and Evelyn Van Westerborg, *The Pattern of United States Import Trade Since 1923* (1952).

TABLE 2
RATIO OF UNITED STATES IMPORTS TO GROSS NATIONAL PRODUCT
(In Current Dollars)

Decades	Year	Percentage	
		(1)	(2)
1869–78		7.1	
1879–88		6.0	
1889–98		5.9	
(1894–1903)		(4.9)	
1899–08		4.7	
1909–18		4.9	
1919–28		4.8	
1929–38		3.3	
	1929		4.2
	average 1937–40		2.7
	1948		2.7
	1952		3.1
	1975		3.2–3.5

SOURCES: Column (1) GNP. 1869–1918 decade estimates, 1919–38 averages of annual estimates of GNP, from Simon Kuznets, *National Product Since 1869*, Table II 16, page 119. Imports: *Historical Statistics of the United States, 1789–1945*, Column M 68, pages 247 ff.
Column (2) GNP: 1929–48 from *The Economic Report of the President*, transmitted to Congress January 14, 1953, page 165; 1952 from *Federal Reserve Bulletin*, November, 1953, page 1212. Imports for consumption 1929–40 from *Statistical Abstract of the United States, 1941*, page 527; 1948 from *Monthly Summary of Foreign Commerce*, January–December, 1948, page 22; 1952 from *Quarterly Summary of Foreign Commerce of the United States*, January–December, 1952, page 23.

One might expect that certain structural shifts which characterize the growth of the American economy would bring about a change in our import requirements. Technological displacement is frequently mentioned as a reason for lower imports, but this argument can be overdone, since technological change is not necessarily import-replacing, as it was in the case of silk and natural rubber, largely under the spur of wartime shortages. For instance, displacement of steel by aluminum in certain uses might well spell increased imports; so would a trend towards alloy steels requiring greater quantities of such imported additives as nickel, chrome, and others. At present, our advanced industrial technology tends rather to bring about a growing use of commodities that we cannot, or can no longer, produce at home in sufficient quantities, except possibly at steeply rising real costs.

The previously discussed marginality of imports may have been partly responsible for reversing the downward trend of the import-income ratio. Coincidental with the dollar devaluation in 1933-34, imports of crude materials and semimanufactures began to lag progressively behind industrial production, while the domestic output of import-competing crude and semimanufactured materials increased. (See G. Lovasy and H. K. Zassenhaus, *loc. cit.*, Chart 1, page 272 and page 286.) Although other factors also entered into the picture, it

would appear that imports were proportionately more affected by the decline in total demand and their share in raw material consumption shrank. By the same token one might also expect that rising incomes would call for a more than proportional increase in imports.[9] This appears to be precisely what we have been witnessing recently. A first substantial rise of the import-income ratio has already taken place, as Table 2 shows; although fluctuations will undoubtedly occur, the projections imply that no renewed decline need be anticipated for the long run and that a further slight increase may be in store.

The Future of the Terms of Trade. The extended range of projections adds perspective to another controversial problem: trends in primary commodity prices and the terms of trade of underdeveloped countries. It will be recalled that this topic has been frequently discussed in international meetings, especially of the United Nations, in relation to problems of financing economic development. A United Nations study (*Relative Prices of Exports and Imports of Underdeveloped Countries* [1949], especially page 121) claims to show that the prices of primary commodities have declined 40 per cent relative to those of manufactured goods over a period of seventy years preceding the second World War. Another report (*Economic Survey of Latin America for 1949* [1951], page 55 and following) suggests that there is an inherent tendency for this price relationship to deteriorate with each successive recession, a theory that is certainly not substantiated by the recessions of 1949 and 1953. The statistical basis for price series going back a long way is open to considerable doubt, and a more recent United Nations report (*Commodity Trade and Economic Development* [1953], page 12) by a panel of experts is much more cautious in its inferences and warns of drawing conclusions for the future from past tendencies.

On the other hand, secular changes of the terms of trade are undeniably of great importance to primary-producing countries as a determinant, not only of their national income, but also of their ability to import consumer goods and development goods. It is therefore relevant to note that the trend which turned progressively unfavorable to primary commodities in the decade preceding the second World War has since been decisively reversed. Table 3 shows the course of import unit values as an index based on the years 1937-40, and Table 4 re-

[9] A rising share of services in national income is often mentioned as a reason for a less-than-proportional rise in imports. But the share of services in United States national income is no longer rising. On the contrary, the share of services in total personal consumption expenditure declined from 40 per cent in 1929 to 34 per cent in 1953. (See *Economic Report to the President*, January, 1954, p. 172.) We are a nation of "doers"; thus even increased leisure does not necessarily mean a smaller share of goods. The contrary may be true—witness the do-it-yourself boom with its increased sale of tools and materials.

TABLE 3

UNIT VALUE INDICES OF SELECTED UNITED STATES IMPORTS BY COMMODITY GROUPS

(Average 1937–40 = 100)

	1948	1975		
		"Low"	"Most Likely"	"High"
Crude and semimanufactured materials	207.4	267.3	300.6	321.4
Crude and semimanufactured materials, excluding petroleum	197.0	198.3	220.7	243.0
Crude and maufactured foods	353.9	434.6	486.4	538.2
Total	233.3	292.7	330.6	357.8
Total, excluding petroleum	226.9	249.6	280.0	308.8

SOURCE: Table 1.

lates this index to one of unit values of United States exports of finished manufactures. It will be seen that by 1952 the terms of trade of crude and semimanufactured materials had improved about one-third above the prewar base. The price relationship of foodstuffs had turned nearly 200 per cent more favorable, largely owing to the unusual price rise of coffee and cocoa. On the whole, the terms of trade

TABLE 4

RELATIVE PRICES OF SELECTED UNITED STATES IMPORTS AND UNITED STATES EXPORTS OF FINISHED MANUFACTURES BY COMMODITY GROUPS*

(Indices, average 1937–40 = 100)

	1948	1975		
		"Low"	"Most Likely"	"High"
Crude and semimanufactured materials	90.9	141.8	147.7	149.9
Crude and semimanufactured materials, excluding petroleum	86.3	116.4	121.3	125.8
Crude and manufactured foods	155.1	230.6	239.0	251.0
Total	102.2	155.3	162.4	166.9
Total, excluding petroleum	99.4	146.6	153.8	159.9

* Import unit value index divided by export unit value index of finished manufactures. See footnote 1 for details, especially the use of an "imputed export price index" for 1975.

SOURCE: Table 3.

of United States imports of the selected raw materials and foods had improved nearly 60 per cent above their prewar base by 1952. "Terms of trade" is here used to describe the price relationship of commodities —primary versus manufactured—rather than, as is customary, that of imports and exports of countries. They are based on the Paasche type indices of United States imports shown in Table 3.

This development restores to primary products the level of price

relationships in United States commodity trade that appears to have prevailed about forty to fifty years ago, depending on which set of statistics is used.[10] Moreover, the passing of the post-Korea commodity price boom, while affecting certain commodities materially, has not, on an average, brought about a return to prewar relationships. Most important, as Table 4 shows, the long-run outlook for the next two decades suggests that the improvement of the terms of trade over the prewar level is likely to be maintained and, possibly, even slightly increased.

As a result, one may feel encouraged to believe that the adverse trend of the depression, and possibly of earlier decades, has been reversed—not passingly but decisively. To use the word "permanently" in this connection would appear too bold, since temporary declines in line with the rhythm of world prosperity cannot be excluded. Thus instability may well remain a very real problem. Moreover, various commodities and, hence, different producer-countries must be expected to fare differently over time. However, the danger of secular deterioration of the terms of trade of primary products imported by the United States appears no longer as the inexorable threat envisaged by the interpreters of earlier data.

IV. *Implications of Increased United States Imports for the Economic Development of Primary-Producing Countries*

Greatly increased exports to the United States would appear to hold out alluring prospects to potential supplier countries. However, among the less developed primary-producing countries a suggestion that exports might be profitably increased meets often with distinct disfavor, largely based on adverse historical experience. Since the volume of exports depends on foreign demand, great fluctuations were experienced in the past, with disastrous results for economies that are dependent on export proceeds for nearly all their essential needs. In such countries, exports tend to be viewed as a symptom of imbalance, in contrast to the goal of greater diversification to which the underdeveloped areas generally aspire. Taking this widespread attitude into account, the following section analyzes the gains that greater exports seem to offer to the internal economy of primary-producing countries.

A near doubling of imports by the largest importer in world trade is bound to have profound effects on supplier countries. The impact will be greatest where the export sector provides a sizable share of the

[10] A long-term series computed by the Pan American Union puts the average price index of Latin-American exports of raw materials and foodstuffs relative to United States wholesale prices in 1953 at 105.7 (1900=100), approximately equal to the 1897 level. See "Terms of Trade of Latin America: Their Development and Prospects," September 15, 1954 (mimeographed).

national product. As a rule, therefore, the less developed primary-producing countries will be most significantly affected. However, in recent years some doubts have been cast upon the gains ultimately realized by such countries from foreign investment in export industries. (For example, H. W. Singer, "The Distribution of Gains between Investing and Borrowing Countries," AEA *Papers and Proceedings*, May, 1950, page 474 ff.) It may thus be worth while to review the opportunities that may arise for primary producers from a growth of exports to the United States as large as that implied in our projections.

In discussing the benefits of trade for the primary-producing countries, it may be well to distinguish clearly between national income aspects and balance-of-payments effects. To be sure, the availability or the lack of foreign exchange for specific imports may also, in the final analysis, affect the growth of production and hence of incomes. This latter mechanism, however, is more roundabout than the income effect of exports and of export-related investment, which will be discussed first.

Income Effects of Increased Exports. In an attempt to appraise the scope of income propagation that arises from increased exports, it may be useful to revert to fundamentals. The following brief sketch will therefore be couched in terms akin to traditional multiplier analysis, even though statistical verification could hardly be successfully undertaken. Larger production for export increases the income of an economy's export sector. Higher incomes in the export sector, in turn, tend to bring about a secondary round of income creation, followed by other cycles in the well-known multiplier sequence. Since the marginal propensity to consume is usually quite high in less developed areas, one may feel inclined to expect a multiplier effect of a high order.

In reality, however, a structural peculiarity of underdeveloped economies probably deprives the multiplier of some of its strength. The area in which this mechanism can freely operate is limited to the monetary sector of the economy; the subsistence sector tends to remain outside the main stream of income propagation. Initially, therefore, the cumulative effect of rising incomes in the export sector may not be as large as one would expect on theoretical grounds. By the same token, however, the effectiveness of the multipler mechanism is apt to grow apace with the widening of the market economy and economic development generally.

Still other impediments and leakages hamper the multiplication of income growth originating in the export sector. The narrow productive base of an underdeveloped economy makes for a low elasticity of supply, especially in the short run. Where backward methods and economic organization obstruct the growth of supply in response to greater

demand,[11] a part of the income gain may be frittered away in price rises. Inflationary pressures that result from the rise in exports and its secondary monetary effects thus would tend to reduce the size of the aggregate income gain in real terms.

If, on the other hand, excess demand is met by greater imports, this leakage will also reduce the multiplier. It seems reasonable to assume that the average and the marginal propensities to import are quite high in underdeveloped countries, especially among the high-income groups (particularly if foreign travel and a proclivity to keep balances abroad are taken into consideration). A rapid expansion of incomes would tend to add to this propensity if profits increase more than wages; in that event, a large part of the increment might be spent on luxury imports, while the lower income groups probably concentrate more on domestic staples. To the extent that the distribution of income would thus affect the propensity to import, it matters considerably whether an increase of export values is due to a change in volume or in price. As a rule, a greater export volume brings about an increase in wages and profits, while higher prices raise profits only, at least in the short run. Hence, since profit earners in the less developed countries tend to import more and also to save more, one feels encouraged to speculate that an increase of export prices makes for a smaller multiplier than a growth of export volumes. On the other hand, such savings from higher prices and profits offer greater opportunities for income growth in the long run, depending on the upper income groups' propensity to invest.

Income Effects of Export-related Investment. Since our projections point to a substantial and sustained rise in American imports of numerous commodities, it stands to reason that sizable investments in the producing countries will be required to achieve the requisite growth of output in the foreign producing areas. A large share of the capital will undoubtedly have to be contributed by the industrial countries to whom a large part of the increased output would eventually be shipped. On the other hand, it is believed that the producing countries derive little benefit from such investment because the multiplier effects "took place not where the investment was physically or geographically located but (to the extent that the results of these investments returned directly home) they took place where the investment came from." (H. W. Singer, *loc. cit.*, page 475.)

There is considerable value in this much neglected approach. Nonetheless, it may be useful to probe somewhat deeper. Undoubtedly the multiplier effect of foreign investment is greatly reduced by virtue of the fact that most, if not all, of the equipment is of foreign origin. Thus

[11] See V. K. R. V. Rao, "Investment, Income, and the Multiplier in an Underdeveloped Economy," *Indian Economic Review*, February, 1952, pp. 58 ff.

the import leakage tends to be large; in fact, it may be conceptually permissible to eliminate such "investment in kind" from the investment total and ponder only the income effects of local expenditure. This includes, in all instances, construction and sundry labor and frequently also tools, accessories, working inventories, etc. Moreover, such investments often require collateral investment in transportation, power, and other fields. Experience shows that this local investment component is quite large—no matter whether it is financed from abroad or domestically.

In such a perspective, there is no reason to doubt that this kind of investment is apt to have cumulative effects on income that resemble those of the purely domestic type, provided such investments keep recurring on an increasing scale—as they well might have to in order to produce growing outputs. Moreover, this effect is additional to the previously discussed income-creating effects of increasing export increments.

A related argument appears to refer to the type and localization of investment. It is said that investments in export industries contribute less than other types to knowledge, external economies, and the integration of the economy. (See H. W. Singer, *loc. cit.*, page 475, and comments by Ragnar Nurkse, *Problems of Capital Formation in Underdeveloped Countries* [1953], page 84 ff.) Varying conditions probably tend to make this statement less valid in certain instances than in others. In any event, it does not appear practical to discuss specific investment propositions in terms of alternatives. As a rule, the host country has only the choice of refusing raw material investments or of making the most of the opportunity by demanding a fair share of the proceeds for its own developmental purposes.

Finally, the suggestion that the remittance of investment incomes is an undesirable drain on the host country's capital resources deserves attention. In the event of investments made in order to increase exports to the United States, the foreign exchange aspect can safely be ignored. From any other angle, the important factor for the economic growth of the producing country is not the amount of profits transferred but the amount retained and reinvested. As long as the demand for the product increases—and our projections point in this direction—expansion will be necessary and is apt to be financed from retained earnings. Recent statistics of the Department of Commerce indicate that, lately, the percentage of earnings reinvested by United States companies operating abroad have been quite high, probably upwards of 50 per cent (*Survey of Current Business*, December, 1953, page 10). To be sure, the part of profits that is withdrawn does not directly contribute to the host country's welfare. On the other hand, this is no more

than the price paid for securing the income-raising benefits that actually accrue to the economy from three sources: the original investment, reinvestment of earnings, and increased exports. Although their combined multiplier effect may not be as large as one might expect on abstract grounds, it is likely to be substantially in excess of unity.

Foreign Exchange Aspects. As a country's population and its income grow—owing to increased production for domestic use or for export—more imports are required to match growing needs. Increased domestic production may gradually reduce import requirements of consumer goods, but more capital goods and raw and intermediate materials are needed to promote further development. Therefore, the capacity to import of developing countries has lately received a great deal of attention.

In view of such growing exchange needs, it seems unfortunate that developing countries frequently do not look with favor at increased trade in raw materials as a source of additional foreign exchange receipts. The historic reasons are, of course, well known. They can be summed up under two headings: the danger of overspecialization and the burdens of overdependence. On the other hand, what are the more desirable alternatives to greater exports? Greater foreign investment—even less welcome than trade from the angle of overdependence—cannot be relied upon to increase at a sufficiently steady rate to match the needs. Foreign aid on the requisite scale is even less dependable and much less acceptable to self-reliant nations as a permanent feature. This lack of choice makes it desirable to investigate whether our projections imply a measure of relief from the alleged dangers of increased reliance on exports.

Our projections suggest a fundamental reversal of previously unfavorable trends that had made export specialization appear so perilous. While the possibility of future fluctuations cannot be excluded, their degree and duration will be subject to more effective control by deliberate antideflationary policies in the industrial countries. Moreover, downswings tend to be more moderate in periods of long-term growth, such as those to which all projections point—provided proper policies are adhered to. Furthermore, the prospective shift in the American propensity to import promises to give the full benefit of our rate of growth to our suppliers as a group, though not necessarily to every country. Even more rapid increases of demand are expected elsewhere. (The expected increase of manufactured goods imports by the United States would indirectly increase the exports of raw materials to this country's suppliers.) Finally, as stated before, the terms of trade, too, have already shifted significantly in favor of primary-pro-

ducing countries. On the average, no long-term worsening appears to be in store and further improvement is quite possible.

Producers of products facing favorable demand prospects in this country and elsewhere can thus look forward with greater confidence than ever before to a sustained growth of their export proceeds, provided they can keep in line with cost and productivity trends in their industry. Given such conditions, it is hard to see why they should continue to shrink from promoting the growth of their exports, save for a somewhat amorphous fear of an "overdependence" that is sometimes characterized as "economic colonialism."

Two elements appear to stand out to justify such fears. First, the relative size of the export sector is seen as a structural defect which places the country at the mercy of factors beyond its control. To increase exports would seem to increase the imbalance and make the country still more dependent. Second, the additional foreign investment required to bring about the growth of export facilities seems to enhance the foreign element in countries that strive for greater self-reliance, both financial and hence—as they see it—also political. As a subsidiary consideration, the argument is heard that the export industries drain resources of manpower and skills—perhaps also of capital—that could be used to greater advantage in development projects of a more general nature.

The force of these arguments diminishes if we look beyond the first stage of the growth process. To be sure, both the ratio of exports to total product and of foreign to domestic investment may initially increase when the export sector is expanded. However, as exports grow, the producing country can secure the wherewithal to expand the other sectors of the economy. Deliberate policy in this direction may be needed and has already become commonplace, as the governments of producing countries have proved themselves increasingly adept in exacting a large share of profits and foreign exchange proceeds from exports. If these are used for economic development, the export sector tends to shrink in relative importance, even though it grows in absolute terms. The share of foreign investment also declines as domestic activities increase. (For example, with regard to Mexico see International Bank for Reconstruction and Development, *The Economic Development of Mexico* [1953], page 16 and Table 12, page 183.)

Investment in export industries and in other developmental activities thus need not be considered mutually exclusive alternatives. Each has its function to perform. A growth of export industries is uniquely suited to provide income and foreign exchange with which a progressive diversification of the economy can be financed. In this manner, exports

gradually help reduce the overdependence that they seem to increase in the early stages of development.

The Gains from Trade. Given appropriate policies, the gains from increased exports no longer appear negligible in the light of the preceding analysis. One can occasionally observe a tendency to dismiss these benefits because the prospective average growth of exports appears to be limited to an order of magnitude of 2-3 per cent per annum, or no more than the population increase. It may be useful to evaluate this growth from the point of view of income and of foreign exchange.

Offhand, one fails to see why exports alone should be expected to take care of population growth, and from that angle a reference to population appears irrelevant. On the contrary, one might well wish total incomes to grow faster than exports so that the relative importance of the export sector would gradually decline. What matters for our purpose, however, are two considerations that emerge from the preceding discussion. In the first place, a given increase of exports tends to bring about a proportionately larger increase of incomes. While it may not be advisable to assign specific numerical magnitudes to the three types of multiplier previously referred to, it stands to reason that they are greater than unity, and probably quite substantially so.

Moreover, these increments of income are of great marginal importance. Where exports provide an important share of total incomes and where in the early stages of development such incomes leave little scope for capital formation, the marginal increase supplied by greater exports might over the years make a decisive difference for the future rate of capital formation and development.

Finally, we might be committing a grave omission in viewing the relevance of greater exports only in aggregative terms. From the angle of foreign exchange supplies, the capacity to import specific goods—or the inability to do so—can be much greater than value figures suggest. The history of exchange shortages is replete with instances of desperate attempts to remove bottlenecks by "robbing Peter to pay Paul."

Leaving aside the consequences of improvidence and injudicious policies, the importation of capital goods for development in particular may directly depend upon growing foreign exchange receipts over and above expenditures for essentials. The specificity of capital goods is such that the cumulative output that depends directly and indirectly on the availability of certain equipment is many times its value. In this respect, it may be permissible to suggest the existence of an "equipment multiplier" which expresses the marginal efficiency of foreign exchange increments in terms of productivity of the capital goods they could buy. Although measurement could hardly be attempted, this concept helps

to illustrate the marginal importance of additions to foreign exchange supplies resulting from exports.

To be sure, the benefits individual countries can derive from their exports depend to a large extent on the composition of their foreign trade, their economic structure at home, and their economic policies. In addition, however, traditional mistrust of export activities as an instrument for economic growth often discourages the very development that foreign trade could serve. In the light of the present discussion, one might hope for a more judicious appraisal of the opportunities that a growing export potential can provide under favorable conditions.

DISCUSSION

C. P. KINDLEBERGER: The two papers before us could not be more different. Baldwin is skeptical both of the meaningfulness of the net barter terms of trade and of the possibility of their measurement. He is even dubious that we know what happened to them in 1875. Aubrey on the other hand is prepared to use estimates of population and GNP in 1975 to arrive at forecasts of the volume, price and value of imports, and the price of exports, taking the forecast of the terms of trade fifteen years further into the future than the previous terminal date of 1960.

While my assigned responsibility is to comment on the paper of Baldwin, whom I shall chide for timidity, I cannot refrain from remarking on Aubrey's courage, or should I say audacity. I understand how he arrives at a range for the volume, price, and value of imports—through consulting experts in particular commodity fields, whose biases, if any, are presumably distributed in random fashion and not systematically affected, as were possibly those of the Paley Commission experts in 1951 by short-term commodity shortages brought on by the Korean war. Even here I have difficulty in suppressing doubts as to the ability of these experts to predict the prices of say coffee and cocoa in 1975, the year when I retire, when market forces have had such difficulty in finding stable levels for these goods in the last six months. But the predicted price level of United States manufactures would seem to have an even more tenuous foundation, and with it, the predicted terms of trade. I must leave to my colleagues the task of commenting on Aubrey's views on what these terms of trade imply for primary-producing countries—views which appear to be based on a favorable opinion of income-creation in the process of economic development where I should have thought that no stimulus to employment as such was desirable—and return to the first paper.

Baldwin is of course correct in adopting a skeptical attitude on the meaning to be attached to the terms of trade and to the validity of the statistical record of experience. But does he not go too far? In the case he mentions of trade taking place between two countries where the opportunity-cost ratios between two commodities were identical before trade, I assume that he has in mind trade based on differences in taste. Here the Mill formula based on identical tastes and different production-possibilities curves will not operate, but relatively little change is required to adjust it: the country experiencing the greater change in relative prices before and after trade may be said to receive the greater gain from trade.

When he discusses the welfare implications of terms-of-trade changes, he stacks the deck, in my view, when he claims that it is possible for one country, in a two-country–two-commodity model, with identical tastes and proportionate increases in productivity in both commodities, to experience an unfavorable shift in the terms of trade but receive a greater gain in utility, measured cardinally, than the other. To reach this result, it is necessary to assume that

one country is rich, the other poor, and that the two goods have very different income elasticities. The greater gain in utility for the poorer country experiencing an unfavorable shift in its terms of trade then means only that the utility of material gain for the rich country is smaller than that for the poor. The rich country gets most of the material benefits of the increases in productivity, but since it is sated, it can experience only a small gain in utility. Taussig's point that the change in terms of trade produced by a shift in demand has ambiguity from the point of welfare is of course valid, though I fail to see its relevance to this case with unchanged identical tastes. The gimmick here is that identical tastes are hardly meaningful for countries of widely disparate real incomes consuming at a great distance from one another on the shared indifference map. This is like the equality of the law described by Anatole France which permits rich and poor alike to sleep under bridges on cold nights.

I agree further with Baldwin that it is necessary to make one's welfare assumptions explicit in a discussion of the terms of trade, and that it is useful to discuss much more than the ratio of export to import prices, including from time to time the volume of exports and imports, changes in productivity which may have supervened in old commodities, the effect of introducing new commodities, etc. But while he tells us that it is possible for the terms of trade to move in ways which are meaningless or at least ambiguous for welfare, has he undermined what the underdeveloped countries would want to claim: the existence of a presumption in favor of the proposition that a country is better off as to its balance of payments, its real income, and its welfare, with more rather than less favorable terms of trade. If other things are equal, this is so. And what Singer has claimed, it may be appropriate to recall, is that things are more generally equal in the case of underdeveloped countries than for others because of inflexibility of resource allocation, a slow pace of technological change, etc.

Baldwin's skepticism on the subject of statistics further seems to me excessive. His separate points—the weakness of unit-values, index-number, and sampling difficulties, the bias of qualitative improvement in commodities and the necessity to take into account services as well as merchandise—can all be accepted. Still further qualifications could be added, such as the desirability of correction for the import content of exports, and the unrepresentativeness of single years such as 1938, in a period of cyclical depression, to which many trend measurements run. One can also be skeptical whether the terms of trade improved or declined over given periods of years, although I would think that there was no doubt but that the British terms of trade improved from 1881 (not 1870) to 1913 since the series of Imlah, Schlote, Silverman, and Debenham show a rise of roughly 20 to 25 per cent after an (almost?) equal and much more precipitous decline from 1873 to 1881. The example of statistical ambiguity I prefer relates to the period from the turn of the century to 1913, when the capital outflow from Britain reached its prewar climax. Here year-to-year changes are considerable and, within limits, fairly erratic. Accordingly, one can pick any sort of trend one wants between 1896, 1900, or 1902, on the one hand, and 1910 or 1913, on the other, to claim that

to me to be that they each throw light, in two different ways, on a major

the terms of trade improved, declined, or moved sideways. Certainly no existing statistical measurement of British terms of trade in this period seems sufficiently clear cut to permit economists to generalize about the short-run effects of capital exports on the terms of trade, as many of us have.

One can easily pick disquieting examples of index-number and sampling difficulties especially when the structure of trade is changing drastically. The Staehle example on Belgium-Luxemburg is not very damaging to the cause of index numbers in my judgment since it relates to a year when structural change was under way and covers a seasonal movement which inevitably will produce a spread between the Laspeyres and the Paasche calculations. Statistical crudity makes one wary of attaching meaning to movements of 10 to 15 per cent. There is still likely to be significance to wide movements. Between 1913 and 1952, the net barter terms of trade of Western Europe, according to some measurements I have made, declined 20 per cent vis-á-vis the United States and improved 50 per cent vis-à-vis underdeveloped areas of the world outside of Europe. If the double factoral terms of trade could be measured, I suspect that the spread would have been even wider.

While therefore I agree with Baldwin that their thesis about the terms of trade has been inadequately demonstrated, I am inclined to believe that Singer and Prebisch have given us a penetrating, intuitive insight into the development of the terms of trade in the course of economic growth. Underdeveloped countries tend to produce commodities in which entry is easy and exit difficult. This limits their power to raise prices in periods of firm long-run demand—certainly after a gestation period needed to expand capacity which may be long in such cases as coffee—and to maintain them in depression. Colin Clark, W. Arthur Lewis, and perhaps Aubrey would argue that limits on natural resources are going to change this position with respect to entry as depletion in the United States proceeds and populations grow. They may be right, although the substitution of capital for land in developed economies, whether on the farm or in factories for synthetics, provides a counterbalance of which inadequate notice has perhaps been taken.

As against the underdeveloped country, the more advanced country, provided it is not senescent, can readjust its resources to price changes, so that in the limiting case with Paasche indexes, its terms of trade are always 100. When import prices rise, resources shift into import-competing lines; when increases in demand threaten a rise in export prices, expansion of export volume heads it off. If the supply of export and import-competing goods is more elastic with respect to price changes in developed than in underdeveloped countries and a fortiori if the supply of export and import-competing goods is inelastic for price increases and elastic for decreases in developed countries, elastic for price increases and inelastic for decreases in underdeveloped countries, there may be something to the Singer-Prebisch thesis. But in this circumstance the appropriate therapy is not price stabilization through buffer-stock and similar devices on the demand side so much as increased capacity to adapt supply.

THEODORE MORGAN: The central significance of these two papers seems

to me to be that they each throw light, in two different ways, on a major policy problem: the disposition of underdeveloped countries to identify economic development with industrialization and diversification and to lay their bets for development primarily on manufacturing industries. The very considerable labors of both Aubrey and Baldwin have a healthy effect. They help set up warning signals against the usual acceptance of the British 1876-1948 terms-of-trade data as significant, valid for the world as a whole, and indicative of future trends.

To the extent an underdeveloped country is persuaded by Aubrey's research that primary products will have firm and probably rising prices, it is encouraged to specialize in their production. To the extent it trusts Baldwin's analysis, it would feel that both the significance and accuracy of terms-of-trade data of the past are questionable.

The basic procedure used in the Aubrey study is to resort to commodity experts for guidance. Imports are the difference between domestic capacity production, allowing for technical advance, and domestic consumption, as estimated. Prices are a "partly intuitive but [to the experts] quite tangible, appraisal of long-run cost trends in their industries." Population is projected, as basis for demand for food, despite lamentable experience in population forecasts.

Certainly these estimates are extremely fragile. The experts are asked to estimate imports as a residual, in the darkness of twenty years ahead, allowing for the unpredictable gifts of technology!

But it is not fair to Mr. Aubrey to emphasize overmuch the extreme precariousness of long-range predicting. The fact is that no method is much good. At the same time, estimates are in perennial demand. The aim is to do the best one can.

By this modest standard, Aubrey's method looks worth trying. One can allow for experts' tending to be conservative in their projections ("better to fail conventionally than to succeed unconventionally") and for their probably being overinfluenced by the experience and trends of their recent past, and still come out with the feeling that predictions of the future that diverge drastically from this consensus-of-experts view at least need strong support.

What about the income effects of increased exports from underdeveloped areas? The discussion implies—or I misread it—that rising money incomes are an advantage. But they are mixed: partly a problem, partly an opportunity. There may result some modest rise in domestic production, which is desirable. There will tend to be rising domestic prices, beyond a moderate level undesirable. There will tend to be a sharp rise in imports of consumer goods. The government's problem is to capture a large share of the windfall increase in incomes and divert it to long-run investment. The early discussion here omits government and so omits the crucial government problem of devising a tax program to minimize the problem and maximize the opportunity.

With respect to Baldwin's paper, the case, I think, is not proved on the major issue of what terms of trade are least objectionable toward indicating the effect of foreign trade on real income. Of his three indexes, he concludes "the commodity terms of trade appear to be among the most objectionable."

But the only criticisms are that an increase in the productivity of resources used for exports can more than offset a deterioration in the terms of trade and that long-run benefits can sometimes more than compensate for short-run injury. These are reasonable qualifications. If they are not taken into account, that seems the fault of the user, not of the index.

The comparative advantage of the commodity terms-of-trade index receives further evidence from the neat illustration that Baldwin gives of how the income terms-of-trade index (total value of exports divided by an index of import prices) can easily go astray.

The third index is the single factoral terms of trade: the commodity terms of trade multiplied by an index of the productivity of the factors of production. This is statistically unusable save for the single factor, labor, which is at best only one of several factors.

Nor is the solution of using a lot of data—"export and import volume indices, productivity changes in the export and internal sectors, and a host of other economic indicators"—generally useful. Nearly all underdeveloped countries, and many developed ones, just do not have a lot of reliable data. And ordinarily the time and funds of the investigator are limited, and he is forced to look to one or several of the most significant kinds of reliable data available.

I am forced to the view that Baldwin's analysis leaves his reject single measure, the commodity (or perhaps commodity plus services) terms of trade, still in first place.

Next, to the data. Some problems of construction of terms of trade simply blur the result. The general weighting problem is an example. But the biases usually found tend consistently to understate the price position of the primary producer. The new products and quality change bias mentioned by Baldwin both tend to understate the relative price position of primary producers, since improvements have taken place predominantly in manufactures. Again, the British data, measuring value of exports and imports at port of entry or exit, have a built-in distortion that further understates the relative price position of primary producers—for as Baldwin illustrates, transport costs have been falling in the past century. More and primary-producing countries have been paying a price for British goods higher than British export prices by a falling margin; and primary producers have been getting paid a rising proportion of the prices of their products valued at British port of entry.

And further still, the Schlote data for Britain, 1801-76, show a very wide improvement in the relative prices of primary products—so wide as to leave no room for doubt as to the trend.

These indications of weakness in the British data quoted from the League of Nations and UN studies have led me to investigate customs and domestic data on the relative prices of primary goods versus manufactures in six other countries for which data appeared reliable enough to use. The countries were the United States, India, Japan, New Zealand, the Union of South Africa, and Brazil. Data from two countries went back to the beginning of the nineteenth century, three to the 1860's or 1870's, and two to 1901 and 1910.

The results show much variety of experience. Two countries show clear rises

in the terms of trade of primary producers (the rise in the United States is remarkable—some 400 per cent since the beginning of the nineteenth century, on the basis of excellent data); four show various changes or ambiguity. With these results can be compared a recent (September 15, 1954) Pan American Union study of the terms of trade of all Latin America. The recent level is found to be about the same as at the beginning of the century.

I conclude the British data are misleading. There has not been any general worsening of the price position of primary producers.

Should we expect such a worsening? Prebisch said yes on the ground that "lack of organization among the workers employed in primary production prevents them from obtaining wage increases (in the boom) comparable to those of the industrial countries and from maintaining the increases to the same extent (in depression)." (*Economic Development of Latin America*, page 13.) Singer said yes since "technical progress in manufacturing industries showed a rise in incomes while technical progress in the production of food and raw materials in underdeveloped countries showed a fall in prices." (AEA *Papers and Proceedings*, May, 1950, page 478.) Kindleberger said yes in his *Dollar Scarcity* (page 122): "The certainty of loss arises for agricultural and primary product countries from the assumption of continuously increasing efficiency of production in these and in manufactured products, and . . . the application in these circumstances of Engel's law." Today Kindleberger is inclined to say yes because of assumed supply elasticities in developed and underdeveloped countries.

Here are four lines of explanation for a fact that is at best questionable. What if the terms of trade of primary products have not been worsening? The general doubt associated with these arguments seems to me to be that they look to matters associated with and derivative from the trend of prices of primary and manufactured commodities rather than to the basic determinants of those prices; that is, the long-run trends of world supply and demand. I suggest that on these basic determinants it is possible to say something, despite scarce data.

ARNOLD C. HARBERGER: Let me announce at the outset my admiration for Mr. Aubrey's bold spirit. Though one may quarrel with the details of his paper and his approach, one cannot help but sense that he is grappling with real and important problems and doing so with great imagination and competence. More cautious minds would shrink from the task which Mr. Aubrey has set himself, but I do not hesitate to assert that our knowledge would advance much more slowly if caution became our byword and the sureness of our conclusions our major goal.

It should be clear, then, that I do not want to carp at small details in Aubrey's analysis. I rather want to point out two major crossroads at which he may have taken the wrong turn. The first concerns his use of a commodity-by-commodity approach in assessing the probable future course of the primary-finished terms of trade. The second concerns his emphasis on the possible multiplier effects of investment in underdeveloped countries.

One cannot be long immersed in the data on long-term growth and develop-

ment before realizing the tremendous role played by technological advance. The great surges of development which our country and others have enjoyed cannot be explained on the basis of the observed increases in the working force and the stock of capital. Half or more than half of our secular growth in output seems to stem from other causes, which I tend to lump rather indiscriminately under the heading of technology. Unfortunately, we know precious little about the forces which govern the rate of technological advance. We rest, in making long-term projections, on the average pace of such advance in the last half century or so, and we treat this rate of advance as a sort of statistical regularity. While regretting its necessity, I do not object to the use of so crude a procedure in taking our sights on the future. However, I do feel that we must keep firmly in mind two facts that can easily be gleaned from existing data: our technological advance has not been at an even pace, but has come rather in fits and starts; even over long periods, there have been wide divergences in the average rates of technological advance enjoyed by different industries. These facts suggest that our crude assumptions are at their best when we deal with rather long periods of time (to average out periods of spurt and sluggishness) and with rather broad commodity groups (to average out individually divergent rates of progress).

One may here ask whether Aubrey is not on safe ground in making independent projections of the relative prices of a series of primary commodities, and then averaging out the results to get an index for the group. Here, I think, we must inquire more closely into the precise method he used. Demand projections, obtained by familiar and relatively widely accepted methods, were juxtaposed to supply projections based in a consensus of expert opinion. Price projections were deduced, again with the aid of expert consensus, from the size of the anticipated gap between supply and demand at recent prices and from the anticipated responsiveness of both supply and demand to price changes. Now I have the utmost respect for the judgments of commodity experts in the daily decisions they have to make. Yet I wonder whether these experts could have been sufficiently indoctrinated in the subtleties of secular growth to give predictions that are reliable for the long run. If we think in terms of a constant price level, we can envision labor costing roughly double its present wage a quarter century hence. In some commodities, technological advance will have operated to make labor three or four times as productive, so those commodities will decline in price. Other goods will have been more or less bypassed by the surge of technology and they will suffer price rises. It seems to me that few experts could be expected to give reliable judgments as to the likelihood and impact of major innovations over the next twenty-five years. Yet do not their price projections imply just such judgments? My feeling here is that these implications were seen only vaguely, if at all, by most of the experts, and that their projections ignore to a great extent the crucial technological elements in economic progress.

All this might be of minor importance if Aubrey's general conclusion were supported by an analysis of the primary-finished terms of trade on a more aggregative level. But it is not at all clear that this is so. I shall confine myself here to presenting a plausible picture of the world a generation from now, in which, contrary to Aubrey's expectation, the terms of trade have

moved against primary products. I make no pretense of its being more than a rough sketch, and I emphasize that I do not rule out the possibility of an equally plausible case being made on the other side. I only hope to preserve the future of the terms of trade as an open question for further speculation and analysis.

Assume for a moment that the population of the world remains stable and that, due to technological advance and related forces, output per head doubles in a generation or so. Let this doubling of productivity occur in both the developed countries and the presently underdeveloped ones and in both primary and finished goods production. It is well established that the income elasticity of demand for primary products is well below unity, and I think we can safely assume it to be somewhere around .5. Under the assumed conditions, market forces a generation hence would call for a net movement of one-fourth of the resources presently in primary production out of the primary and into the finished goods sector. This is a movement of substantial magnitude, and it probably would be impeded by the usual barriers to mobility. Yet if the movement were not accomplished, primary product prices would fall relative to finished goods prices; indeed, such a fall would appear to be the market's way of calling forth the needed movement.

Now let us relax the assumption of stable populations and assume that the population of primary-producing areas increases more rapidly than that in the rest of the world. This, clearly, would only accentuate the downward pressure on primary product prices. To reach Aubrey's conclusion we must assume either a rather strong bias in technological advance against primary products or a rise of limitational factors (ore reserves, land, etc.) to a place of almost unprecedented importance.

My second point has to do with Aubrey's use of a multiplier-type analysis to assess the effects of increased exports and export-related investment in underdeveloped economies. Multiplier analysis is at best with problems of cyclical unemployment. Here, owing to a number of rigidities in the price mechanism, it is usually reasonable to view increments in output as if they had no cost in terms of foregone alternatives. Secular analysis typically—and I think properly—assumes resources to be fully utilized. A rise in the price of exports then enhances the real income of the exporting country just as a rise in a person's wage enhances his real income. An increment of capital adds to the output of a country its marginal productivity. One needs, I think, more justification than Aubrey offers before one abandons this traditional picture. One can go a part of the distance by assuming that disguised unemployment is rife in the less developed countries, with labor in particular having effectively zero marginal product in many areas. But I feel that disguised unemployment of this type is the special rather than the general case. Almost by definition, it cannot exist in a market economy, and to the extent it has survived in a nonmarket sector, resisting the blandishments of a going wage in the market economy, special explanations, varying from place to place, will probably be required. In particular, there is no reason to expect that such disguised unemployment, rooted in custom and tradition, would be dissipated through the workings of a Keynesian multiplier or an export or investment base.

ECONOMIC RESEARCH AND PUBLIC POLICY

By J. A. LIVINGSTON
Philadelphia Bulletin

To prepare this paper, I sent out a questionnaire to seventy-six economists and statisticians and received replies from twenty-five. Among the respondents was Robert R. Nathan, who started off by saying: "You are a lazy bum making others work for you. However, you have done that successfully in the past and I don't blame you for continuing."

Another of my unpaid advisers was J. Frederic Dewhurst, who declared: "You deserve the title of Mr. Questionnaire, not only because of the headache you send out every six months, but because of your circular letter. Are you trying to get everybody else to write this paper for you?"

Both Nathan and Dewhurst referred to my custom of collecting semiannual forecasts from fifty to sixty economists. The result of those questionnaires led me to the conclusion two years ago that economic forecasters are wrong about two times out of three. Since then, I have revised my faith upward. The economists have been remarkably accurate in anticipating the 1953-54 recession and recovery. The record has so improved that I can now say that economists are right about half of the time. Since business can move up, down, or sidewise, this is reasonably good. There are two chances out of three to be wrong. Hence, a fifty-fifty record is better than chance.

This forecasting record is cited because of its relevance to our subject. In a society in which the government assumes responsibility for economic stability and high employment, forecasting becomes a part of the public domain. National policy cannot be made without some assumptions about the future. And the hazards of forecasting will be reduced if the government achieves economic stability. The less erratic is the business cycle, the less eratic will be forecasting. That will set off a nice chicken-and-egg argument: Did accurate forecasting produce economic stability or economic stability accurate forecasting? And it is desirable for the government to adopt measures which lessen dependence on forecasting. Of this, more later.

We are in a new era. It began with the Employment Act of 1946, which established a Council of Economic Advisers to assist and guide the President in formulating his economic program. It established in

the Congress a Joint Committee on the Economic Report, which studies proposals of the President, and then suggests implementary[1] legislation. As the result of the law and the Great Depression of the thirties which foreran the law, the American people expect the government to prevent depressions—to protect them against protracted economic disuse and disability.

Forecasting is unavoidable because administrative wheels grind slowly. It takes three to six months to wind a tax bill through Congress. It takes as many months to put through appropriations. Changes in defense schedules likewise are authorized by the various committees slowly. And, if Congress closes its doors in July or August and the economy needs legislative treatment in October or November, it is just too bad.

Only an emergency would induce a President to call a special session. That is politically possible if business is in a tailspin, if unemployment is developing rapidly, bankruptcies rising, and the stock market falling. The needs of the times would communicate to the President and Congress what had to be done. It is much less likely that the President would call a special session to combat inflation—if higher taxes were needed, say, to check a roaring excursion into overfull employment and a price boom which might ultimately degenerate into a business bust.[2]

Therefore, the President and his advisers and the Joint Committee on the Economic Report must formulate judgments on what is likely to happen long in advance—as much as a year—so as to be prepared for countercyclical action. This country was fortunate in the 1953-54 recession. Some tax reductions became law, in line with President Eisenhower's recommendations and others in spite of his initial resistance. These imparted an uptow to consumer income and business profits which helped to moderate the decline.

I directed the questionnaire to economists accustomed to forecasting. They are familiar with its hazards, frustrations, and necessity. They have been willing over the years to subject their judgment to the harsh caprice of actual event. I wanted to find out from them what, if

[1] The Committee itself does not initiate legislation. But members of the Committee are on various legislative committees of Congress and so are in a position to influence fiscal, monetary, farm, and other legislation.

[2] I realize that in the summer of 1948, President Truman, after his nomination for President, called a special session of the 80th Congress for legislation on selective price, materials, rationing controls, stronger rent controls, and an excess profits tax. Congress went through perfunctory motions of heeding the President's requests. No legislation resulted. Mr. Truman's action was regarded as a political maneuver to put the Republican Congress on the spot. Inflation is never entirely displeasing. It was not displeasing in 1948. Furthermore, there were signs—notably falling farm prices and leveling off in the consumers' price index—that inflation was not as great a worry as the President said it was. In fact, Christmas business sloughed off and a recession developed in 1949.

any, additional information would make forecasting more scientific—would make them feel more confident of their predictions. I wanted a generalized judgment of the nation's statistical and research needs.

To be sure, the Subcommittee on Economic Statistics of the Joint Committee on the Economic Report conducted hearings on this subject in July. Well-known economists from government, business, universities, and research organizations appeared. They mentioned the missing weapons in our statistical armory. They commented on the need for greater detail in some series and for greater speed in others. They discussed the pros and cons of speed versus accuracy.

But at no point did economists and statisticians, as a group, submit an order of priority. What data are they most anxious to have? It was this gap I hoped my questionnaire would fill. It has done that only partially.

The answers indicate the concerns of the moment. When the next slump comes, if it comes, we shall be able to determine whether economists have been concerned about the wrong things. "The future is only the past again, entered through another gate," said Paula, in the closing line of Sir Arthur Wing Pinero's *The Second Mrs. Tanqueray*. In economics, however, the past offers so many gates that it is all too easy to enter the wrong one.

There was unanimous accord among the respondents—implied or specific—that the government has the responsibility to provide regular censuses of population, manufactures, business, and agriculture. These are absolute musts—benchmarks for our understanding of the world we live in. Schedules should be regular, uninterrupted by budgetary ups and downs. This reconfirms the recent findings of the Intensive Review Committee appointed by the Secretary of Commerce to evaluate the work of the Bureau of the Census.

Beyond that, the comments indicated that economists and statisticians expect the government to continue its collection of basic statistics, such as employment and unemployment, consumers' prices, wholesale prices, housing starts, banking data, new orders, sales and inventories, consumer expectations, and so on. There was not a single suggestion for curtailment. But there were several suggestions that in forecasting—in analyzing probabilities in business—statistics as statistics can be overemphasized. "We don't need more data," said one respondent, "we need better judgment."

My first question was: "What are the most important gaps in our statistical information which prevent or hinder the federal government in making effective policy decisions to maintain economic stability? (Your answer might well cover data which are seriously lacking or seriously deficient in quality.)"

Forty different types of statistics were mentioned. One economist expressed a desire for more information on industrial accidents; another for more frequent data on the gross national product in constant dollars; another for information on the costs of doing business; another for additional seasonally adjusted data on federal receipts and expenditures; two for information on the number of unemployed persons not receiving unemployment benefits; another for better information on farm expenditures; another recommended the accumulation of regional information.

Two demands stood out. Ten economists asked for better information on construction. Among the specific points covered were: more information on the supply and demand for new homes; more complete information on vacancies and conversion and demolition of old dwelling units; general improvement of all data.

One respondent said: "Additional data on real estate market developments might be of strategic importance in the next few years." Another economist was less oracular. He said: "The great construction boom of the twenties aggravated the collapse of the thirties. What about the soundness of the present building boom? Judged by the needs of the entire business cycle—not just the boom phase—are we overbuilding temporarily? And, if so, is not the government with its credit policy more responsible than anyone else? These are matters we know too little about."

Seven requested more detailed data on new orders, sales, and inventories. Oliver P. Wheeler, of the Federal Reserve Bank of San Francisco, commented: "The key role which inventories play in most short business cycles and often in major cycles, seems to call for more 'disaggregation' in the study of the inventory cycle."

There were requests for more data on consumer spending, savings, and intentions to spend or save. There were, also, requests for information on businessmen's expectations. Three economists asked for improvement in the BLS indexes of productivity.

The second question was: "What do you think the government's role should be in economic research? Should it engage solely in gathering of statistics on current developments or should it engage in long-term research projects on the effects of government policy on the economy? (This last type of research has usually been done by private organizations, such as the Twentieth Century Fund, National Bureau of Economic Research, etc.)"

Fourteen of the respondents felt that the government should engage in long-term research. Usually, the feeling was positive and enthusiastic. Six objected. The principal objection was that the government would not be impartial, that the research would be politically moti-

vated. Five did not answer the question.

The third question was: "What economic policies followed by the government in the past year do you consider most open to criticism? Would better statistical data or research have pointed out beforehand the shortcomings of these policies?"

The economists seemed to divide into two groups. Those who might be described as of the New Deal-Fair Deal school felt that the tightening of money rates was a mistake. They felt that the government should have taken more positive steps when defense expenditures were tapered downward. The increase in unemployment would not then have occurred and should not have been countenanced. Economists associated with labor unions or with labor ties were emphatic in this latter view. They expressed the feeling that the government has the responsibility of maintaining an ever expanding economy. There is no need for layoffs, letdowns, and unnegotiated shorter work weeks.

On the other hand, economists associated with banks or business firms felt that the present administration had done all right. This group is not nearly so united in its thinking as the New Deal-Fair Deal group. It is opposed to the policy of keeping the economy in perpetual high gear, ever expanding. Constant government ministration to maintain expansion would only lead to inflationary boom and then to a bust. One respondent said: "What this country needs is more frequent and milder recessions." Some in this group questioned the easy-money policy. One said that after tightening credit the administration loosened up too soon. Yet there was even some criticism of the hard-money policy in early 1953, which indicated the lack of agreement.

The fourth question was: "What economic policies would you recommend at this time? (In your answer, please indicate any important assumptions you are making about the economic climate during 1955.) Do you think that additional data or information now available would give you firmer convictions about the policies to be pursued? If so, what information would you need?"

The eighteen answers to this question produced a standoff. Nine respondents felt that the government ought to give the economy an upward push—by spending on roads, public works, defense, or by reducing taxes. Nine others felt that the present administration's policies were fine, that no great federal exertions are required, that an upturn is at hand. Immediate and large injections of government spending are unnecessary.

One respondent said the principal problem was to guard against overspeculation; another said the principal objective should be to stop inflation. Here, the inference is that the easy-money policy is leading to excessive expansion of home building and construction of all kinds.

As investment funds seek outlets, they may go into projects which are not too well planned and which, eventually, will lead to business failures and overcapacity—redundant and competing shopping centers, for instance.

There were no positive expressions of what additional information would have led to firmer convictions about policies to be pursued.

I believe the response to the questionnaire indicates that economists feel the government must assume responsibility for providing statistics and research necessary to decision making, both public and private, in order to "promote maximum employment, production, and purchasing power."

Yes, private research should be encouraged. The federal government ought not to try to take over all statistic gathering and research. When trade associations and industries gather facts, fine. But if it happens that an important segment of industry is "unguarded," then the federal government must gather the information. The cost of gathering information is small relative to the cost of unemployment and the psychological shock of a swift, sudden decline.

The 1937-38 slump is a case in point. Had we known more about sales, new orders, and inventories, perhaps we would have been wise enough to have prevented the economic heartache. In 1936 and in 1937, the Federal Reserve Board raised reserve requirements and tightened credit when industry was running out of new business. At the time, we had virtually no satisfactory data on new orders, sales, or inventories. It is possible that government policy aggravated the 1937-38 readjustment—one of the sharpest and swiftest on record. Immediately after the drop, many agencies—the National Industrial Conference Board, private research organizations, and the Department of Commerce—decided to plug the gap. Information on new orders, sales, and inventories was the statistical rage!

The answers to my questionnaire are significant in this regard. The emphasis on better construction and housing data and the emphasis on new orders, sales, and inventories hark back to the two most recent and important slumps: the Great Depression of the thirties, in which the falling off in new housing was one precursor, and the slump of 1937-38. These emphases are understandable. Men remember what hurt or fooled them most recently. This is the stable-door method of preventing depressions. You close the statistical gap of the last depression.

We cannot neglect the indicators, the barometers, which might have given us foresight in previous declines, but neither dare we assume that the causes of earlier depressions will repeat themselves in the same

ways. In a changing economy, men's excesses, which cause slumps, take new forms. Economists and statisticians—particularly those in government—must be ever alert to changes in the character of the economy and statistics so as not to be beguiled.

We all know how statistics lose their meaning. Once, carloadings was considered a significant indicator of business conditions. But during the twenties freight cars became larger. Carloadings did not reflect the increase in traffic of the period. Just when we became adjusted to this shift—we allowed for the increased tonnage per car—the truck became an important freight mover. Carloadings then declined, not only because cars were more capacious, but also because freight was moving over highways instead of rails.

Consider pig iron. The stock market used to rise and fall with it. Now it is obsolete. Steel has taken over as the nation's work-horse metal. Yet even this is changing. Steel is not so dominant in metal-working as it once was. Aluminum is up and coming. Once we reckoned aluminum in pounds. Now it is a tonnage metal. And it is used in operations which usually require more machine-hours than steel—in airplanes and airplane parts, for example. As aluminum increases in importance, steel may decline in importance as a statistical guidepost.

Business failures are another indicator which may be undergoing a change. This is the era of the large corporation. Business firms have branches in many places. The small family-owned firm represents a decreasing percentage of total assets. And since the small firms are more likely to fail than the large firms, business failures may be less sensitive as an indicator of economic change than formerly.

Let me try to illustrate this. You have probably read that John Wanamaker's has closed its New York store; that Alexander Smith & Sons, carpet manufacturers, is shutting its Yonkers plant, which was built during the Civil War; that Gotham Hosiery has abandoned its knitting mill in Philadelphia; that International Shoe shut a plant in Manchester, N.H. In the twenties, each of these plants would have been a corporation or a company by itself. Each closing would have entered the bankruptcy or business-failure totals. And as a result of such shutdowns, creditors would have uncollectible accounts. That is not so today.

These are business failures of a type. An establishment is outmoded. Trade has passed it by. Equipment is inefficient and obsolete. A bigger plant has been erected by the parent company elsewhere. But no bad debts result. The parent company is still solvent. Yet the local area is directly affected. An employer is moving away. Disemployed workers will have to find jobs in other companies in the area or move away. Personal distress—nonpayment of bills or defaults on mortgages

—will result. The local impacts are pretty much the same. Yet the failure at the local level does not register in the business failure statistics.

We know more than we ever did about what makes our economy tick. We have more statistics, more information, better-trained statisticians and economists, and better mechanical equipment. But we also have a far more complex economy with which to deal. And we know that economists differ on when and how to cope with inflation and deflation. If we knew, really knew, there could be no difference of opinion. We know some facts, but not all the facts. Hence we interpret the facts we know differently. When all the facts about a problem are on the table, the answer dictates itself.

Not only do differences develop about how to achieve maximum production and employment, but differences develop over the methods for detecting changes in the level and "quality" of business. Some economists study the behavior of business indicators, such as business failures, steel operations, new orders, sensitive commodity prices, new incorporations, paper board production, lumber production, housing starts, and so on. By analyzing each series, they hope to detect declines or upturns in business early enough to permit government action to prevent either inflation or deflation. And among this group, there are differences over the relative importance of the so-called "barometers."

Another school of economists relies on models—economic models, not Conover or Powers models. Attempts are made to estimate the gross national product by making projections. These projections are aided by data which possess a "forward look," such as the SEC-Commerce compilations on plant and equipment expenditures by industry, the University of Michigan studies of consumer-spending plans, and so on. Sure, the parts of the economy are interrelated and the forward-look components help to indicate direction and total expansion or contraction. But here again the accuracy depends on the assumptions the model builder makes and on the reliability of the forward-looking data. Sometimes estimates based on surveys turn out like the election polls.

Federal budget estimates, which ought to be dependable, have been off the target—even over comparatively short terms. As one recent example, in January, 1954, the government estimated that tax receipts in the fiscal year starting July 1, 1954, and ending June 30, 1955, would amount to 62.7 billion dollars. But by September, 1954, the estimate was lowered 5 per cent, to 59.3 billion. That change, of course, could be attributed to events not fully anticipated—to changes in business conditions and the tax law.

The government did not do too well in its estimates of its own ex-

penditures, either. In January, 1954, the estimate of spending for the 1955 fiscal year was 65.6 biliions; in September, only nine months later, that was revised downward by 1.6 to 64 billion which is not too bad. But we do not know what the actual results will be by the time June, 1955, rolls around. In this instance, the forecast of reductions in tax receipts and expenditures is partly offsetting. The errors almost canceled one another.

But errors can be self-reinforcing instead of counterbalancing. Then the model goes way off. This is its nature. An assumption in one sector of the economy often influences assumptions in other sectors. As an instance, if you estimate that government outlays on defense will drop, you might also assume that consumer purchases will drop because of resulting unemployment. This partially explains the forecast of eight million unemployed in 1945 and 1946. The economists in the Office of War Mobilization and Reconversion knew government outlays would drop. But they underestimated the speed with which industry would reconvert; hence they overestimated disemployment and underestimated payrolls. As a result, low estimates of consumption, production, and employment were all in the same direction.

Each economist is bound to have his own predilections—his own methods—in trying to forecast business changes and in trying to formulate government policy to abet a high level of employment and a rising standard of living. I do not believe that the government can afford to rely solely on the barometric approach or model building for forecasting. Both are necessary.

Furthermore, in the effort to determine what action the government must take to influence the course of business, a model is essential. Both the analyst and the policy-maker want to know whether construction needs bolstering, or consumer purchasing, or business outlays. The model suggests areas in which government support is needed to achieve a high-level employment.

The model builder is like Professor Higgins[3] in Shaw's *Pygmalion*. Just as the Professor tried to correct and recorrect Eliza Doolittle's speech so that she could pass as a lady, the economist builds a model of what is likely to be. Then he recasts it—according to his conception of perfection. Government policy then is directed to obtaining this ideal. If the policy is correct and successful, the resultant model, like Eliza, is the modeler's dream.

Under the Employment Act, the government must engage in constant business cycle analysis. The Council of Economic Advisers takes

[3] Not to be confused with Benjamin Higgins, author of *What Do Economists Know*, a charming, clarifying set of five lectures.

the lead in this, assisted by the many research and statistical agencies of government, the Department of Commerce, Bureau of the Census, Federal Reserve System, Bureau of Labor Statistics, and so on. This has immediate application to current business conditions and fiscal policy.

But government must not stop there. I think it ought to undertake —directly or through private research organizations—studies into long-term economic trends or contingencies. I have in mind studies such as the Paley report on America's resources. We have to review periodically the long-term prospects for supplies of basic commodities, such as petroleum, iron ore, copper, aluminum, and so on, to determine our competitive position vis-à-vis the rest of the world. We talk about the possibility of becoming a have-not nation. We must also study the dangers of becoming a high-cost nation.

Likewise, it is desirable for the government to continuously examine the economic consequences of postwar technological improvements, of automation. Are we on the threshold of a great expansion in output per man which will make the forty-hour week obsolete? Will that result in large-scale unemployment and an economic crisis? Or will the labor unions exert countervailing power on industry and obtain reductions in worktime at rising hourly rates to offset increases in productivity?

The government might also want to make continuing studies of consequences of the great flow of savings into savings institutions. Raymond Goldsmith has worked on phases of this problem. Will insurance companies, savings banks, and other institutions be so pressed to invest funds that they will become parties to industrial and commercial overexpansion?

We have to keep abreast of the effects of an aging population on production and consumption and the tax burden on the younger population. We also will want to study the impact on the economy of the increase in babies. In the next ten years, will we need more schools, factories making children's clothes, etc.? Not only does gross population have an impact on business, but so do its segments. In our emphasis on the aged, on geriatrics, we must not forget the economic influence of the younger generation.

Government fiscal policy, itself, calls for special long-term research. We have certain theories about the effect of taxation on consumption and investment decisions. But are they justified? Do high corporation taxes, for example, discourage business investment? Or does business merely pass along the taxes to the consumer and, for purposes of reinvestable funds, hold back earnings from stockholders?

Similarly, it would be well to re-examine the effect monetary policy

has on the business cycle. We assume easy money encourages businessmen to borrow and to expand and that high interest rates have the opposite effect. How valid is that?

Another long-term research project would be the relationship of the farm and urban populations. The government has to support farm prices today because of surpluses. But is it possible that the increase in mouths to feed and bodies to clothe in the decade ahead will result in an enlarged demand for foods and fibers, thus increasing the economic bargaining power of the declining number of persons who remain on the farm? Is the farmer moving into a new strategic position—from political to economic strength?

Other subjects which might come under the government's purview are the expansion in state and local debt. In the next five or ten years, the burdens assumed by states, cities, and towns in providing hospital, school, road, police, and fire services could create a taxpayer crisis. Could they also cause a financial crisis?

I also think that the sale of revenue bonds—for the building of roads, schools, and other civic improvements—is a source of potential financial danger. We are creating in the United States state-sponsored enterprises which have the benefit of tax exemption. Yet the service of these bonds—the payment of interest and the building of sinking funds—depends entirely on income. If for any reason a road is poorly planned, the bonds would default. Is this device abused? And could it result in a loss of faith in municipal and state credit?

Long-term research has this advantage over short-run analysis. It lifts minds and imaginations out of day-to-day detail. Secular changes are observed. New economic probabilities can be dreamed of in the freedom of not trying to find solutions to immediate problems. The present is better understood for knowing the past and peering starry-eyed into the future.

At the outset, I alluded to the difficulties of forecasting, yet the necessity for it. If a business decline occurs when Congress is out of session, it would be months before Congress could act—even assuming the President were to call a special session. Therefore, it would be well for us to introduce greater automation and flexibility into our anti-depression armory so as to depend less on prophecy.

We have certain flexibilities now. The graduated income tax is a negative form of flexibility. As personal incomes drop, the burden of taxation falls. Similarly, as corporation earnings decline, the corporate tax burden is reduced. The government shares about half the drop in pretax income. Such automatic reductions in taxes cushion the severity of the business decline. But they do not increase income. They are not contracyclical in effect.

Unemployment compensation also is automatic in response to a recession. Workers are laid off. Their incomes do not collapse. Old age pension payments also come into play. Persons who are entitled to pensions who lose jobs can draw on their pensions after unemployment compensation is used up. Thus we have automatic income-supporting measures.

But what if these are not enough? What if the decline proceeds rapidly?

We ought to give serious consideration to empowering the President—or the President in consultation with a Congressional committee—to take positive action to lift income when Congress is out of session. For example, we talk of the need for a continuing shelf of public works—to be spread around the country when unemployment rises. But building highways, constructing public buildings, dredging rivers, and expanding research all require funds. Ought we to set up a special reserve fund of two to five billion dollars to be used by the President at his discretion or on some automatic signal, say, when unemployment reaches five million. (Herbert Stein, of the Committee for Economic Development, has gone into economic automation in great detail.)

Similarly, should the President have standby power to reduce the tax rate in the lowest income bracket, so as to immediately increase the amount of income available to persons who have jobs and whose incomes have not been reduced? Should he be permitted to cut certain excise taxes? These actions would be contracyclical in effect.

Congress is always reluctant to give the Chief Executive discretionary authority. (Probably it will take a crisis to jar Congress loose from this reluctance.) Congress might be more receptive to this idea if some Congressional strings were attached. Suppose a small, special, standby committee were established to act with the President if an emergency develops while Congress is out of session. This committee might consist of the majority and minority leaders of the House and the Senate, the two ranking members of both parties on the two Appropriations Committees, the House Ways and Means Committee, the Senate Finance Committee, and the two Banking and Currency Committees, and the Chairman of the Joint Committee on the Economic Report.

The methods for dealing with a recession would already have been set up. What would then remain to be determined by the President—or by the President and a Congressional committee—is the timing: when to put the measures in force. And a time limit should be established on tax reductions. The rates should automatically be restored at the end of a year—unless Congress legislates to the contrary in the meantime. (I am not suggesting an automatic anti-inflation program. That would require raising tax rates. I hardly think Congress would

grant the President or a committee power to notch taxes up—even within limits.)

The purpose of an automatic antideflation program is self-explanatory: to anticipate unanticipated changes in business conditions. It would be a stopgap, pending the convening of Congress and the enactment of formal legislation to meet the specific problems. It would take the burden off the Council of Economic Advisers to forecast correctly six months to a year ahead.

I have one further point to make on economic research and public policy. I propose a new agency—a private agency—to be the private counterpart of the President's Council of Economic Advisers. I think it ought to be financed by a foundation, such as the Rockefeller, Ford, or some other organization interested in the social sciences. It would report to the public at large. Its function would be to analyze economic trends just as does the President's Council. It would appraise the Council's report to the President, the President's report to Congress, the policies recommended by the President, and the economic effects of Congressional legislation. This private council of economic advisers would have the responsibility of analyzing public statements of government officials—the President, the Secretary of the Treasury, the Secretary of Commerce, and so on—to determine their validity.

Such an independent research organization is necessary because of the institutional character of government research and government policies. Within government, there is freedom to criticize and argue. But once an executive determination is reached, once a policy has been set, the critic, if he is to remain in government service, is silenced. He must then become a conformist. He must be on the team. In this, government is no different from a corporation.

This institutionalization of attitudes within government has dangers. Take a simple example. A budget is developed. It calls for a certain level of taxes and outlays. It is argued back and forth. Finally, it is adopted. Some government economists may feel that expenditures are greatly overstated—that the Department of Defense may not be able to let contracts as rapidly as the budget implies. That being the case, the income-supporting implications of the budget are exaggerated. Therefore, they feel that the Council of Economic Advisers, the Department of Commerce, and other government agencies should discount the budget.

But here is an authoritative document. It is the President's final word on income and outgo. How can government economists argue with *the word?* Department of Commerce or Department of Labor economists cannot make brash statements about the budget being unrealizable. They have to accept it. Most persons in government do ac-

cept it. After all, a good many worked on the estimate. So it becomes imbedded in economic analysis. And since government outlays constitute about 15 per cent of the gross national product, the budget becomes heavily weighted with a fixed concept.

That is not the worst. Government tends to develop and parade a Pollyanna attitude. No President, Secretary of the Treasury, or Secretary of Commerce can make a public statement that business is not going to be good. Official statements—from those of President Hoover to Presidents Truman and Eisenhower—have ever been in an optimistic vein. The government has the responsibility of seeing that business is good. If it is not good, then it must try to help make it good. To proclaim poor business might create poor business. (The forecast of eight million unemployed in 1945-46 is the exception. Here the purpose was to impress on Congress the need for legislation. Also, it was a special situation—reconversion from the war.) Certainly, therefore, the public expressions of persons in government tend to be optimistic. And these opinions are the ones that are communicated to most persons. Doubts are communicated in private discussions, perhaps, but propriety calls for the "good word."

This is a political necessity. Every two years the government stands before the voters—in the Congressional elections. The party in power does not lose by glossing over unpleasantries. It keeps a stiff upper lip, regardless. That is the nature of the government: openly optimistic, regardless of what sober discussions go on in the inner councils. And that can have a self-mesmeric effect. Persons often think the way they talk!

Therefore we need a countervailing research power against institutional optimism: a nonpolitical organization whose sole objective would be to make a review of government economic statements and policies, not for the purpose of being critical and opposed, but for the purpose of being analytically objective. It could agree as well as disagree with government statements and legislation. Nor would it complain if government failed to follow its suggestions. It would look to the future, not beat dead horses.

No organization in existence today satisfactorily fills the bill. The Joint Committee on the Economic Report was set up by Congress for this purpose, but, unfortunately, the two-party system is not conducive to impartial research and reports. On critical issues, this committee usually splits.

Grover Ensley, staff director of the Joint Committee, did a remarkable job in developing a "joint report" on the President's 1954 *Economic Report* to Congress. But even then there were several exceptions and special points of emphasis by the Democratic members. And I doubt that even Ensley's exceptional diplomatic skill would be able

to get a truly joint report during a period of acute difference of opinion on ways and means of coping with either inflation or deflation. These reports tend to be compromises rather than objective analysis. If there were ever the prospect that the President's report were to be a major campaign issue in an election year, the chances of committee agreement would, in my opinion, be nil. Regardless of the objectivity of the committee's staff, the committee itself cannot be entirely objective. So it cannot perform this function.

Research organizations which sell their services often develop material which differs from government projections and policy. But such findings are limited to clients. The National Industrial Conference Board does some work in business analysis, but it has other functions and it depends upon subscriptions from business organizations. The National Bureau of Economic Research has the personnel for a task of this type, but it is devoted to long-term research rather than analysis applicable to immediate economic developments. It would not say "boo" on current events.

Labor research staffs frequently criticize the administration's program, but these criticisms are as much political as economic. Banks publish monthly reports on business, but are seldom single-mindedly devoted to business cycle analysis—with the purpose of informing the public about the strong and weak points in the business prospect, the efficacy of government policy, and the need for changes, if any. There is no lack of critics and analysts of government policies. But there is no organization set up for the specific purpose of making studies of cyclical progress and problems.

This organization would have to be headed by an outstanding economist—a person who could command the respect of economists of the New Deal-Fair Deal school and of the so-called "more conservative" school. (Professor Higgins classifies economists into four schools: the Latter Day-Laissez Faire, Keynesians, Marxists, and Institutionalists.) I am putting my stress here on political leanings rather than method or approach to economic analysis.

The organization would need a competent staff comprising specialists in government finance, banking, industry (broken down into hard goods and soft goods, with subdivisions for important sectors, such as automobile and steel), consumer savings and spending, building and construction, state and local government plans and outlays, etc.

A staff such as that of the Federal Reserve System would be ideal. The Federal Reserve, however, has to be looked upon as a branch of government. It cannot issue independent statements. Reserve research must be directed toward assisting the Board in making policy decisions which are co-ordinated with the policies of the Treasury. Thus complete research independence is neither possible nor, for that matter,

permissible. What is needed is a critic-commentator of government economic policy—a research group which would commend when policies seem wise, which would point out dangers if policies and statements seem unwise or impetuous.

Such a group is required because the government today commands more economists and statisticians than any other organization. Size equals power. The fact that recent results of the Council of Economic Advisers have been excellent does not guarantee the future.

Such an independent organization, with a strong board of directors, would command the attention of the President, his advisers, and the Congress. Its observations would alert persons in and out of government to shortcomings in government concepts, projections, and policies. It would remind all of us that the government's word is not absolute. It would guard against a stampede of monogenous opinion, fostered by government, which could lead to boom and then, perhaps, bust. Furthermore, it would furnish a yardstick against which to measure government forecasts. This organization, even as the Council, would put its judgment on the line in its reports and observations. Its survival would depend on its record—on the help it gives to all of us in understanding the economic currents and crosscurrents.

To summarize:

1. The government must undertake to improve existing statistics and develop any statistics necessary to increase understanding of the economic world we live and will live in. That is a virtual mandate under the Employment Act.
2. Statistics will not supplant judgment, however.
3. Long-term research is as much a government responsibility as short-term analyses of the business cycle.
4. To make up for the shortcomings of forecasting, we should invest the President with limited powers to undertake contracyclical action when Congress is not in session.
5. A counterpart of the President's Council of Economic Advisers should be established outside government to provide objective analysis of government plans and projections.

I think we are all humbly aware that the past is dead, the future is unborn, and the present is imperfectly understood. Sometimes I am sure we shall conquer the business cycle, that we shall limit declines to short periods of time and modest percentages. But I have doubts, too. At such times, I fear that not until after the next crisis will we know what kind of research we should have had to prevent it. But that does not mean we do not need research. We most decidedly do—better research and better judgment.

DISCUSSION

EWAN CLAGUE: At the outset let me emphasize that this is a good paper. Mr. Livingston asks the right questions, discusses the right issues, recognizes the handicaps and the necessities of forecasting, and makes some important suggestions.

My first comment is to indorse wholeheartedly Mr. Livingston's call for better statistics. Among those needs which were ranked highest by the economists was better construction statistics. This priority is correct in my opinion. Both from the seasonal and cyclical points of view, construction is probably our most important industry. At year-end in 1954 the economic situation would be quite different if the construction industry, instead of reaching new heights of activity, had turned downward. The recession of 1949 was counterbalanced in the same way.

Yet the statistics on construction are among the least satisfactory in our whole kit. The housing starts series of the Bureau of Labor Statistics has recently been revised and is now in excellent shape. However, it is a national series only. There are no corresponding data for localities, metropolitan areas, states, or regions.

There is no longer any data—national, state, or local—on the major characteristics of building, data which we formerly obtained. These would include the prices, the sizes, the materials used, etc. Correspondingly, we need information on the major characteristics of the buyers and renters, such as their previous residence, income, family size, etc.

Then, too, there was formerly a survey of builders which yielded information which should be of great value in forecasting the outlook for construction. This included the land holdings of builders, their building plans, recent sales experience, and their inventory of materials.

The data on dollar volume of construction put in place are fairly good, although they have some very important weak spots. Information on the construction of private nonresidential building, such as offices, factories, hotels, etc., needs revision and improvement; likewise the data on state and local public construction, which we are counting on to buoy up the economy during the next few years. We need and ought to have a good nationwide monthly series on the value of construction work started as well as on construction put in place.

Finally, there is urgent need of a good series on construction costs. There are some private series, but these are both limited and incomplete, and are based largely on the wages of building labor and the price of building materials. Over a long period of time these two will overstate building costs because there is no allowance for increases in productivity. What is needed in this field is nationwide coverage of various types of building structures and with some allowance for productivity increases.

Next, I should like to comment briefly on the problem of the establishment

of good government statistics. Government statisticians are competent practitioners and their integrity has been demonstrated time after time. There is no occasion for any criticism of government statistics on either of these bases. With respect to some other points, however, the situation is not so good. In general, there is not adequate maintenance and repair of statistical series. In order to avoid the obsolescence of which Mr. Livingston speaks, it is necessary to keep constantly checking the quality and significance of the data collected in any field. Furthermore, when the quality deteriorates there should be funds for repair and revision. This requires adequate funds specifically provided for this purpose.

Another weakness of government statistics is inadequate analysis and interpretation. This is not an exclusive function of government since there are many outside users, but in the first instance a minimum amount of analysis and interpretation is absolutely essential. Furthermore, this should be done by the collecting agency, since they are the only ones who are constantly in touch with the original data. Raw data are often misleading, as, for example, in unemployment compensation where the weekly claims figures often vary, not because of changes in unemployment, but because of administrative factors. The originating agency is the only one which can properly interpret these changes for the general public.

Finally, for a number of years public attention has been called to the gaps in government statistics. The Congressional Joint Committee on the Economic Report has called attention to this problem. Two things are needed: the elimination of series which are obsolete and the development of others which are urgently needed.

In conclusion on this point I should like to emphasize that in government statistics the public gets what it is willing to pay for in accuracy, frequency, and promptness of publication. Of course, there can be economy in statistical work—the development of sampling in recent years is clear evidence of that. However, there are limits beyond which it is not possible to go. It is the responsibility of the statisticians to make clear to the public the limitations on the data being published and the requirements for improvement.

I want to comment briefly on differences of opinion among economists. Mr. Livingston says that economists differ widely, but if we only had all the facts the answer would dictate itself. This is not quite true. There is one factor missing; namely, the value judgment of economists. For example, in an economic downturn some economists might favor a little inflation (or at least reflation) while other economists might recommend deflation (or at least letting things alone). Suppose that the eventual outcome would be exactly the same and a business recovery would take place. However, some people, including economists, will prefer the first solution while others will prefer the second.

These differences have nothing to do with economic analysis but only with personal preferences. Economists ought to watch this more carefully, since the public interprets all these differences as indicating the complete lack of any basic economic principles. My point in connection with Mr. Livingston's paper is that there is little use in gathering more facts and

"sweating out" more analyses if the differences of opinion are due solely to personal preferences and value judgments.

I want to say a few words about Mr. Livingston's remarks on forecasting by government officials. I do not take as dim a view as he does.

In the first place, all forecasting, if widely believed, tends to affect the results forecast, either directly or adversely. For example, if a high government official should forecast a sharp business depression (and publish it), he might possibly induce a recession through popular response to his forecast. (Perhaps this greatly exaggerates the influence which people generally would give to official judgments.)

Another example occurred this year. The outlook for home building took a little downturn in early 1954; so the government passed legislation facilitating the consumer purchase of homes. As a result, the downturn in the first half of 1954 was converted into a sharp upturn in the second half. So the forecasts of early 1954 had to be revised in the light of action to prevent their taking place.

Finally, I think that the reassurance by the President of the United States that the Administration would act forcefully to prevent a real business depression undoubtedly influenced the economic situation. Since the government has powers and resources capable of producing significant economic results, the business world can and does act on such reassurances.

I am not referring to Pollyanna statements which government officials may make concerning what business will do. I am referring to those statements which indicate what the government itself will do, on which government officials presumably have good judgment.

Finally, I should like to say just a few words about time and timing in economic policy. We economists have a most important obligation to make clear to the public the significance of time in economics. I stress this because the layman tends to expect results immediately. Even the economic actions which produce the quickest results take some little time to bring about those results. Some far-reaching policies may take months, or even some years, before their influence is fully effective. So we have not only the difficulty envisaged by Mr. Livingston; namely, that government policy-makers have to gauge the time allowances of their policies and therefore must strive to be forehanded in their actions. We have also the additional complication that the public does not always understand the time requirements and, therefore, will express impatience or dissent from policies which will work out all right, given a little more time. If I have any one suggestion to make, both to government economists and to those in business and other fields, it is that they try to keep the public informed on this crucial point.

GROVER W. ENSLEY: I like the realism and insight of much of Livingston's presentation. I would underscore his statement that "in a society in which the government assumes responsibility for economic stability and high employment, forecasting becomes a part of the public domain." It is well to remember in our discussion, of course, that the federal government does not guarantee "stability and high employment," but rather to use its resources

and powers in co-operation with "industry, agriculture, labor and state and local governments" to that end. Under the circumstances, I would certainly agree that "national policy cannot be made without some assumptions about the future." Although we could quibble over the meaning of words like forecasting, projections, and assumptions, whether we forecast or not is academic today. The only question is, how can we improve our forecasts? This idea, of course, was clearly recognized in the Employment Act of 1946 and in that Act's prescription for the President's annual *Economic Report.*

The Joint Economic Committee has prepared and published a nation's economic budget annually for several years, not as a forecast or prediction, but rather as a projection for the next year of levels of economic activity needed to achieve the objectives of the Employment Act and to show the nature and magnitude of adjustments from present trends and plans and expectations needed in order to achieve the objectives. (H. Rept. 1256, 83rd Congress, 2nd session.)

No matter how good the process of forecasting or projecting may become, however, economists should not expect prompt compensatory legislation to result solely from a prediction by the Council of Economic Advisers, the Joint Economic Committee, or a private economic supreme court, that a turning point in economic activity will come at some given date during the coming year. Under many such circumstances we can only prepare in advance to legislate speedily at a time the turning point becomes evident, relying upon monetary policy, the built-in stabilizers, and administrative flexibility to cushion and to provide corrective forces against the initial effects of an unfortunate turn-around.

I would go along with Livingston in granting administrative discretion for increasing and decreasing public works expenditures within certain limits. I would suggest, however, that the Congress is often more sensitive to the needs for making tax adjustments than the Executive. And I am not as pessimistic about special sessions of Congress if need for economic legislation arises.

Livingston correctly says that the government must not stop with short-run analyses. It ought to undertake "studies into long-term economic trends or contingencies." Our Committee has given emphasis to this type of research. Several staff reports have focused on the long-run outlook. The most recent of these reports—released in November—was titled: "Potential Economic Growth of the United States During the Next Decade."

Naturally, I liked Livingston's reference to the recent hearings and report of the Subcommittee on Economic Statistics of the Joint Committee. All of the witnesses at these hearings placed the then pending census appropriation bill at the top of the "must" list. The subsequent appropriation to carry out the census of business and manufacturing resulted, in large part I believe, as a result of this testimony.

The Subcommittee report (H. Rept. 2628, 83rd Congress, 2nd session) also placed a high priority on four additional areas; namely, savings, inventories, consumer expectations, and business intentions. At the Subcommittee's request, the Board of Governors of the Federal Reserve System has created

five task groups of outstanding technicians to make exhaustive studies of present data, concepts, and sources and to propose practical programs for improving information in these areas. Information on the nature and scope of these studies was released by the Joint Economic Committee last week.

The President's program to be unfolded to the Congress in January, 1955, will propose significant steps forward in our statistical programs, based, in part, on the hearings and report of the Subcommittee on Economic Statistics. The profession and other private users will all need to get behind this program and see that it is legislated.

I have only one real disagreement with Livingston's paper. It deals with the recommendation that there be created a "private counterpart" to the government's Employment Act agencies. He puts it in terms of "a countervailing research power against institutional optimism." At the outset, let me make clear that we are for private economic research to supplement and assist the Council of Economic Advisers and the Joint Economic Committee in carrying out their statutory responsibilities. The Employment Act recognizes the important role of private research. We know of the good work of the National Planning Association, the Committee for Economic Development, the Twentieth Century Fund, and the many, many other private agencies.

But I would be very skeptical of recognizing one of these agencies as being the institution to which we must turn as sort of a supreme court of economics, even though their views were not binding. I am a little uncertain as to who is supposed to appoint or designate this private economic council of supermen with their priestly powers to issue oracles. I raise this point because it seems to me that the entire proposal for such a private institution as official critic of government economic analysis and policy is in conflict with American principles of responsible government. We have always conceived of agencies performing functions upon which governmental policy is to be founded as necessarily being responsible to the people. Suppose for the moment that we had had such a private council a year ago. Suppose, also, that it would have been headed by as disinterested and objective an economist as Professor Colin Clark. Now if Clark's dire and ill-founded predictions and corrective program of a year ago had been followed, the resulting embarrassment would have plagued the party in power for years; yet our friend Clark would have escaped almost unscathed back to the cloisters of Oxford University and temporary oblivion. An irresponsible economist can live in oblivion, but a responsible political party cannot.

Livingston is concerned about the objectivity of government economic research. He correctly does not question the professional competence and responsibility exhibited by the rank and file of government economists. On this we certainly agree. I know of no more able, sincere, nonpartisan, objective —and I might add underpaid—group of technicians than in the federal career service. I am sure the economic profession regrets the wringer through which the career staff of the old Council of Economic Advisers went in the spring of 1953. Thanks to the professional standing of the three members of the present Council, restoration of a professional staff was possible. A bad precedent, however, has been established in that executive agency.

Perhaps the party line must be followed in the Executive Branch, once a decision or recommendation has been formulated, but the Executive is just one branch of the federal government. There is the Congress where the final policy-making power rests, and no one can charge that the Congress always follows a party line. Furthermore, minority reports are useful in explaining opposing views to the public.

I do not believe the facts bear out Livingston's statement that all government operates in an optimistic vein. That has not been true of all Council reports, nor has it been true of the Joint Economic Committee reports and staff materials. Livingston himself refers to the pessimism of the Administration in 1945 and early 1946. The Administration was seriously concerned about inflation in 1948. I believe the Joint Committee's hearings and report of February, 1954, dampened somewhat the optimism expressed in the President's *Economic Report*, and put into better perspective the need for tax revision—including one billion dollars of excise tax reductions in the face of Administration opposition, more liberal highway and rural electrification authorizations, farm supports, liberal housing, and social security programs.

It is important, of course, for government to provide courageous leadership. In so doing it may give a confident air with respect to the economic outlook, but this can certainly boomerang with resulting loss of public confidence if based on unfounded or false optimism—and successful public officials realize that fact.

For the very reason that elections come every two years, our elected representatives are alert to reality. As a matter of fact, I doubt if any group is more alert to reality than elected public officials. Livingston said: "The party in power does not lose by glossing over unpleasantries." But it does not win by glossing over unpleasantries, either, and the unemployed congressmen walking the streets every other November prove it!

The Joint Economic Committee is criticized by Livingston for "usually" splitting along party lines. But this was not true of the rather historic report of the Douglas subcommittee in the spring of 1950, when it recommended that a flexible monetary policy replace the rigid one. It was not true of the Joint Committee in July, 1950, when it unanimously recommended an immediate and substantial increase in taxes to offset the inevitable inflationary pressures brought on by the Korean war. It has not been true on countless other issues dealt with in Committee reports over the years.

It is to be expected that the underlying and differing economic philosophies of the two major parties should come out in Committee reports. But if there is an emergency with clear-cut evidence of marked inflation or marked deflation—and we have seen such emergencies—I am confident the Joint Committee and the Congress would reach a remarkably wide area of agreement on major economic policy in a sincere endeavor to deal with the problem. I am thinking about July, 1950. Also, had the recession not stopped last summer, we would have seen united action on antirecession measures. The most troublesome problem is formulating policy in a period of uncertainty with a mixture of trends and forces at work—when honest differences of opinion may be expected. Unfortunately, we have not developed anything comparable to the

"rabbit" test which will enable us accurately to predict economic turning points.

I dare say there is more difference of opinion on the economic outlook and economic policy among so-called "objective" economists than among congressmen. Until recently I would have hedged some in making such a sweeping statement because of the tariff issue. But I am not so sure now, having read articles and briefs written by a Harvard professor on the textile industry, a Dartmouth dean on the chemical business, and other economists on other industries and regional problems. Livingston gives economists a batting average of .500 on short-range forecasting. The fact of the matter is that honest men, objective men, differ as to what the economic outlook is and what appropriate policy should be. This is very desirable in an area in which there will always be uncertainty as to the facts and their implications. Disagreement and debate serve to bring out all the possibilities.

We have private research and economic interest groups and universities bubbling over with professional economists analyzing the economic outlook, criticizing, and recommending policy. We have the fourth estate, so ably represented at this session today. We have a form of government which encourages and guarantees this process. We are not short on outside and independent analysis and criticism and recommendations, and that is good. Our Committee doors are always open to economists who wish to give us the benefit of their thinking. The Joint Economic Committee's hearings provide an opportunity for all segments of the economy to present their views to the Congress. But it is the essence of our system of government that our democratically elected representatives synthesize these analyses and arrive at workable programs. That is their responsibility, and we can hold them accountable.

STANLEY LEBERGOTT: Josh Billings once remarked: "Everyboddy wants tew talk, few want tew think, and noboddy wants tew listen." We must, however, surely make an exception for this fascinating and thoughtful discussion by Mr. Livingston. I am going to address my comments to the questions which he asked in his survey and to the answers he listed.

What data are needed to develop policies for economic stability? The answers to his questions on this subject provide an interesting paradox. In Mr. Livingston's apt phrase, the respondents found that the federal government needed "forty different types of statistics." But when they came to their own needs, they found no gaps whatever: "There were no positive expressions of what additional information would have led to firmer convictions about policies to be pursued." Surely they must have overstated the government's needs or understated their own.

Let us, nevertheless, concede that we do have significant gaps in our statistical armory. I believe that the forty suggestions reveal an unfortunate hope—a hope that statistics can be so perfectly tailor-made that economic analysis would be pretty much unnecessary. Let us examine some of the needs that were mentioned. One is the lack of quarterly deflated GNP data. This is a gap indeed. But is it serious? Anyone who examines the basic data will, I

think, conclude that a cost-of-living index will do remarkably well for quarterly deflation of the GNP data between the available annual estimates. Another gap listed was "the number of unemployed persons not receiving employment benefits." But we can approximate trends in this group tolerably well by deducting the number who receive benefits (BES) from the number employed (Census). More data on industrial accidents were also demanded. Considering that Mr. Livingston's question concerned those data needs "which prevent or hinder the federal government in making effective policy decisions to maintain economic stability," one can only conclude that this suggestion was made in jest.

More detailed data on new orders, sales, and inventories? Indeed such data would be an advance. Yet considering the quality of our existing figures, it is somewhat surprising that the need expressed is for more rather than for better data. That needs for data do exist there can be little question. This survey points out some. But we would do well not to take what may be offhand opinions of twenty-six respondents as a very positive basis for judgment. We shall, in addition, want to review such considered statements as those of the Watkins Committee (to which Mr. Livingston made an invaluable contribution) and the admirably succinct summary of the data needed for making policy decisions on economic stability in the statement by the chairman of the Council of Economic Advisers to the Talle Committee.

Should government study the impact of its economic policies and study long-term trends? Asking such a question comes close to asking whether parents should get up early on Christmas morning to look at the children's presents: there is little alternative. The Treasury must study the state of the bond market before launching a new issue. The Reserve Board must evaluate general economic conditions at length to anticipate the impact of a change in reserve or margin requirements. The Secretary of Agriculture has to review the impact of a proposed set of parity prices for the forthcoming year, etc. However, it is clear that many of these studies will not be publicly available, as is, I think, implied in the question.

Moreover, while long-run analyses are made, they are undoubtedly not made as systematically and as coherently related to each other as is implied in Mr. Livingston's question. The Census Bureau, for example, has made long-term projections of both population and labor force. The Office of Business Economics has projected markets for a broad range of industries. Transportation activity has been anticipated by the ICC and by Public Roads. Forecasts of exports and imports have been made for use by international agencies, etc. In sum, a considerable amount of such work has been done. If I may interpret Mr. Livingston's question to that effect, a considerable amount should continue to be done, but perhaps more systematically, with greater integration and with more presentation of the results for broad public use wherever possible.

To Mr. Livingston's proposal for "a private agency, to be the private counterpart of the President's Council of Economic Advisers" I would say: "Come on in. The water is freezing, the bottom is rocky, and you'd better watch out for the old tomato cans." The more economists who address them-

selves to these problems the better. I am certain, for example, that reviews and judgments on the state of the economy made by Messrs. Burns, Jacoby, and Stewart would be of great value for government policy making even if they were outside government service, heading such a private agency.

I would add two limited comments. First, that if there is "a government economic line," as Mr. Livingston suggests, there is nothing like the uniformity of public utterance nor the restriction of data that this would imply. Read the *Survey of Current Business*. Read the *Federal Reserve Bulletin*. Read the reports of the Agricultural Outlook Conference. You will find them, perhaps, stuffy, reserved, in broad agreement. But they do differ. More importantly, they give the facts. They provide the basis for independent judgment by economists throughout the country. Moreover, there is considerable point in not seeking *a* single authoritative critic. In a pluralistic economy such as ours, is not there point to combining and balancing off the statements of both political parties (in the invaluable reports of the Joint Committee on the Economic Report), of the National Planning Association, the CED, the AF of L and CIO, the Chamber of Commerce and NAM, etc.? Each can give us an important perspective on the problem. Each has a significant contribution toward a combined judgment. (I would venture an incidental comment—which, however, does not detract from the merits of this useful proposal. Just as desirable as it would be to have another competent group of economists working in this area, so one could anticipate a gradual attrition of prestige if they were to engage in it: the prestige of economists is usually inversely proportional to the number of detailed predictions they make.)

On Mr. Livingston's final point—the desirability of an antideflation policy—I will not comment directly. The problems of an automatic policy have been well canvassed and this enticing subject is outside my bailiwick.

Although, as Mr. Livingston points out, such a policy would reduce the need for CEA projections, we will, of course, continue to need projections and forecasts. The Treasury, for example, will still have to forecast something like GNP, personal income, and corporate profits totals to make its forecast of government revenues.

In evaluating the need for such a policy, we would do well to consider what I would call the visible hand theory. I would not challenge Mr. Livingston's assumption that even with an automatic policy Congress would not grant powers which the President could exercise before five million persons were unemployed. Yet major steps were taken in 1954, without such a policy, when unemployment was under four million. I refer to the action of the Congress on excise and personal taxes early in 1954 and the program of the President adopted by the Congress in the Housing Act of 1954. In practice, Congress may be willing to support more substantial intervention in economic affairs than it would accept as set down in general premonitory legislation.

BERT SEIDMAN: The American Economic Association is to be commended for including the problem we are discussing in this year's program. It is a subject which should encourage us as economists to take our minds off detailed

problems in specialized fields and to step back for a moment and appraise our work as objectively as we can.

Mr. Livingston has set the stage for such a self-analysis by economists. His paper stimulates us to consider and appraise the role we economists can and do play in the formulation, principally by noneconomists, of public policy.

At the outset, I want to heartily endorse Mr. Livingston's emphasis on the urgent need to fill the gaps in statistical data. A number of investigations during the past year have concentrated on this problem. There is an increasing awareness, principally, but—I am happy to say—not exclusively, on the part of economists and statisticians, of the need for a wider range of and more accurate statistical information.

I particularly like the stress placed by Mr. Livingston on the disturbing fact that we do not now know which sector of the economy will be strategically most critical in a future period of economic stress and therefore we cannot be sure as to what types of economic data will be most needed. If anything, this should reinforce us in the determination to fill as many statistical gaps as possible.

If I had to put all my eggs in one basket, I would especially emphasize the need for more and better productivity data. It has become trite to forecast the tremendous technological improvements and concomitant rapid increases in productivity we will witness in the years ahead; but this is a case where repetition of a prediction does not detract from the importance of its implications. Since measurement of productivity changes is essential for determining labor and capital requirements as well as the production potential of the economy, it is most important that we concentrate now on improving and expanding productivity data.

If I had time I would like to elaborate on the types of statistics we most urgently need, but this is not our principal concern. Perhaps it is enough for me to agree wholeheartedly with Mr. Livingston that there are serious gaps in statistical and economic data which need to be filled in as rapidly as possible.

Now, what about this business of forecasting? Many economists feel that economic prognostication is greatly overemphasized, and that this is especially unfortunate in view of the tendency for the general public to regard the projections of economists as precise predictions. But regardless of the desires of professional economists, economic forecasting is here to stay. Moreover, we should not deceive ourselves into believing that economic forecasts are being made only by trained economists—whether in unions, banks, corporations, universities, or government. Actually, economic forecasts are also being made—often with no conscious realization of it—by literally millions of people who are not technical economists and know little or nothing about economic data.

Let me give an example from the field of collective bargaining. We union researchers are trying to provide as much data as possible for our trade-union constituents for collective bargaining purposes. We are gratified by the increasing use to which our economic data are put. But we are also aware that many unions, as well as many employers, particularly on the local level and in small plants, rely not on formal economic data but on their own feelings, intuitions,

or what have you, as to economic prospects when they sit down at the bargaining table.

Actually, many economic decisions of great importance—certainly of great importance in the aggregate—are made on the basis of just such lay judgments of the future of the economy by congressmen, consumers, farmers, businessmen, union representatives, and many others. Their decisions greatly affect the course of economic trends. Indeed, they may determine whether the projections made by professional economists are borne out by actual events. What should particularly give us pause is that, as Mr. Livingston has pointed out, since economists have had at best only a fifty-fifty record in this business of forecasting, our record may be no better than that of the millions of nonprofessional forecasters.

This may seem to be a defeatist and pessimistic note, but that is not my intention. Rather it seems to me that these facts present a challenge to economists. Since I have no great faith that the judgment or intuition even of economists can be appreciably improved, I would stress again the need for sharpening the tools of economic analysis available to economists. This does not mean that we should seek data for their own sake, but we should make every effort to obtain every bit of data we can get to aid our admittedly imperfect judgments. This means most urgently a halt to the attrition of the government statistical programs which, after all, provide us with the main body of statistical information we must have for effective research and analysis.

I react with mixed feelings to Mr. Livingston's suggestion that the government has a responsibility to engage in long-term economic research. Some long-term research by government agencies is certainly desirable both as a means of making more useful and meaningful the short-term data collection, research, and analyses of these agencies and also to contribute to a more general understanding of fundamental economic problems.

On the other hand, it seems to me that precisely because the government agencies cannot, by the very nature of their status, achieve complete, or even nearly complete, objectivity, I question whether we should place our main reliance on government for studies of long-term economic problems and developments. I would hope that our major source of light on such questions would continue to come from the universities, the National Bureau of Economic Research, and other independent groups.

Mr. Livingston's fourth point—his recommendation that the President be given limited powers to undertake emergency antideflation action—was not included in the preliminary draft of his paper; but if it is an afterthought, it is an extremely worth while and constructive one. While I am not prepared to comment in detail on his suggestion, I do think its implementation would strengthen our economic armory. The President's hands are not completely tied, and there is some grounds for feeling that his existing authority has not been used fully or quickly enough. Nevertheless, his authority to combat incipient depression needs to be strengthened. I do think that the five million unemployment figure Mr. Livingston suggests is too high and that antidepression action ought to be taken before economic activity declines to that level. This is perhaps a detail—although an important one. But I heartily

agree with the general principle embodied in Mr. Livingston's recommendation.

This brings me to Mr. Livingston's intriguing suggestion for the creation of a private counterpart to the Council of Economic Advisers. My first impulse was to endorse this suggestion enthusiastically.

What Mr. Livingston has said about the lack of impartiality of the various private and governmental agencies engaged in economic research and analysis is certainly true. The biases of such groups are for the most part not conscious and are certainly not deceitful, but they do exist and they cloud the impartiality of their work.

I would certainly agree with Mr. Livingston that the President's Council of Economic Advisers labors under such a handicap. Dr. Burns might have thought when he became chairman of the Council that he could somehow maintain an Olympian impartiality and no doubt he has sincerely attempted to do so. But by now he has found that the Council—like any other agency of the government—is of necessity merely an arm of the Executive. This is no less true of Dr. Burns's Council than it was of Mr. Keyserling's, however more willingly Mr. Keyserling may have accepted that role. Moreover, we have seen that when Dr. Burns was unwilling to subordinate himself to the over-all policy of the Executive, as in the case of the so-called "Midyear Economic Report" issued by the White House last summer, he was simply brushed aside.

Is the answer to the admitted biases of the many existing agencies carrying on economic research and analysis and making economic projections a private council of economic advisers? As attractive as Mr. Livingston's idea is, I think that consideration of his proposal must lead to its rejection.

First of all, how do you get a body which would have the universal acceptance which Mr. Livingston rightly feels it would need? Is it really possible to find to head this organization "an outstanding economist—a person who could command the respect of economists of the New Deal-Fair Deal school and of the so-called 'more conservative' school"? I have very grave doubts that such a person actually exists or that he would long retain such an exalted status if required to assume the task Mr. Livingston would assign to him. Nor would the job of assembling a staff of experts in various fields with the same measure of universal acceptance be an easy one.

Let us suppose that somehow these hurdles can be met. Do we really want to have any economist or group of economists—regardless of how expert, respected, or accepted—placed in a position as the final arbiter in questions of economic analysis and forecasts? I, for one, prefer the existing situation where consumers may pick and choose among economists and their wares and select the product which will best meet their needs for economic information.

I have the very strong feeling that if we were to accept Mr. Livingston's proposal for a private council of economic advisers, we would simply be jumping from the frying pan into the fire. Mr. Livingston urges his suggestion on the grounds that "it would remind all of us that the government's word is not absolute." He tells us that the new organization "would guard against a stampede of monogenous opinion, fostered by government, which could lead to boom and then, perhaps, to bust."

It all sounds very fine. But what about the danger that the new organization

would have the very weaknesses that Mr. Livingston warns us against? Here would be a new fount of monogenous opinion on which government officials and other policy-makers would have every reason to rely because of its supposed absolute objectivity and exalted professional caliber.

Yet we all recognize that not even the best of economists is infallible. In fact, I doubt if even Mr. Livingston's private council of economic advisers would better by much the .500 batting average of the general run of economists. We must not run the danger of establishing an organization which will be virtually immune to criticism when we know full well that it will not always be right. Rather than attempt to create a final arbiter in the field of economic ideas, I would urge that we direct our energies instead to improving the tools of economic research and analysis for the use of all economists.

MERRILL A. WATSON: The instructions for the panelists in this session, although clear, offered great latitude. We were told that the session was designed to present an appraisal of the adequacy of government statistics, particularly for the public and private purpose of business cycle analysis and economic stabilization. We were told further that we might discuss whatever we found challenging in the main speaker's paper and make whatever comments we wished. My discussion of the speaker's paper will be from the viewpoint of the business economist.

The speaker has drawn five conclusions in his paper. The first, based on replies from twenty-five economists, is that the government must undertake to develop a better-equipped statistical armory. The second deals with the changing importance of economic data, the shortcomings of forecasting, and, above all, the need for judgment in the analyzing of data. In his third point the speaker emphasizes the need for long-term economic research by the government. Fourth, he stresses the need for greater flexibility and automation in our built-in shock absorbers and greater authority for the President. Fifth, a new private agency, a counterpart of the Council of Economic Advisers, is recommended.

I should like to comment first on some of the questions contained in the survey of economists. Certainly a few of the answers indicate a reaching for somewhat irrelevant data, although there is no use in debating that point here. One might, however, wish the respondent who stated that "we don't need more data; we need better judgment" had expanded on his views.

I am rather surprised that only a few of the economists surveyed emphasized the need for more current information on orders and inventories, particularly in private industry. There are important statistical gaps in this field. My own observation has been that there is a great need for more and prompt information of this type for forecasting purposes in individual industries. For example, in such private statistical-gathering organizations as trade associations there are very few groups which collect the necessary data for measurement, analysis, and forecasting of the short-term cyclical movements which we often term buying movements or inventory accumulations. A complete picture here, for example, would show the flow of raw materials, production of finished goods, sales, and inventories in dealers' hands for a manufacturing

group selling to the retail trade. It would be too much to hope for a measure of inventories in the hands of ultimate consumers. This information might be supplied by a weekly index of purchases of raw materials; a weekly index of new orders received for finished goods; and monthly data on production, sales, and inventories at the manufacturing level. When to this is attached a picture of sales and inventories of finished goods at the retail level, collected on a monthly basis, a fairly comprehensive "flow chart" for an industry is available.

Some experimentation in this field has indicated that such a flow chart will reveal not only the extent of inventory accumulations and shortages but also the turning points in short-term industry cycles. I might also say that to some of us in industry the collection of this type of information is a general goal. In far too many cases in manufacturing groups we have been placing emphasis on the collection of monthly information on production and inventories. This has provided a basis for historical measurement of long-term trends but has been of little value in detecting and measuring the short-term industry movements which plague many of the consumer goods industries.

I should not like to leave the impression that I believe that the filling of these statistical gaps and the developing of a flow chart of this nature will play an immediate part in curtailing or eliminating these short-term industry cycles. Frankly, I do not know whether it will or not. My observation so far is that the flow charts are extremely interesting and may, over the longer term, be used to a greater extent by businessmen in determining policy. So far, they appear to play a minor role in influencing business policy one way or the other. In the first place, businessmen have not yet developed enough confidence in the information to make proper use of it. Second, businessmen who are participating in an inventory-building boom by shifting their own inventories to their customers' shelves even with proper warning are not inclined to stop and permit their competitors to enjoy increased volume. Once an industry is engaged in this pleasant activity, it is likely to continue until retail shelves are filled, at which time the entire "buying movement" subsides. However, I believe that such a flow chart of activity over the long run will be used more intelligently by business.

The third question was divided into two parts: "What economic policies followed by the government in the past do you consider most open to criticism?" And, "Would better statistical data or research have pointed out beforehand the shortcomings of these policies?" The latter part of this question was apparently not answered. With respect to the first part, the economists were about equally divided, according to their liberal or conservative leanings. What can be said here except that the division of opinion is what might be expected of a sample group of economists and that an advisory board composed of such a group would not be particularly helpful to a policy-making administrator.

The fourth question ("What economic policies would you recommend at this time?") is really a continuation of the third and produces the same division of opinion, illustrating again the dilemma of a public administrator depending upon the economist for proper theoretical guidance. This concludes my comments on the survey of economists.

In his second conclusion the speaker points out some forecasting errors and stresses that additional statistics will not reduce the need for judgment. He supports the view that probable errors are still too great to place complete confidence in forecasting as a basis for developing adequate countercyclical measures to promote economic stability. Certainly this can be said without in any way denying the fact that substantial gains have been made in the way of improved forecasting in the last few decades. New data have been developed and new approaches to the use of data have contributed to progress in forecasting. Furthermore, the many studies of business fluctuations have provided a basis of knowledge without which stabilization policies dealing with inflation, deflation, and unemployment could hardly have been attempted. I am sure we can agree that this progress is likely to continue in the future and that the interest in forecasting will continue as great or even greater than it is today. The more forecasting we do the better we shall do it.

As another point, the speaker makes it clear that while we cannot have complete confidence in economic forecasting to provide an accurate map of economic developments, neither can we depend on automatic snubbers or built-in stabilizers to provide sufficient counteraction when economic events require it. He therefore suggests the President be provided with limited powers to undertake contracyclical action when Congress is not in session. In theory, it is hard to argue this point. However, there are some real hurdles to surmount in reaching this goal.

In the first place, it involves, as the speaker recognizes, the problem of a delegation of powers by Congress. This Congress has not always been willing to do, and it may well involve a substantial political struggle without result. Assuming that the powers would be granted, however, there is then the question of co-ordination of the rather complex governmental administrative structure to get prompt action. Finally, there is the problem of when and what kind of action should be taken. The conflicting advice of economic advisers with respect to action that should have been taken during our recent inventory recession has already been mentioned. If, as a result of this conflict of economic opinion, administrators would have had to form their own opinions on these economic matters whether or not they were equipped to do so, the action taken may or may not be good for the economy.

Finally, practically all of these issues have political overtones. Even with a stand-by committee composed of representatives of both parties the acceptability of any proposed administrative action will invoke great argument. Unless the economic situation is deteriorating to such a degree that all political elements agree on the need for immediate action, there may well be political reverberations which will cause hesitancy and, in fact, postpone action.

As his fifth and last point, the speaker proposes that a counterpart of the President's Council of Economic Advisers should be established by some foundation to provide objective analysis of government plans and projections. Such a group, he believes, would be free from the restrictions which frequently bind those in governmental service.

It may be regretted that the speaker did not include this proposal in his questionnaire to economists so that we might have observed some additional

reactions to this proposal. Perhaps the opinions would have divided about equally in answer to the question on the need for more economic data where a little better than half wanted more data and slightly less than half disagreed or did not care to answer the question. Probably most of us have an insatiable demand for more data and more information and would not, therefore, disagree with any proposal which might provide additional economic information on instability in our economy.

However, I am sure that even an outstanding group of economists working from Olympian heights would have the same problems as economists generally. In the first place, as we have seen, different economists may use the same data and yet arrive at different conclusions. Second, even if unity of approach and conclusion is achieved, the recommendations of the group would probably be characterized as representing either the conservative or liberal school of economic thought and be completely unacceptable to economists of the other school. If its conclusions should be found to be in direct opposition to those of the Council of Economic Advisers, the resulting debates and arguments would add confusion rather than build public confidence in economic research.

Finally, one might wish that an additional recommendation or proposal had been made for developing a broader understanding of the need and importance of economic research both in the political and business field. There is one recent particularly glaring example of how public policy-makers ignore the need for economic data. It was only a short time ago that a Congressional committee decided that such basic economic yardsticks as the various censuses were unnecessary, at least for several more years. And that, in spite of the fact that Congress had undertaken certain responsibilities with respect to economic stability and with the knowledge that there have been great changes in the economy since the last census. To most of us the postponing of the census of manufactures, for example, would have been a catastrophe. Yet it was only through organized effort and the support of many groups that these measures of our economic growth were reinstated.

Moreover, I am sure that the congressmen who voted against the appropriations of funds for this purpose have not changed their minds as to need for the information. This experience can be more or less duplicated by many business economists who are waging a constant fight for a better understanding and appreciation of economic data by businessmen.

It is not surprising, therefore, that all too frequently congressmen and public administrators are not guided by economists, even where good economic research has been done and carefully thought out recommendations are available. Whether it is in the field of business, agricultural policy, labor, or taxes, the ultimate decisions are made by legislative or administrative bodies more responsive to public will and opinion than to sound economic policy.

It is for these reasons, I think, that many business economists experience more frustration in marketing their own product than in finding adequate data. One of the real problems here is the matter of communication. Economists have little difficulty in communicating with each other. But many of us have great difficulty in communicating with the average businessman and with politicians. Part of the difficulty stems from the use of the type of lan-

guage so well described by George Orwell in his admirable essay, "Politics and the English Language." Part comes from the fact that many politicians and businessmen who constitute the market for economic data are even now skeptical of the value of economic research and have little confidence in what economists and statisticians tell them—particularly when different economists tell them different things. Certainly progress has been made in winning the support of businessmen and politicians, but it has been agonizingly slow.

In summary, it may be said that economic research has made it possible to develop a foundation for public policies which can combat the instability in our economy. The economist and economic research are gaining increasing recognition and playing a growing part in providing an analysis of the reasons for instability and prescribing methods of treatment. The influence of economic research for both public and private policy determination is spreading, although at a slow pace. There are important statistical gaps existing today and they should be filled. But what can be done to speed up and broaden the use of the economic data we already have? The prescription here is not easy. Certainly improved communication between the economist and his market will help. In addition, however, if some program either through public or private channels could be organized to broaden the public understanding of the need for and use of economic data and their importance in solving the problem of business instability, it would be most helpful.

ECONOMIC STABILIZATION, FORECASTING, AND THE POLITICAL PROCESS

PROBLEMS OF FORECASTING FOR ECONOMIC STABILIZATION

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I

When the Congress, in the Employment Act of 1946, required the President, in his *Economic Report* to set forth "current and foreseeable trends" in the levels of employment, production, and purchasing power, and the Council of Economic Advisers "to gather timely and authoritative information concerning economic developments and economic trends, both current and prospective . . .," it was evidently their judgment that forecasting could, and should, be employed as a tool of economic stabilization. Yet, there is a fairly common disposition in economic circles to reject forecasting as impossible or too unreliable to be useful in economic stabilization. Indeed, one of the authors of a recent, authoritative symposium on income stabilization reached the conclusion that "in its present stage of development economic forecasting is much too inaccurate to be used as the basis for changes in stabilization measures."[1]

There are good reasons, of which you are all aware, for this pessimistic view of the potentialities of forecasting. But the plain and stubborn fact remains that—I should warn you that this is a main thesis of my paper—there is no escaping the necessity of economic forecasting. Present actions have their effects in the future. If government has any stabilization purpose at all, then in making present decisions as to these actions it inevitably must make frequent, recurrent, and often crucial forecasts. They may be made consciously or unconsciously, scientifically or unscientifically, at one end of Pennsylvania Avenue or the other, but they are still forecasts. There are just two issues, which we hope are not wholly unrelated: (1) how good is the forecasting going to be and (2) how much are economists going to participate in the process?

Before we get down to serious business, let me dispose of two refuges to which economists sometimes flee, especially those who, by virtue of official position, are actually called upon to make forecasts. One is to eschew forecasting as being beyond the competence of any economist,

[1] "The Role of Economic Forecasting in Income Stabilization," by Everett E. Hagen, in Max F. Millikan (ed.), *Income Stabilization for a Developing Democracy*, p. 200.

and when a policy decision or recommendation must nevertheless be made, to assume "no change" in the level of general business activity. This is obviously an implicit forecast, and simply forces the user of the forecast to decide whether or not the assumed "no change" is correct, and if not, to make the necessary adjustments. The other is to make a forecast but to becloud it with a fog of qualifications and ambiguous adjectives of sufficient density to render it largely invulnerable to later attack, but also largely opaque from the viewpoint of those required to make policy decisions. This is another case of passing the buck.

I said a moment ago that forecasting is necessary if government has any stabilization purpose at all. I am assuming throughout this paper that government does have a stabilization purpose and that in fact we want to do an effective job of economic stabilization. The ultimate objective would presumably have two facets: first, the achievement of a long-run growth in national output which does not depart significantly from our capacities for growth, within the limits of voluntary preference for leisure over goods and services, i.e., maximum technological advance coupled with a minimum of involuntary unemployment; and, second, a stable price level, with a gentle upward trend viewed as a not altogether undesirable consequence but not adopted as conscious public policy. Conflict and compromise with other national policy objectives will inevitably preclude full achievement of this goal, but within the limits thus determined, our success in economic stabilization will be measured in important part by the extent to which we are able to make reliable and useful forecasts and to act intelligently on those forecasts.

II

The conventional broad classification of stabilization measures is automatic versus nonautomatic. Automatic stabilization measures presumably require no current forecasting; the forecasting was implicitly done when the law was enacted. The problem relates to their effectiveness. Our system of automatic stabilizers is not symmetrical; not all of the stimulative automatic actions in deflation are matched by corresponding repressive automatic actions in inflation. Moreover, as has often been pointed out, the automatic stabilizers can, at best, mitigate the severity of a decline, and beyond a certain point (e.g., protracted unemployment), their stabilizing effect dwindles toward zero. It is conceivable that a system of automatic stabilizers could be invented which would absolutely stabilize real income, but it would require a rather wide departure from capitalism as we know it. I do not mean

to understate the significance of existing automatic stabilizers. It may well be that they offer a sufficient solution to minor and very short-run fluctuations. But, in the contemporary economic environment, if fluctuations of more than a minor character are still possible—as I believe they are—the automatic stabilizers offer only a partial and limited answer to the problem of economic stabilization.

The nonautomatic stabilizers, however, require conscious decision and positive action. It takes time to recognize and identify an economic development requiring action directed toward stabilization, to determine the appropriate action, and to translate the policy determination into law or regulation—especially if a Congressional enactment is required. A still further time interval ensues between the change in law or regulation and its effect upon incomes, spending, and other economic factors. Such stabilization actions will affect economic behavior usually several months, and often as long as a year or more, after the need for action is recognized. To stabilize effectively, a decision to counteract a decline must be made well in advance of the decline. But this can be done only with forecasting.

It is no solution to the forecasting dilemma to stipulate that stabilization action should be determined by rule of thumb after the fact; e.g., that counterdeflationary measures should be taken after unemployment has risen to a specified percentage of the labor force or that counterinflationary measures should be taken after the BLS index of consumer prices has risen by x per cent for y consecutive months. A forecast is inherently implicit in such a rule of thumb; namely, that recent directions of economic change will continue or at least not be reversed. The tightening of Federal Reserve credit controls early in 1953 is a good example of a delayed action after the fact where the implicit forecast turned out to be considerably in error. The easing of credit policies a few months later, however, is an example of an action which would scarcely have been indicated by an after-the-fact rule and which, therefore, was presumably based on an explicit forecast—a forecast which turned out to be pretty good.

Let me hasten to add that taking action after the fact is usually better than nothing. It will produce an answer which, though probably inadequate and inevitably tardy, is in the right direction more often than not, simply because the momentum of our economy is such that straight, short-run projections of recent tendencies turn out to be right more often than not. Nevertheless, to the extent that anything more than a compensation for past fluctuations is attempted by taking action after the fact, implicit forecasting is involved; it might be called "forecasting by assumed momentum."

III

So far I have argued that forecasting is necessary to successful stabilization without indicating what kind of forecasting I have in mind. Forecasting by a government agency for stabilization policy purposes differs in one important respect from general economic forecasting for private business purposes. In either case, the forecast must be based upon certain initial assumptions as to forces outside the economic system per se, such as war, cold war, or changes in law, or other governmental action. The private forecaster may simply assume no change in government policy on the ground that political forecasting is beyond the competence of the economist, and then leave it to the user of his forecast to make his own assumptions as to changes in government policy and make his own adjustments in the forecast. Or he may make a best guess as to what the political process is likely to bring forth in the way of tax changes, expenditure programs, and the like, and then having stated his assumptions, proceed with a definite forecast. There is nothing inherent in the method to preclude a forecast of another 1929-32.

In the case of a forecast by a stabilization agency, however, government policy is not a completely exogenous variable. The forecasting activity is itself a part of the process of policy determination. A forecast by the Council of Economic Advisers, for example, will normally involve two steps. The first is a preliminary forecast, based on the assumption of no change in private or public policies (or perhaps on the assumption of certain changes which are expected to be made for nonstabilization reasons). This preliminary forecast might well indicate that recessionary tendencies are in prospect. But it cannot stop there. The whole purpose of the forecasting process is to identify those policy changes which are needed to prevent a depression from developing—changes which are needed to make the preliminary forecast become untrue. It must proceed, after identifying these needed changes, to incorporate them into its analysis, to forecast the effects of these changes, and thus to point the way to satisfactory levels of economic activity. Such a forecast is not likely to be a flat prediction of a depression, certainly not another 1929-32. A better term, perhaps, for this kind of a forecast is a "conditional economic projection"—a projection that certain levels of economic activity should prevail on condition that prescribed changes in public policy, and perhaps also certain changes in private policy, are made.

This does not necessarily mean that such a projection by the Council of Economic Advisers must always point to a high level of economic activity or to stable prices. The Council might determine that, as a result of actions taken or not taken in the past, some decline in business activity or some inflation in prices is beyond the effective capacity

of available or conceivably obtainable stabilization measures. I doubt, for example, that any realistic deployment of stabilization measures in 1946-47 could have prevented some inflation. The seeds of inflation were sown long before. Similarly, the seeds of recession may be sown by past failure to control the excesses of the boom. Or the Council's projection might reflect the judgment of government as a whole that other national policy considerations, such as national defense, take priority over stabilization policy and that completely successful stabilization is therefore precluded. But apart from such situations, the "conditional economic projection" should point toward a satisfactory level of production, employment, and purchasing power. This is the mandate of the Employment Act.

I dwell upon this point because I have encountered two attitudes toward the forecasting activities of the Council of Economic Advisers, both of which I believe to be in error. One is the impression, because the Council's projections do point to good levels of economic activity, that the Council is nothing more than a propaganda agency, trying to talk us into prosperity. This attitude arises in part from a lack of candor in the preliminary or initial analysis of economic prospects by the Council. But also, and perhaps more fundamentally, it arises from a failure to recognize the character of this conditional economic projection—that it is not a "cold turkey" forecast, but rather a statement of what should eventuate if appropriate policy changes are made.

The second misimpression is the notion, sometimes subscribed to within the walls of the Council as well as elsewhere, that the requirement for a reasoned but optimistic conditional economic projection, as the end result of the forecasting and policy analysis process, necessarily precludes a frank and forthright statement of the unfavorable aspects of the economic outlook in the course of the analysis. This is admittedly a delicate issue. The Council is presumably an authoritative forecaster. It is authoritative, not only because of the presumed competence of the members of the Council and its staff and because it has approximately full access to all available information, but also because it is a part of the presidency—because it plays an important role in determining what government policy shall be and therefore to some extent is in a position to make its forecasts become true. Because it is thusly authoritative, its pronouncements carry weight in the public mind. The "feedback effect" of a pessimistic analysis therefore could conceivably be considerable. In light of this circumstance, can—or should—the Council be completely frank and forthright in its preliminary analysis?

As I see it, there are two aspects to the answer to this question. First, in spite of the admittedly authoritative character of the Council's forecasts, it is my experience that officials of government typically

exaggerate the public impact of what they say. (Occasionally they underestimate, as in the case of Mr. Wilson's dogs, but this is the exception.) The belief that a frank, preliminary analysis of all factors involved, including unfavorable elements, will necessarily have adverse effects rests on the implicit assumption that businessmen and others who make important economic decisions have little or no knowledge of what is likely to happen beyond that which is told to them by government. This view seriously underestimates the economic literacy of Americans, notably businessmen. They form their judgments on the basis of a variety of economic information, of which government pronouncements are only a part. The feedback effect of a frank analysis, in the business community at least, is a moderate one at most.

Second, it is entirely possible to assess squarely the unfavorable elements in the business outlook in an unsensational and matter-of-fact manner which will minimize any feedback effect. True, if an *Economic Report* were to open with the shocking words, "A serious depression is impending unless drastic action is taken," the feedback effect might be enormous. But a report which calmly and dispassionately analyzes the many facets of the economic outlook, the unfavorable along with the favorable, need not arouse excitement. Indeed, failure to recognize unfavorable elements, which are clearly apparent to others, may well be destructive of public confidence by suggesting that government does not know the economic facts of life and is therefore unprepared to cope with these unfavorable developments. In my judgment, the most confidence-creating course of action is a frank and highly objective preliminary forecast coupled with a policy prescription based on that preliminary forecast adequate to insure realization of our stabilization objectives.

IV

The character of a forecast, or economic projection, is inevitably influenced by the method employed in making the forecast. Forecasting methods based on single or exclusive causes, whether they be sun spots, weather, or changes in the quantity of money in circulation, have now been generally and properly thrown into the discard. So have techniques based on an assumption of rhythm in business fluctuations. One could scarcely have lived through the last twenty-five years and still adhere to the theory that business conditions can be determined by extrapolating one or more sine curves.

Another forecasting technique deserves more attention, if only because it is a technique which apparently is looked upon with some favor in the offices of the present Council of Economic Advisers. This is the technique, developed most thoroughly at the National Bureau of Eco-

nomic Research, of identifying certain economic variables which, in the past, have tended to lead general business fluctuations, and of watching these series closely for signs of an impending turn. Mr. Geoffrey Moore has identified eight such series which, I am sure, are familiar to all of you.

This technique of forecasting has an appeal because of its statistical objectivity, and it unquestionably has certain usefulness, particularly for calling the turn on short-run fluctuations. As a sole or even primary instrument of stabilization planning, however, it suffers from four serious defects which, I am sure, the National Bureau fully recognizes. First, the technique is largely symptomatic. It is heavily dependent upon the assumption that certain structural relationships within the economy will persist into the future. By implication it assumes, for example, that the relationship between commercial and industrial construction contracts awarded to total output is impervious to changes in industrial technology, or that the relationship between so-called "sensitive" prices and prices in general is not affected by shifts in the composition of national output. Second and more important, because it is symptomatic, there is nothing inherent in the forecasting process which throws light on the fundamental operative causes of economic change. The technique itself, therefore, provides no indication of needed remedial action.

Third, by focusing attention on turning points, the lead-and-lag technique may lead the forecaster into paying insufficient attention to the probable level of economic activity. A case in point is the degree of satisfaction with economic prospects expressed in the January, 1954, *Economic Report,* apparently based on indications that no major downturn was probable, in spite of the fact that for several months prior to that time, and since, business activity failed to grow in proportion to increasing productivity and an increasing labor force. And, fourth, it is short-run forecasting. At best, it can predict only a few months into the future. Such short-run forecasting is, of course, highly useful. But the much greater need is for longer run projections which permit a more fundamental diagnosis and a more effective use of long-lead-time stabilization techniques.

A much more useful and promising approach to forecasting for economic stabilization is income and product model building—a technique which can be traced back to 1932, and perhaps even earlier, but which has been practicable only since the expansion by the Department of Commerce in the late thirties of their national income and product accounting series and which has been experimented with on an extensive scale only in the past ten or fifteen years.

The technique is now well understood and rather widely used. Three outstanding recent examples are *Markets after Defense Expansion,*

published by the Department of Commerce, Gerhard Colm's *The American Economy in 1960*, and *Potential Economic Growth of the United States during the Next Decade*, by the staff of the Joint Committee on Economic Report. All three of these studies, incidentally, disclaim being forecasts. To the extent, however, that the projections of demand made in each of these studies are supposed to be realistic and feasible potentialities, given proper public policies, they come close to being forecasts of the type I have in mind.

One important advantage of income and product model building is that it encompasses the entire gamut of economic activity. The method itself requires the forecaster to consider, in as much detail as is practicable and necessary for the purpose, all of the components of the totality of demand. The completeness of the analysis is limited by the sufficiency and accuracy of the raw material, factual and theoretical, which is fed into the model, not by the model technique itself. For the same reason, model building tends to prevent the forecaster from placing undue emphasis on one particular variable. The mere act of listing all of the components of gross national expenditure and assigning reasonable values to them subdues the temptation to select one dramatic one, such as residential construction or military expenditures, and attach exaggerated significance to it.

A related advantage of income and product model building is that the technique requires the projection not only of demand for total national output but also of potential supply at given levels of employment. It forces the forecaster to recognize and make allowance for the effects of growth in the labor force, increasing labor productivity, changes in the labor participation rate, the average work week, etc. Failure to do this can easily lead, for example, to complacent conclusions to the effect that 1954 is "our second best year," which it definitely is not when measured in terms of potential output. This is a particularly relevant consideration as regards forecasting by the Council of Economic Advisers. The Employment Act requires the President to set forth levels of production, employment, and purchasing power needed to carry out the policy of the Act; that is, realizable output goals against which performance can be measured. The *Economic Report* of last January failed to define these goals. Such goals, whether they be maxima or minima, optimistic or pessimistic, are an essential element in income and product model building.

Perhaps the most important advantage of income and product model building is that it integrates aggregative economic theory and the forecasting calculation. Economic analysis becomes a part of the forecasting process and not supplemental to it. In essence, model building is translating macroeconomic theoretical concepts into numbers, past and prospective. Theoretical concepts as to functional relationships, such as

the relationship between income and saving or between growth in real disposable personal income and business investment expenditures, can be put into appropriate combinations of numbers in the model. Moreover, prospective changes in these functional relationships can be reflected in the model. They are not necessarily assumed to be fixed, as in the case of the leads-and-lags approach.

Model building, however, also has its disadvantages and limitations. One disadvantage—which is not inherent in the method but which is usually present in actual application—is a temptation to focus on highly aggregated magnitudes at the expense of detail. This disadvantage, I think, has been a more real one to the econometricians who have attempted to reduce the relationships involved in the model to a finite system of equations of manageable proportions than it has been to those who use the nonmathematical or quasi-mathematical approach to model building, where a much larger number of variables can be handled in an approximate, if nonalgebraic and inexact, fashion.

A second disadvantage in model building—which again is not inherent but which I believe is usually present—is some tendency toward a pessimistic bias. Projections of demand include demand for those already existing goods and services which can be estimated from data on consumer stocks, sales, apparent trends in consumer habits, etc. But they also include the demand for the new and the unknown. It is easy to see and predict the decline of the old but is extraordinarily difficult to foresee the rise of the new.

A third practical difficulty is that the process is inevitably slow and laborious, and it is scarcely a one-man job. The voluminous data required for a well-reasoned projection are slow in becoming available. The time required to analyze the data and all of the major interrelationships involved and to revise preliminary assumptions and subsequent calculations is considerable. The slowness of the technique, on the other hand, is in some respects an advantage. It prompts the analyst to concentrate on fundamental changes in economic activity rather than on the most recent wiggle in this or that curve. What is lost in the way of currency of forecast of very short-run fluctuations may well be more than compensated for by less ephemeral and more thorough analysis of the intermediate and longer run fluctuations which constitute the important economic stabilization challenge.

V

This leads me to the second main thesis of my paper, as follows: Forecasting techniques have advanced to the point where they are beginning to provide a reasonably reliable basis for economic stabilization policy making. I believe that much of the pessimism regarding the potentialities of forecasting, to which I referred earlier, is exaggerated

or outdated. I am not suggesting that we have the problem solved, or that anything resembling perfect forecasting is possible. Rather, I am suggesting that it is possible to develop income and product models which, supplemented by leads-and-lags and perhaps other analyses, show the general direction of economic change, its approximate though not exact timing, and at least a rough gauge of the magnitude of that change. And I am suggesting, further, that such projections provide a sounder basis for stabilization policy making than do intuitive judgment, extrapolation of the *status quo,* or action after the fact. Let me indicate briefly some of my reasons for this cautiously optimistic view of forecasting.

First, it hardly needs to be said that great progress has been made toward a better understanding of the process of economic change in the last two decades. This is not mere breast-beating on behalf of the economics profession. A healthy scientific skepticism toward our profession is all to the good, and we should remind ourselves frequently of past failures of economic analysis, especially of those of the late twenties. But skepticism and self-abnegation can be carried too far. It is a plain fact that economic science has made great strides in the past twenty years or so. Thanks largely to the revolution in economic thinking touched off by the depression and by the *General Theory,* we have added enormously to our supply of tools of economic analysis. We are in a much stronger position to attack and solve economic problems than we were a short generation ago.

Second, one of the main exogenous variables of former years is no longer so unpredictable: government policies affecting economic activity. This may be attributed partly to the commitment of the Employment Act, but it is also due to the clearly evident increase in economic understanding on the part of responsible officials of the government—notably members of the Congress.

Third, the adequacy of data needed for forecasting is gradually improving. Although much remains to be done, recent years have witnessed a steady improvement in the quantity, quality, and timeliness of economic statistics. The series on manufacturers' new orders deserve special mention in this regard. And beyond this general improvement, one statistical innovation of great signifiance has been made: the application of scientific sampling techniques to the analysis of intentions to spend, such as the Commerce-SEC and McGraw-Hill surveys of business investment anticipations and the consumer surveys conducted by the Michigan Survey Research Center for the Federal Reserve and certain private organizations.

These surveys are relatively new. They suffer from certain inherent sources of error, including: the fact that many respondents, even large corporations, frequently make only very tentative plans as to future

actions; and individual responses inevitably involve an implicit forecast as to general economic developments, which may prove to be wrong. The surveys have not been tested by a major decline in business activity. Nor has enough work yet been done to develop them to their maximum usefulness. Experience to date, however, is encouraging. The Commerce-SEC projections of total expenditures for new plant and equipment have corresponded closely with actuals, both as to direction of change and magnitude, in every year except 1950, and the industry-by-industry projections have been accurate as to direction of change in a high percentage of instances. (See Louis J. Paradiso's paper on "Economic Projections by the U.S. Department of Commerce," scheduled for publication in the *Proceedings* of the American Statistical Association meetings of September, 1954.) The Michigan surveys of consumer attitudes toward purchases of durable goods have accurately predicted direction of change and have not been too far off as to magnitude. (See, for example, Rensis Likert's statement in *Hearings* before the Subcommittee on Economic Statistics of the Joint Committee on the Economic Report, July, 1954, pages 288-295.)

As experience with these surveys increases, it should be possible to improve them further and to develop improved techniques for compensating for inherent sources of error particularly if better information can be obtained as to the character of implicit forecasts and as to degree of intention; i.e., the firmness of the planned expenditure. Some progress along these lines has already been made.

If I may continue to digress for a moment, I would like to mention briefly two other improvements in data which would make a further contribution to our ability to forecast. One of these would be the regular publication by the Department of Commerce of quarterly, seasonally adjusted estimates of gross national income broken down on an after-tax, after-transfer basis which would be comparable with our present so-called "gross national product" series. What I have in mind in this regard is the kind of arrangement of data which used to be published semiannually by the Council of Economic Advisers under the title, "The Nation's Economic Accounts." In spite of statistical imperfections, this tabulation is an extremely useful analytical tool. Unfortunately, it now survives only in occasional publications of the Joint Committee on the Economic Report, in a vestigial annual appendix table in the *Economic Report of the President,* in a chart with no numbers attached in *Economic Indicators,* and in Gerhard Colm's work at the National Planning Association.

Second, we need better information for measuring and projecting changes in the economy's productive potential. We need quarterly series showing man-hours worked in total and in the nonfarm private, the agricultural, and the civilian government sectors of the economy. And

we need more comprehensive and accessible measurements of productivity and more research into the causes of productivity increase.

There is one facet of the productive potential about which my own thinking is not yet clear and which there is not time to develop in this paper. Three years ago the Paley Commission focused our attention on the proposition that the real unit costs of the material and energy inputs into the American productive process may have finally emerged from their secular decline and may now be predominately on the increase. Ever since then, I have had the uneasy feeling that our rather facile projections of past trends in real output per man-hour—particularly the longer run projections—do not give adequate consideration to this possibility.

My fourth and final reason for optimism—to return to the main stream of my argument—is the impression, which I cannot prove, that by the pragmatic test forecasting has fared much better in recent years than it did some years ago. The projections of the Council of Economic Advisers, though frequently ambiguous and hedged about, have been essentially accurate, save only for the misfire of January, 1949. The projections following the Korean invasion were generally good, in spite of the complications injected by strong psychological factors ("Is This World War III?") and in spite of exaggerated estimates by the military as to anticipated levels of defense expenditures. The model builders, almost unanimously, forecast the current—or should I say recent—recession, at least as to timing and, with a good deal of trepidatory hedging, as to magnitude also.

In spite of this improved record, there is still widespread reluctance to bring models out into the open. There have been a few exceptions—notably the reports of the staff of the Joint Committee on the Economic Report under Grover Ensley's direction. The Congressional members of the Joint Committee deserve commendation for encouraging these studies. But these are exceptions. In the main, there is still an air of black magic about the business of forecasting. The time has come, I believe, for serious and responsible forecasters, especially those in authoritative positions, to open their books to the public, to state specifically their assumptions as to exogenous factors, to describe fully the techniques and logic by which estimates are calculated, and to develop preliminary projections in the form of quantitative models. But I would like to emphasize again that in forecasting for economic stabilization purposes, the number work is only the beginning of the process. More than mathematics, whether it be higher or lower mathematics, is required in forecasting for economic stabilization. The important part of the job is the application of judgment and understanding of both economic process and political process to the identification of those changes in economic policy, private and public, which will lead to high employment and stable economic growth.

POLITICAL ELEMENTS IN FULL EMPLOYMENT POLICY

By STEPHEN K. BAILEY
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Earlier in this century, a distinguished economist wrote a book the last chapter of which was entitled, "If I Were Dictator." It was a glowing account of what economic measures would save society if only politicians were not around to louse things up.

My theme today is built around the dismal forecast, now widely shared by most economists, that politicians are likely to be around for some time to come. Let me hasten to add, however, that there are stabilizing factors at work and that the secular trend is bullish, although the rate of development may not be great enough to guarantee in the short run the maximum utilization of all the resources of the American Economic Association and the American Political Science Association.

It is ten years, almost to the day, since a group of economists, political scientists, and politicians sat around a table on Capitol Hill and drafted a so-called "Full Employment Bill." In the eyes of history, this temporary confluence of professions may have been of more importance than the Employment Act itself. For one of the central problems before both economists and political scientists is how to make politicians as smart as we are and (if I may be pardoned a heresy) vice versa. This is what Lord Macaulay had in mind when, referring to currency reform in the reign of William II, he said: "It would be interesting to see how the pure gold of scientific truth found by the two philosophers was mingled by the two statesmen with just that quantity of alloy which was necessary for the working."[1]

The problem is not new. What is new is the degree of our own self-consciousness that an interdependency exists. One of the salutary results of the Employment Act has been the forcing together of professional economists and professional politicians on a scale hitherto unknown in this country. In Mr. Nourse's felicitous expression, "the research division, the engineering division, and the sales division," have been constrained to sit down together. Even if Mr. Nourse himself found this confluence at times an unhappy one, he has enormously increased our sum of knowledge about the pitfalls of this necessary collaboration.

I do not wish to attempt a detailed summary of the obvious. You are more fully aware than I of the political elements in full employment policy. In fact, some of the most sophisticated writing in this field has been done by economists. I need only refer to the writings of Mr.

[1] Quoted in C. H. Hankin, *The Mental Limitations of the Expert* (India: Butterworth & Co., Ltd., 1921), p. 27.

Blough and Mr. Nourse to make my point. They, and others, have listed the tough ones.

But purely as a preliminary catechism, let me recapitulate a few major points:

1. America is a vast and highly pluralistic society. There is no hand, seen or unseen, which is capable of homogenizing all of the diverse political and regional appetites of our country into a uniform, Grade A public interest. This frequently leads to shifting compromises and "second bests" in economic as in most other policies.

2. Full employment in a free society is an aggregate concept. The constituent pressures on a congressman are often highly "unaggregate."

3. Politicians, like some other people who shall remain nameless, occasionally differ on the subject of what policies actually will produce a condition of full employment. The difference between politicians and others in this respect is that if politicians guess wrong, they run the risk of losing their jobs. They have a tendency, therefore, to do a very cowardly thing: they hedge.

4. There is in America a tradition of what Burt Marshall has called "serendipity." Serendip is an ancient term for Ceylon. Mythology recalls that there were once three princes of Serendip. In Mr. Marshall's words, these three princes were "special favorites of benign fortune. Things never worked out as the princes planned them, but they always worked out remarkably well, nevertheless, and the princes went through life blessed with unintended and unforeseen happy consequences."[2] The principle of serendipity accounts for whatever truth there is in Grover Ensley's claim that, on the whole, Congressional action in the economic field during the past ten years has been sounder than the advice of economic experts. Serendipity makes some congressmen smug —more smug, perhaps, than is warranted by an uncertain future.

5. The phrase "full employment policy" seems to imply a consistent and co-ordinated federal effort. That co-ordination is made remarkably difficult by the extraordinary jogs in Pennsylvania Avenue from 17th Street, N.W., to 2nd Street, S.E. Twists of institutional jealousies and prerogatives in the tax and appropriations lanes alone make driving hazardous and destinations uncertain.

6. Even within the Executive Branch, Departments like Agriculture, Labor, Commerce, Interior, Treasury, and the Veterans Administration do not always see eye to eye with each other—or with the President. And there is of course the problem of independent and quasi-independent agencies like the Federal Trade Commission, the Interstate Commerce Commission, the Federal Reserve, and Mr. Wright Patman.

7. Even within the Executive Office of the President, there is no

[2] Charles Burton Marshall, *The Limits of Foreign Policy* (Henry Holt & Co., 1951), p. 46.

assurance that all those who should, actually will meet together in the lobby of the Sherman Adams Hotel.

8. State and local governments make decisions about spending and investment on the basis of political considerations frequently ignored by department heads and presidential advisers in Washington. I happened to be Mayor of Middletown, Connecticut, when Mr. Humphrey, with the help of some friends in the Federal Reserve, slammed on the brakes in the spring of 1953. Fortunately for Middletown (and perhaps for Mr. Humphrey and the United States), his foot slipped. We would have built the schools anyway, but an interest rate of 1.90 is a lot easier on the books and the ballots than 3.25. And never underestimate the power of local officials. Representative Seeley-Brown is a Republican and I am a Democrat, but I called him Seeley and he called me Steve.

9. State and national elections come periodically. It is becoming increasingly obvious that a "healthy readjustment" is easier in odd years than in even.

10. Finally, the apocalyptic spectrum known as international relations is in the hands of politicians, here and abroad. Full employment policy is mightily conditioned by international events, even if the reverse is also true.

These are some of the major political variables which stand between the scientific economist and consistent economic policy. They are what I would call the centrifugal forces in our government. The search of both economists and political scientists in the past two decades has been for countervailing, centripetal forces which might increase the rationality of decision making in the field of economic policy. We have a long way to go, but I think we ought to count our blessings. Progress has been made, and the progress is impressive.

The first centripetal force has been in the field of economics itself. Through brilliant analytical work, the economics profession has narrowed its own areas of disagreement. There are few economists left, for example, who would argue for a tax rise in a major depression, as Roosevelt did. The crystallizing orthodoxies of the economics profession make their way into both branches of the federal government, and after a slight delay have an enormous impact upon policies. As Lord Keynes wrote so brilliantly:[3]

> The ideas of economists and political philosophers, both when they are right and when they are wrong, are more powerful than is commonly understood. Indeed the world is ruled by little else. Practical men, who believe themselves to be quite exempt from any intellectual influences, are usually slaves of some defunct economist. Madmen in authority, who hear voices in the air, are distilling their frenzy from some academic scribbler of a few years back. I am sure that the power of vested interests is vastly exaggerated compared with the gradual encroachment of ideas.

[3] J. M. Keynes, *General Theory of Employment, Interest, and Money* (London: Macmillian & Co., Ltd., 1936), pp. 383-384.

There has been a heartening increase in agreement among the members of the Joint Committee on the Economic Report during the past six years, and I submit that this increase would have been impossible without the establishment of prior agreement within the economics profession itself.

The second centripetal force has been at the level of executive organization. The tentative, and almost accidental, character of Executive Branch interest in problems of economic stabilization forty years ago is reflected in a letter written to President Wilson by his Secretary of Commerce dated May 7, 1913. The letter reads as follows:[4]

> Sir: There is much practical wisdom in the suggestion made by you to Secretary McAdoo yesterday, respecting the advancement of public works under present conditions, that I venture to add a word in the same line.
>
> The point of attack in the matter will not be with the executive departments. These, inspired by you, will act quickly when authorizations and appropriations allow. It is the chairmen of the two appropriations committees who control the situation. I would suggest a conference, at early convenience with these chairmen and with Mr. Underwood, and others if you prefer, to point out to them the practical value to us now (not at a future date, but today) of such appropriations as will permit the progress during this summer and fall of contract work as fully as possible.
>
> The Department of Commerce has asked appropriations for lighthouse and other work already authorized, aggregating something over a million dollars, at many different points. They are no more needed than other public works, but they would add to employment and the distribution of funds, and if appropriations were deferred to the next session of Congress a year would be lost, during which much might happen. Practical common sense would seem to indicate the wisdom not only of appropriations for needed public works, but the political expediency of having those appropriations quickly, for the reason so cogently stated by the Secretary of Labor yesterday.
>
> Respectfully,

In regard to Executive-Legislative relationships, this letter is as hot as tomorrow's headlines. Many of the problems it raises are still unanswered. But there have been major changes in the institutional mechanisms for apprising the President of what he can or should do. The great landmarks in this development have been, of course: the Budget and Accounting Act of 1921; the Reorganization Acts of 1939, which established the Executive Office of the President; the Employment Act of 1946; the National Security Act of 1947; and the subsequent administrative additions and changes which have been made within the Executive Office framework itself. The Executive departments and agencies still ride off in a variety of directions; strategic and tactical moves in relation to Congress are still handled gauchely at times; but the clearance and advisory mechanisms in the Executive Office have markedly increased the chances that policy free-wheeling will be a matter of political choice not of staff default. And these mechanisms are constantly being refined. The reconstitution of the Council of Economic Advisers in 1953, for example, established an Advisory Board on Economic Growth and Stability under the chairmanship of Arthur Burns.

[4] Letter from William C. Redfield, Central Records of the Department of Labor in the National Archives, 16:71.

This interdepartmental board includes representatives, at the under or assistant secretary level, of Treasury, Agriculture, Commerce, and Labor. It also includes a member of the Board of Governors of the Federal Reserve System, the Assistant Director of the Bureau of the Budget, and Dr. Gabriel Hauge of the White House staff. There is no necessary magic in interdepartmental committees. Washington probably suffers from too many of them. But in this particular case, an interdepartmental advisory committee may be an extremely useful educational and policy co-ordinating device.

A third series of centripetal forces have developed in Congress and in the structure of political parties. The Legislative Reorganization Act of 1946 was not an unmixed blessing in this regard, but it did result in the establishment of Majority and Minority Party Policy Committees in the Senate, it did cut down on the number of standing committees, and it did focus attention on the problem of diffusion in Congressional activity. The very existence of the Joint Committee on the Economic Report and the Joint Committee on Internal Revenue Taxation with their respective staffs indicates that Congress is aware of the need for a more unified appraisal of fiscal and economic programs.

Both Republican and Democratic National Committees have increased their research services to Congress and have sincerely tried to find common denominators of agreement on economic issues within their respective parties.

Regularized channels of communication between the President and the Congress have increased in quality and quantity during the past two decades. I refer not only to the care now taken in the preparation of the three annual presidential messages—on the "State of the Union," the budget, and the *Economic Report*—but to the new liaison status given to the Vice President, the weekly meetings between the President and his Congressional leaders, and the more systematic use of department heads in defending the President's program before the Congress.

These are all encouraging signs. But, unfortunately, centripetal mechanisms, no matter how carefully they are designed, are not enough. As J. S. Mill once said: "In politics, as in mechanics, the power which is to keep the engine going must be sought for outside the machinery." In the final analysis, economic stabilization like any other policy goal depends upon the system of value priorities and the political skills of those who wield political power. No matter where we turn in government we find that economic goals are conditional. Man does not live by bread alone. The goals of physical survival and of freedom itself are not necessarily in conflict with, but they are certainly antecedent to and higher on the value ladder than any circumscribed economic goal—no matter how desirable. To get at these value interrelationships we need

a lot more than a Council of Economic Advisers or a Joint Economic Committee. We need political leadership at both ends of Pennsylvania Avenue—leadership which is capable of performing the highest task of statesmanship: the mobilizing of power behind articulated goals. In the kind of governmental system we have in America, this is an extraordinarily difficult assignment. Lines of responsibility and accountability are not clear cut. The vast and frequently competing congeries of power represented by interest groups in our society are not easily moralized or even contained. The shifting sands of public opinion and the swiftness of changing events make the very task of goal setting and political implementation extremely hazardous. And yet the mobilization and harnessing of political power is the only master key to the development of consistent programing in the economic or any other field.

This puts an extraordinary burden upon the President of the United States. Mr. Truman realized this when, without prophetic insight as to his successor, he stated that the President's desk is like a general's. Truman outlined what he called the five vital steps of strategy: (1) estimate your own resources; (2) estimate your enemy's resources; (3) form a judgment as to what is to be done; (4) implement that judgment with a plan; and (5) persuade your leaders of the value of that plan, and mass your forces for the attack.

This is almost a perfect description of what most presidents have not done or have done badly. Presidential leadership means what human leadership has always meant: taking account of multiple forces and interests in a more subtle and forceful way than traditional, formal procedures generally permit. Possibly the greatest single weakness of the present administration is its faith that institutional arrangements, secretariats, and rationalized divisions of labor in the Executive Branch are by themselves adequate to deal with the complex and diverse value configurations of our national polity. The setting and dramatizing of goals, especially in the economic field, have been intermittent, often contradictory, and almost casual. It was not until the exigencies of a political campaign forced the issue that the President was willing to set economic goals for longer than a month or two in advance. I only hope and trust that this reluctance to perform the highest function of presidential leadership was not due to staff suggestions that because forecasting is not economically "scientific" the President should ignore his prophetic role. If this were the case, it only lends fuel to the fire of my own conviction that untainted, professional, scientific economists belong back in a dark corner of the Budget Bureau, not at the level of direct advice to the President. However hard it may be on the egos of some economists, the Council of Economic Advisers was not set up to please the American Economic Association. The Employment Act is

quite clear about the policy functions of the Council. The Act states explicitly that each member of the Council shall be a person who, and I quote, "as a result of his training, experience, and attainments, is exceptionally qualified to analyze and interpret economic developments, appraise programs and activities of the government in the light of the policy declared in Section 2, and *to formulate and recommend national economic policy* to promote employment, production and purchasing power. . . ." (Italics supplied.)

The Council, in short, was established to help the President perform his goal setting function, to help him as a sorter of values, and to help him in his role as a mobilizer of political power. This makes the Council, inevitably, a participant in the political process, not an adjunct thereto. The Act implies that members of the Council should be something more than economists, that they should be in tune with the value premises of the President, and that like the President they should be politically expendable with changes in administration.

I am aware that this is a controversial subject among economists—and that I am taking an unpopular position. But I must as a political scientist state my position with some emphasis. I do not believe that economists have any right to participate in high policy making without running the risks of political involvement and political compromise. Those who will not play unless they are guaranteed immunity against politically-tailored value commitments remind me of the early Fabian socialist, Joseph Burgess, who refused to compromise on some issue during a general election and thereby lost his seat in Parliament. George Bernard Shaw complained bitterly as follows:[5]

> When I think of my own unfortunate character, smirched with compromise, rotted with opportunism, mildewed by expediency . . ., putrified by permeation . . . I do think Joe might have put up with just a speck or two on those white robes of his for the sake of the millions of poor devils who cannot afford any character at all because they have no friend in Parliament. Oh these moral dandies, these spiritual toffs, these superior persons. Who is Joe anyhow that he should not risk his soul occasionally like the rest of us?

If an economist wishes to limit his activities to objective economic analysis, he has a perfect right to do so; but that right should be exercised about three steps or three blocks removed from the President. What some economists call the "scientific" function is an indispensable function in modern government, but it is a totally different function than that prescribed for the Council of Economic Advisers by the Employment Act. If the Employment Act had established the Council as a Supreme Court of Economic Truth, it would have created an administrative monstrosity. The Presidency creates a magnetic field which structures everything it touches with the electricity of politics. An economist has no option. He must either escape the field completely,

[5] Hesketh Pearson, *G.B.S.* (Harper & Bros., 1942), p. 156.

or he must be drawn into the magnetic orbit. The economics profession cannot have it both ways. It cannot claim political immunity for Mr. Burns and at the same time ignore the fact that he made a major public speech in Detroit, Michigan, two weeks before a national election.

With all due respect to Mr. Nourse, the tortured early history of the Council was in part his own making. He attempted to make the Council into something it was never intended to be and, at its peculiar level, never can be: an oasis of pure economic science. Mr. Burns, with rare and notable deviations, has doubled in spades. This perspective has tended to make the Steelmans and the Hauges, the Snyders and the Humphreys the true economic advisers to the President and has stringently limited the Council as a true policy arm of the Chief Executive. Mr. Keyserling may have carried the logic of his own position and personality to unnecessary extremes, but this was in part an understandable reaction to the sincere but pristine myopia of his distinguished predecessor.

The present sorry and bitter hiatus between the Council and the Joint Committee is more than a matter of personalities. It is a direct outcome of a misreading of the proper role of the Council. There may be good tactical reasons for keeping the Council away from public hearings before Congressional committees, but there is every reason in the world to keep constant and friendly informal contacts rolling between the Council and the Joint Committee. There is every reason, that is, if both the Council and the Joint Committee are policy-oriented. If the Council is not policy-oriented—if it conceives of its chief function as that of adding up freight car loadings to give the President a Monday morning statistical summary of current indices and short-run projections —the Joint Committee is inevitably left high and dry as a potential weapon of presidential influence in the field of economic policy.

Goal setting, although the supreme task of a President, is not worth much unless there is political follow-through. In the economic field, unless the Council helps—at least covertly—in this follow-through, the President must depend upon *ad hoc* expedients which lack both the strength of considered strategy and the ideological force of economic sophistication. A little recent history is perhaps germane at this point.

In November, 1947, the Council of Economic Advisers reported to the President and the Cabinet that the then inflationary dangers were serious to both the short-run and the long-run stability of the economy and suggested a ten-point legislative program to handle the situation. The ten-point program was submitted to a special session of the Eightieth Congress with neither proper bill drafts nor Executive Branch clearance on testimony to back it up. As far as I can judge from a study

of the records, there was not even a suggestion of strategic thinking about pressure groups and Congressional support. In January, 1948, the President reiterated this demand for anti-inflation legislation in his "State of the Union" message. Again there was no conscious strategic planning in the Executive Office or elsewhere for the active mobilization of a working coalition to get results. In the summer of 1948, President Truman called a special session of Congress and again submitted an anti-inflation program. Virtually the only concession to strategic thinking this time was a hurried call to Paul Porter to testify, after a superficial briefing, before a Congressional committee on behalf of the President's program.

After the November elections and in preparation for the Eighty-first Congress, the President appointed Dr. Nourse, then chairman of the Council of Economic Advisers, as head of an *ad hoc* cabinet committee to work out an omnibus economic stabilization bill. In January, 1949, after much of the technical preparatory work had been finished, Secretary of Agriculture Charles Brannan took on the job of developing agency and Congressional support for the bill. Here, after fifteen months of virtual futility, was the first serious attempt to look at the economic stabilization program in terms of building effective consent behind it. But even here, the strategic planning was pitifully inadequate.

All this adds up to the fact that the development of a consistent economic policy in the federal government cannot even be approximated without the central formulation of a political program and the mobilization of a successful coalition of power to carry the program through.

I cannot conclude without pointing out that presidential leadership —even with the aid of a policy-oriented Council—is only one facet of this problem. Congress must respond with a more consistent power focus than it has developed up to now. That focus can never be as clear as the President's, for the kaleidoscopic representative function of the Congress interferes—and probably should interfere. But the tolerances for programmatic anarchy should, in my estimation, be narrowed. Insofar as the force of ideas is important here, the Joint Economic Committee even now performs a significant function; although that function could be strengthened if a more sophisticated provision were made for a "legislative clearance" liaison between the members and the staff of the Joint Committee on the one hand and the members and staffs of the standing committees dealing with major economic legislation on the other. It is possible that a joint economic-budget committee, as suggested by Mr. Colm, might help in the development of more consistent economic policies; although, if the Congress went that far, I think that I should prefer a joint committee on the state of the

union which might operate each year for not longer than three weeks and whose sole job it would be to hear testimony on the interrelationships of the three presidential messages.

But again, the real key here is not minor institutional changes to help spread the cold light of reason. The Spanish writer, Unanumo, thinking of Goethe's dying words, "Light, light, more light," declared passionately, "No, warmth, warmth, more warmth; for we die of cold not of darkness. It is not the night that kills but the frost."

The great job of the Congress and of our political parties is to build responsible communities of interest around the major value goals of our society—not to reflect but to refract and moralize the divisive pressures in our democracy. Whoever, through personal leadership or through refining the administrative and political instruments of government, is able to develop more cohesive and responsible power behind more coherent and intelligently constructed programs will have solved the key political elements in full employment policy.

DISCUSSION

SOLOMON BARKIN: Both papers reinforce the demand that the Council of Economic Advisers abandon its aloofness and assume direct responsibility for making economic forecasts for the short run and evaluations of the adequacies of our short-run and long-run economic performance in terms of the primary objectives of the Employment Act of 1946. The Council was created to help realize a high-employment economy. Its members must help the Chief Executive formulate policies to attain this goal. Despite the determination of both the Council and the President of the United States to avoid the test implicit in the Act, the very exigencies of our political life compelled their acceptance during the last few weeks of the political campaign. They abandoned comparisons between current and past performance and admitted that the real measure of our economy is its ability to realize the full potential of growth and utilization of man and resources. This turn gave many of us confidence that our national government will more conscientiously seek both economic stabilization and growth.

Economists, both in and out of government, must undertake to make forecasts of future developments, analyze the implications of current economic institutions, and elaborate on the effects of alternative policies or plans. Their occupation involves the organization of past economic events into orderly patterns which suggest a sequence of occurrences, a time relationship, and a series of effects. Seeking to understand the past and the present, they cannot reject the responsibility for forecasting and projecting the future, for they will then be denying the utility of their knowledge and the continuum of human affairs within the area of their competence. Neither of these positions can be stoutly defended by economists serving active economic or public agencies. Their responsibility is to provide their principals with the insight and knowledge, no matter how carefully they define their assumptions and express the limitations of their own efforts.

The economist must concern himself with short-run, intermediate, and long-run forecasts. The objectives are stability and growth at all times and throughout an entire period. To minimize the short run is to overlook the political implications of temporary periods of distress or the disaster which may befall us in the long run from short-run reverses. Professor Turner properly adds his voice to the many who have cautioned against mechanical techniques for forecasting. However scholarly the source or the auspices, one must be suspicious of the results. They have proven treacherous at critical points. The techniques overlook the familiar observation that however similar the future is to the past, it is necessarily different. Structural and institutional changes are constantly occurring to alter performances. Governmental and political factors make obsolete older relationships. Laws are passed and institutions are created to change the old, so that new conditions will be born. The past, therefore, is a defective guide for the future.

The lag-and-lead technique for economic forecasting is built upon such past performance. It may be helpful in signaling short-run economic turns,

but it cannot be used to predict the length or intensity of cycles. In the hands of the astute student of economic developments it is useful for analyzing economic events. The trade-union economist must include them in his battery for study, because he must be able to preceive the downturn long before it is evident to the community at large. His responsibility is to alert trade-union leaders and the government to the need for corrective action.

The defects of the mathematical projection are made evident by a study of the shortcomings of a commonly used procedure for forecasting economic trends in the textile industry. It is assumed by some textile economists that the two-year cycle of textile activity is still characteristic. But in fact, significant changes in inventory practices of apparel manufacturers and retailers and the lively competition among apparel items resulting from the revolutionary changes in our clothing habits have reduced the influence of this inventory factor. In this era of innovation, past relationships are being strikingly modified. Management itself is more keen about and anxious to introduce new production methods and final products. Suburban communities and the new patterns of living flowing from full employment and higher living standards are changing the propensities for consumption of different types of goods. Mechanical projections of prewar or early postwar experiences are likely to lead to deceptive results.

The increased use of the projection technique is to be applauded, but it should not be described as a forecasting tool. It is primarily an analytical procedure for examining the implications of current institutions, usually for some point in the future, and for spelling out the probable effects of specific policies. It is most useful for studying the steps necessary to realize such specific goals as are spelt out in the Employment Act of 1946 and to examine the practicability and feasibility of individual programs. The statement that our defense program had to be reduced because our economy could not carry the load was proved unsound by this type of analysis. A full employment economy may be realized by a number of courses, such as high labor income, high capital expenditure, or a large governmental program. Which course should we adopt and what are the practical challenges and institutional changes necessary for the realization of each of these alternatives? Light on the analysis of these questions is shed by the projection technique.

We are abundantly provided with social goals and national values. In addition to the objectives spelt out by the Employment Act of 1946, we have inherited many from our recent history. The thirties left us with an inheritance of widespread sensitivity to our national well-being. Adequate national and allied defense is a continuing goal. In addition, groups are agitating for specific targets, such as better schools, medical facilities, roads, and other vital services. The trade-union movement is considering the possibility of a shorter work week. All these must be examined in the light of their effect on our economy. The model technique is particularly well suited to such investigations.

Many employing the forecasting technique have specific short-run and immediate business or local interests. They are concerned, not only with the performance of the American economy as a whole, the shortcomings of its

over-all attainments, and the need for general stabilization programs, but also with the impact on specific industries, areas and population groups. Unfortunately, the men making long-run projections of economic events have made few sallies upon the task of spelling out the specific effects of national economic growth upon the component industries or services. Studies on the effects on specific geographical regions have also been limited. As a result, the projection technique is unable to provide us as yet with the material for the full attainment of stabilization and growth.

We are currently witnessing the secular contraction of such large industries as coal, leather, and textiles, which have suffered in the midst of high national economic activity. The preoccupation with global figures has minimized specific concern for these groups. Even when the Council of Economic Advisers and the Joint Committee on the Economic Report were moved to interest themselves in these problems, they were distant and half-hearted. Yet, can one consider our stabilization authorities to have fully discharged their responsibilities unless they have similarly concerned themselves with the well-being of the individual sectors? They have committed the error of forecasting "by assumed momentum," which has proved false and misleading in connection with all three areas. We have not experienced any long-term recovery in any of them. Moreover, the decline of these industries has created serious economic problems for the textile areas of the eastern seacoast, the mining areas of the middle states, and the leather centers. Global studies must be joined with investigations of specific sore spots if stabilization is to be fully realized.

HOWARD R. BOWEN: At the outset I should like to pay my respects to our speakers for introducing into the lexicon of economics two useful new terms. I refer to Mr. Bailey's "serendipity" and to Mr. Turner's "forecasting by assumed momentum." Serendipity, I take it, is a twentieth-century version of the "invisible hand," and "forecasting by assumed momentum" suggests the wisdom of Ecclesiastes: "The things that hath been, it is that which shall be; and that which is done is that which shall be done: and there is no new thing under the sun."

I am sure that our speakers are well aware that the positions they have taken are highly controversial. Perhaps they planned it that way. At least, each paper has aroused in me a lively spirit of dissent. First, let me take up Mr. Bailey's paper.

His general position, as I understand it, is that America should have a comprehensive national economic policy that is consistent with our basic values; that this policy should be formulated in the Executive Office of the President; and that this policy should be put into effect by means of a political program under the leadership of the President. With this much I am in agreement. But then, Mr. Bailey implies, the Council of Economic Advisers is to be a principal instrument by which all of this is to be done. Specifically, he implies that the Council should be not only a formulator of economic policy but also an active agent in seeking adoption of that policy by Congress. Here I find myself in disagreement. I think that the Council—at least if it is to consist

of economists—cannot, and should not, assume such responsibility. Professional economists are not suited to perform an essentially political task. If they were asked to perform it, they would be prevented from accomplishing the job for which they are equipped; namely, to give objective advice on the current state of the economy and on measures which might promote economic stabilization.

In my view, the Council should be a group of highly competent professional economists whose job it is to sift and place at the disposal of the President, in a comprehensible form, all the relevant accumulated knowledge, theoretical and empirical, of the entire economic profession. This is a tremendous task. If done well, it must be done by men who are primarily scholars rather than practical politicians. And if done well, it is a full-time job which leaves no time for political activity.

I do not mean to say that political work is unnecessary. Mr. Bailey has performed a great service in emphasizing that the job of getting economic policy adopted does not end with economic advice. Yet I doubt that the persons who are competent to give economic advice will also be effective as political operators. And I think that asking them to do both jobs will prevent them from doing either.

Obviously, as good economists, the members of the Council should not ignore political realities in formulating their advice. Also, I can agree with Mr. Bailey that the President should have the right to appoint and remove the members of the Council at his pleasure. He should have advisers with whom he can work and who are sympathetic toward his aims. But the Council should be primarily concerned with offering sound economic advice rather than with political expediency or political action.

Doubtless our government needs stronger political leadership; but it also needs more honest, disinterested advice on what is the right thing to do in order to achieve given ends. In the field of disinterested advice, professional economists have something to offer; in the field of political leadership, they have precious little to offer. I think economists should stick to their lasts.

Mr. Turner's position, as I understand it, is that stabilization policy requires forecasting; that within recent years we have learned to forecast with considerable reliability; and that we should confidently use our newly acquired skill in the formulation of stabilization policy.

I agree that considerable theoretical and statistical progress has been made in recent decades. I also agree that governmental economic policy has been fairly appropriate within recent years—though I suspect the explanation lies largely in serendipity and in forecasting by assumed momentum.

Regarding our ability to forecast the future reliably, I remain a skeptic. I have been profoundly impressed by the consistent differences of opinion among competent economists as we have headed into each minor recession since World War II. In each case, some have warned of inflation, some have expected a minor recession, some have envisioned a major depression, and some have looked for no change. In this guessing game, some have been right for the wrong reasons, some have been wrong for the right reasons, some have been wrong for the wrong reasons, and doubtless some have been right for

the right reasons. But have those who have been right for the right reasons been consistently right? And, if so, has this been a matter of chance or luck? Even if there are some conspicuously successful forecasters, how does a government identify them and see to it that they are in positions of influence?

Perhaps we need a statistical study of forecasters and their forecasts—with their annual batting averages published in the financial pages—in order to identify those persons who ought to be the official forecasters for stabilization policy.

It seems to me that we are not yet ready to speak with much assurance about future economic conditions. We still have not taken the really crucial test; namely, prediction of a major downturn. If and when such a catastrophe comes I have no doubt there will be those who will have foreseen it. But how are we to know which forecaster to listen to before the fact?

As you see, I am inclined to be a bit more pessimistic than Mr. Turner about our ability to forecast. On the other hand, I am more optimistic than he about the efficacy of built-in flexibility and the usefulness of compensatory action after the fact. Once we are in a depression or inflation, the fact can be recognized. At that time, there is some justification in forecasting by assumed momentum. At that time, moreover, political obstacles to fiscal policy and other stabilization measures are likely to be weaker than they were before the fact. Hence I conclude that in our present state of knowledge we had best continue to rely on compensatory policies rather than on policies based on specific forecasts which will at best have uncertain reliability.

RICHARD A. LESTER: Professor Turner asks that the Council of Economic Advisers face up to its statutory duty to forecast economic magnitudes. The fact is that for the past year members of the Council have made a number of predictions of one sort or another. For instance, last June at a press conference the chairman made a forecast with respect to capital investment, after the finger had been put on him at a presidential press conference. Professor Turner's plea, I take it, is for more systematic and well-documented forecasts and fewer economic forecasts via speeches and press conferences.

Professor Bailey warns that the Council members are participants in national policy making and should face up to the implications of their political positions. His admonition raises the question whether Council forecasts are not bound to contain political bias. At various times since 1946, Council members' predictions appear to have been designed to influence Congressional action or private spending, and it may well be that the function of policy formation and advocacy and the function of forecasting economic conditions should not be merged in a single agency.

At the meetings of this Association five years ago, Professor Erik Lindahl explained that in Sweden a group of economic experts in the government performs the technical task of calculating economic developments under alternative assumptions with respect to governmental policy and programs. The choice of the policy and programs is a separate function left to the political authorities. In the performance of its technical task of forecasting results under varying assumptions, the Economic Research Institute, headed

by Professor Erik Lundberg, has enjoyed a relatively independent position while housed in the Finance Department, and it apparently has avoided a political bias in favor of the government in connection with its forecasting activities.

In my opinion, serious consideration should be given to the establishment of some such semi-independent group of experts in the use of forecasting techniques in Washington. I have little faith in mechanistic methods of forecasting, and I am sure that the forecasts of any expert group would frequently be wrong, but a continuing agency would learn from experience and from the professional criticism of others. Such a group might be located, to use Professor Bailey's words, "in a dark corner of the Budget Bureau." Unlike the members of the Council of Economic Advisers, they should not change with each change of administration; continuing tenure would give them protection from—should I say "undue"—political influence and also the advantage of accumulating experience.

In looking back at the year 1954, I am surprised to find how close some private business forecasts came to predicting developments in, say, the level of the Federal Reserve index of industrial production or unemployment levels. Their January, 1954, forecasts contrasted with the Council's optimism for the immediate future. Perhaps there is a lesson to be learned in this difference of predictions.

Personally, I would place more stress on forecasts for the intermediate period than has been the practice in Washington. By intermediate I mean one to three years hence—specifically now that would be January, 1956, to January, 1959. Over the twelve months immediately following the Council's report, heavy reliance must be placed on built-in stabilizers and on discretionary action by the Executive Branch that requires no Congressional action that year, such as monetary policy. (Speaking of built-in stabilizers, the Council's recommendations last January with respect to state unemployment benefits were rather naïve and fruitless, and its discussion of minimum wages harked back almost to Nassau Senior. Lack of state or federal action to correct benefit levels under unemployment compensation that have been flattened down by increasingly low ceilings has given a great push to the guaranteed annual wage in such industries as automobiles.)

Each year the Congress should consider and take action to cover the forthcoming intermediate period. Admittedly, changes in the international picture make such planning and programing difficult and subject to alteration from time to time. It is, however, hazardous to neglect the intermediate years if the private groups who forecast so well for 1954 come close to being correct in their current forecasts of industrial production and employment for the years 1956 and 1957.

This country cannot afford to waste resources through idleness to the tune of 30 billion dollars of gross national product a year or three to five million unemployed. We need too badly such items as schools, roads, housing, and defense, not to mention the effects of unnecessary unemployment upon internal morale and our international prestige. Council Chairman Burns pointed out last October here in Detroit that "we are living in an age of

revolution" in which our economic system is in a world struggle for survival with communism and that we must, therefore, "be ready to take vigorous steps to help maintain a stable prosperity."

Forecasting and programing for the intermediate period should help to bring down to earth the repeated promises or threats of "bold steps," by focusing attention on a period longer than month to month or quarter to quarter. Of course, I am not unmindful of the fact that federal administrations have a four-year term and may find it difficult to plan beyond the next presidential election. Planning for the intermediate period is, however, one bold step that should be tried. It might very well help to increase the economic light and reduce the political heat.

CYCLICAL EXPERIENCE IN THE POSTWAR PERIOD

THEORETICAL SIGNIFICANCE OF THE 1949 RECESSION

By RENDIGS FELS
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There have been a number of good discussions published on the recession[1] of 1949, among others by one of the discussants at this session. The chief reason for bringing up the subject again is to use the episode to throw light on business cycle theory.

I shall be especially concerned with why some cyclical downswings seem to cumulate, resulting in severe depressions, whereas others come to an end quickly. Before I say anything more about cumulative contractions I had better define the term. By cumulative contraction I mean a business cycle downswing in which aggregate money demand for goods and services (or gross national product in current dollars), real consumption, and real private domestic investment all decline in at least two successive quarters (seasonally adjusted), the decline in the second quarter being greater than in the first. The idea of a cumulative contraction, which gathers momentum like a snowball rolling downhill, is familiar in the literature. But how do we distinguish cumulative contractions from any business cycle downswing? The mechanism of the cumulative contraction has two aspects: one monetary and one real. A reduction in output in current dollars ordinarily reduces disposable incomes (in current dollars), which reduces investment, and so on. Similarly, a reduction of real output reduces real investment, etc. It is possible for a contraction in money terms to occur without a contraction on the real side and vice versa, but in the actual world one kind of contraction cannot go on for long unless accompanied by the other. Consequently, the first step in ascertaining why a given business cycle downswing was mild is to find out whether both kinds of contraction occurred. For example, during the downswing which the National Bureau of Economic Research recorded as occurring in 1945, real consumption rose; therefore, there was no cumulative contraction. In cases where the downswing involves both the money and the real aspects of the mechanism, the next step is to inquire whether the downswing cumulated; i.e., gathered momentum. Consequently, the definition of cumulative contraction includes the proviso that the decline must ex-

[1] I define a recession as a business cycle downswing in which there is no cumulative contraction. I define a business cycle downswing as the period between a peak and the succeeding trough as determined by the National Bureau of Economic Research (see Arthur F. Burns and Wesley C. Mitchell, *Measuring Business Cycles* [1946], p. 78).

tend over two quarters and must be greater in the second than in the first.

The question why some but not all downswings result in severe depressions is important for theory. In some theories, such as that of Colin Clark and, to a lesser extent, John R. Hicks, the economy is inherently unstable, so that any downswing will lead to a severe depression unless some exogenous force intervenes. In those theories which utilize the distinction between major and minor cycles, on the other hand, downswings are mild if long-run investment opportunities are favorable and severe if long-run investment opportunities are unfavorable. Professor Hamberg and I both adhere to the latter view, and I am inquiring whether the 1949 experience proves us right.

The question why some downswings are severe, others mild, is not only interesting theoretically but also important practically. Formerly, when economists believed that full employment was the normal state of affairs, it was appropriate to focus on the upper turning point; that is, on the question why the economy should ever lapse from normal. We now know that there are a number of reasons why a downswing may get started. In the present economic and political climate of the United States, we can expect downswings to get started from time to time, as they have in the past. The problem of maintaining stability will be to keep the downswings minor. To do this we need to know a good deal more about what determines whether the downswing becomes a recession or a depression.

The downswing of 1949 is commonly referred to as an inventory recession. The reason for this is simple: the decline in inventory investment was actually greater than the decline in gross national product. I have no quarterly figures for GNP in real terms, but its decline was probably divided about evenly between output and prices. As usual, the fall in output was concentrated in manufacturing and the fall in prices in agriculture. Total civilian employment fell little more than 1 per cent between the fourth quarters of 1948 and 1949. Unemployment rose more than employment fell because the labor force was growing, but the decline of employment is a better measure of the force of a cyclical downswing. The contraction was evidently extraordinarily mild. It was also unusually short. The National Bureau of Economic Research dates the peak in November, 1948, and the trough in October, 1949, giving the contraction a duration of eleven months. Out of twenty-three contractions since 1857, only four have been shorter.

Few illustrations of the traditional theory of cyclical contraction can be found in the recession of 1949. It is true that in particular markets there was an "inventory-deflation spiral . . ., with reduced inventory investment driving prices down and falling prices inducing

smaller inventory investment" (Elmer C. Bratt, *Business Cycles and Forecasting* [4th ed.; Richard D. Irwin, Inc., 1953], page 346), and with the cuts in output decreasing the need for inventories and leading to still further cuts in inventory investment and output. It is true also that the decline in inventory investment was great enough to induce declines in a number of aggregate measures of business activity—not only GNP, employment, and industrial production, but also disposable income and investment in producers' durable equipment. These declines undoubtedly contributed in familiar ways to prolonging the downswing.

But one important aggregate was scarcely affected at all. Personal consumption expenditures in current dollars fell off in only one quarter, the first quarter of 1949. (Consumption of nondurables also fell in the second and third quarters, but this was more than offset by rises in durables and services.) If I had quarterly statistics of real consumption, they would probably show no decline in any quarter. Since a decline of real consumption is an integral part of a cumulative contraction, it is clear that no cumulative contraction (as I have defined the term) got under way. The fact that real consumption was maintained makes it easy to understand why the recession of 1949 did not degenerate into a depression. But this is only a first step in the analysis.

We must next inquire why consumption held up so well. To some extent the result can be credited to the combined effects of built-in flexibility, delayed effects of the tax reduction of the spring of 1948, and increased government expenditures; but if we take the statistics at face value (a hazardous procedure), we conclude that this was not the major factor. More important was the rise in the consumption function (see Table 1).[2] The reduction in disposable income was more than offset by a decrease in personal saving.

TABLE 1

GNP ET HOC GENUS OMNE

(Billions of current dollars, quarterly rates, not adjusted for seasonal variation)

	GNP	Personal Income	Disposable Income	Consumption	Saving
Fourth quarter, 1948	70.9	54.4	50.7	49.0	1.7
Fourth quarter, 1949	67.7	52.7	49.0	49.7	−0.7
Change	−3.2	−1.7	−1.7	+0.7	−2.4
Percentage change	−4.5%	−3.1%	−3.4%	+1.4%	

SOURCE: *Survey of Current Business.*

[2] Let us assume that normal marginal propensity to consume out of disposable income is one-half (the exact MPC assumed makes little difference to the comparisons that follow). With the decline in disposable income shown in Table 1, we would expect that consumption would have declined by 0.85 billion dollars. It actually rose 0.7 billions. This implies that

There are three explanations for the rise in the consumption function.[3] The best-substantiated is increased availability of automobiles (except for the fourth quarter, when model changing slowed output). This was one industry which had not caught up with demand in 1948. Automobile purchases in the next year increased by 2 billion dollars. To some extent, the fact that at the time of the recession an important industry was still expanding to meet demand deferred from the war must be regarded as an accident, but to a considerable extent this was a probable result of the forces which produced the inflationary expansion of 1946-48. Those forces made a bunching of the catching-up stages for different industries likely, but one could hardly have expected that all important industries would have reached that stage simultaneously. Here was an internal or endogenous mechanism which tended both to produce a business contraction and to ensure that the contraction would not make much headway.

The second explanation for the rise in the consumption function is well established, but its effects are difficult to measure. Consumer credit controls, which had been reimposed on September 20, 1948, were relaxed on March 7 and April 27, 1949, and finally allowed to lapse altogether on June 30. Statistics on consumer credit could readily be arranged to support the contention that the impact on consumption was considerable, possibly amounting to an annual rate of several billions of dollars. But the behavior of these statistics can be explained, at least in part, on other grounds such as increased availability of automobiles or the return to prewar seasonal patterns. Abolition of consumer credit controls was, of course, an external or exogenous factor.

A third explanation for the rise in the consumption function is plausible, but it seems impossible to get strong evidence for it. Slichter

consumption was 1.55 billion dollars higher in the fourth quarter of 1949 than it would have been had the consumption function not risen. If there had been no built-in flexibility and no change in tax rates, disposable income might have fallen by the full amount that GNP fell, namely, 3.2 billion dollars; i.e., 1.5 billions more than it actually fell. If we still assume that the marginal propensity to consume is one-half, consumption was 0.75 billion dollars higher in the fourth quarter of 1949 than it would have been without built-in flexibility and the tax cut. Therefore, under these assumptions the rise in the consumption function appears about twice as important as built-in flexibility plus tax cut. This no doubt puts an undue burden on the statistics, but there seems to be no question that the consumption function rose. In Table 1 I have taken advantage of the fact that the peak and trough both came in the fourth quarter of the year (ignoring the fact that the third quarter of 1949 was about as low as the fourth); this made it appropriate to use quarterly data not adjusted for seasonal variation, since the unadjusted figures presumably are more reliable. If I had followed the usual practice and used seasonally adjusted annual rates, the rise in the consumption function would have appeared nearly three times as important as built-in flexibility plus tax cuts; this would have been true irrespective of whether the third or the fourth quarter of 1949 was taken to be the trough.

[3] In addition, it should be mentioned that liquid assets were still fairly high and consumer debts fairly low; but these are reasons why the consumption function could rise, not why it did rise. The lag in adjusting to reduced incomes also deserves mention, although it could not explain why consumption in current dollars rose.

asserted that consumers in the fall of 1948 began postponing expenditures because they anticipated falling prices. Since consumers' stocks of goods were low and their bank deposits still large, he predicted that postponement of consumption would not last long. Since Slichter is a careful observer of contemporary business conditions, we can regard his assertion as a primary rather than a secondary source; and since he used it to make a successful prediction, we have a more convincing reason for believing his hypothesis than the mere weight of his authority. But there are nevertheless reasons for skepticism. For one thing, Slichter on a later occasion told a different story. For another, a single prediction may be right for the wrong reason. We therefore want confirming evidence. The "1949 Survey of Consumer Finances" provides some confirmation, though it is none too strong. If Slichter was right, here was another endogenous mechanism tending both to bring about a downturn once inflation died out and to limit the extent of the downswing.

The rise in the consumption function was not the only important reason why the recession was mild. As Gordon and Hamberg have stressed, autonomous investment was high; or to put the same matter in other words, long-run investment opportunities were favorable. Investment was still directed in large measure at shortages inherited from the war and therefore was not much affected by minor fluctuations in business activity. This is why investment in both construction and in producers' durable equipment fell so little. It should be noticed, however, that an endogenous factor was at work on the construction of houses. A decline in costs, as building materials fell in price and premium wages disappeared, bolstered construction because builders were able to shift into production for a lower priced market. In addition, an external factor may have affected private investment; namely, easier credit conditions following reduction in the reserve ratios required of member banks. But the main point undoubtedly was the autonomous character of private investment other than most inventory investment.

While we are on the subject of autonomous investment, we must discuss the most important external factor of the 1949 recession: the steady growth in government purchases of goods and services throughout 1947, 1948, and the first half of 1949. The increase in the first quarter of 1949 amounted to nearly three billion dollars and was caused primarily by higher military expenditures and increased foreign aid. Inasmuch as GNP declined only ten billions during the recession, seven billions of it in the first quarter of 1949 (annual rate), it can be seen that this was of great importance in keeping the contraction from cumulating. The level of government purchases reached in the first quarter was maintained throughout the year, a decline in the federal

component in the last half of the year being offset by rising state and local expenditures.

The foregoing analysis is still not adequate to explain why the recession was so mild. In the case of the downswing of 1920-21, there is evidence of strong autonomous private domestic investment and a rise in the consumption function; yet the earlier downswing quickly degenerated into a violent deflation. The chief reason for the difference appears to be speculation in inventories in 1919—a factor that was almost totally absent in the inflation of 1946-48. The history of depressions suggests that a speculative boom is a predisposing factor for a severe downswing. But businessmen were cautious during the upswing following World War II. The reason for their caution lay in their memories of the depression of the thirties, knowledge of what happened in 1919-21, and the highly publicized predictions (themselves partly the result of the depression of the thirties) that World War II would be followed by a depression.

Another difference between the downswings of 1921 and 1949 lay in the structure of the economy, which led to a different state of expectations. The inflation during and after World War I created the expectation that prices would have to fall; and whether that expectation was rational or not, in fact there were no serious obstacles to keep them from tumbling down. Expectations that prices would fall meant that when they began to fall they soon were falling precipitously. By the end of World War II, strong impediments to falling prices had grown up: powerful labor unions, price supports for agricultural products, and (it seems to me) an industrial shift towards maintaining prices in the face of falling demand. In addition, there were indirect assurances against falling prices: unemployment insurance and a government policy of actively combating depressions. These well-known developments gave businessmen and consumers the expectation that prices would not in any case fall very far, and the expectation in turn was a factor in keeping their decline moderate. There have been many theoretical contributions to the question of whether price inflexibility helps or hinders in maintaining full employment. The contrast between 1921 and 1949 suggests that in the latter case inflexibility helped avert what otherwise might have been a serious cyclical contraction.

Several years ago I published the statement that price flexibility in 1920-21 "appears to have shortened the depression by increasing aggregate spending." ("The Effects of Price and Wage Flexibility on Cyclical Contraction," *Quarterly Journal of Economics*, November, 1950, page 608.) There is no contradiction between this and the statement just made. On account of the speculative boom of 1919-20, there

would have been a cumulative contraction in any event. Price flexibility made the cyclical decline in prices greater but made the decline in output less; hence, price flexibility in that case was beneficial. The lack of price flexibility in 1948-49 helped keep a cumulative contraction from ever starting. The contraction of 1920-21 would have been worse if prices had not been flexible; the contraction of 1949 would have been worse if they had been flexible. This tends to confirm my earlier findings that price flexibility can help or hinder a cyclical contraction depending on the special circumstances of each case.

Such were the factors at work. The process whereby they brought the recession to an end can be described in brief compass. The small decline in output together with the rise in government spending and the maintenance of consumption enabled businesses to do in a short time what little liquidating of inventories they felt desirable. They soon were in a position of having to expand output in order to keep inventories from falling too far. When they did so, expansion got under way.

Now let me summarize the reasons for the mildness of the recession of 1949 preparatory to discussing what it does—and also what it does not—tell us about the nature of downswings in general. First, there were several endogenous or internal mechanisms at work: recent experience with severe depression, which served to prevent a speculative boom during the upswing; shortages inherited from the war,[4] with the high probability that some of them would not be satisfied by the time the recession began; the decline in the cost of building houses; and (possibly) the difficulty consumers had in continuing for long the postponement of expenditures which may have contributed to the upper turning point. Note that none of these endogenous forces can be depended on to operate in all future contractions. Second, there were two changes in the structure of the economy: the great increase in the importance of built-in flexibility and (to demonstrate how awkward the English language is) the increase in price inflexibility. The former can be depended on to help limit future downswings and the latter cannot. Third, there were several favorable external or exogenous forces: the increase in government purchases, the end of consumer credit controls, the cut in taxes, and the lowering of required reserve ratios of member banks. All these forces combined to prevent a cumulative contraction from getting under way. Which factors are classed as exogenous depends on the purpose at hand. The factors resulting in high, sustained, autonomous investment could appropriately be called exogenous; but I omit them. Since the question I am investigating is

[4] The war, itself, was exogenous, but the reaction of the economy to the shortages it created may properly be regarded as endogenous.

whether in a period of favorable private long-run investment opportunities a cumulative contraction can get under way and lead to heavy unemployment, it is irrelevant to inquire whether the reasons for the favorable opportunities are endogenous or exogenous.

I put the matter this way in order to stress that the recession of 1949 throws no light on the question I posed at the beginning: do we or do we not live in such an unstable economy that any downswing is potentially disastrous? External events, as well as some internal ones which cannot always be depended upon, intervened to prevent a cumulative downswing. If one had started, we just do not know what the result would have been, however tempting it may be to form a judgment and defend it as eternal truth. Colin Clark may or may not have been right when he wrote that "any fall . . . which has once reached a certain velocity will accelerate itself, and nothing is likely to stop the fall until investment has been reduced to the minimum replacement level. . . ." ("A System of Equations Explaining the United States Trade Cycle, 1921 to 1941," *Econometrica*, April, 1949, page 115.) I believe that Professor Hamberg went too far when he wrote "that an economy can withstand the impact of such inventory recessions as those experienced by the U. S. during the twenties and again in 1948-49, without tumbling into a major depression would appear as impressive testimony to the *stability* of the economy." (*Metroeconomica*, 1954, page 63—Hamberg's italics. This statement should be interpreted in conjunction with his discussion of 1948-49 in the *Economic Journal*, 1952, page 14, where he felt that the 1949 recession "bode [d] ill for the future.")

My conclusion disappoints me. Like Professor Hamberg, I want to use minor recessions such as 1949 to prove that our economy is not nearly as unstable as Hicks and Clark believe. But as long as this is an open question, economists ought to give different advice to the President and Congress when a business contraction starts than if we were sure that with investment opportunities favorable contractions come to a quick end by themselves. The temptation is great to conclude, consciously or unconsciously, that because the 1949 recession came to an end quickly future contractions will do the same.

I must, however, defend my view against an econometrician who tried to use this recession to throw doubt on Hicks's hypothesis of extreme instability. He started with a three-equation model consisting of the multiplier, the accelerator, and the national income identity, and fitted American statistics for 1947-50. He found a value for the accelerator far below what is required for explosive behavior and concluded that Hicks's hypothesis of an explosive accelerator appeared to be contradicted by the facts of this case. (Gene H. Fisher, "*A Simple*

Econometric Model for the United States, 1947-50," *Review of Economics and Statistics*, February, 1952, pages 46-48. He found an accelerator of 0.482, whereas to be explosive the value would have to be greater than 1.0.) But about two and a half of the four years he studied represented full employment. Under Hicks's theory, full employment may (and in this case undoubtedly did) reduce investment below the amount firms desired. For these years it is easier to reconcile a low value of the accelerator with Hicks's theory than it would be an explosive one.

It is not too difficult to reconcile Hicks's theory with the facts of 1948-49. His theory includes allowance for humps and slumps in autonomous investment. From the point of view of his theory, the 1946-50 cycle was a Bactrian camel. As you doubtless know, a Bactrian camel has two humps separated by a small slump.

In saying this, I do not mean to defend Hicks's theory. As Schumpeter once said, when we hang a man who deserves to be hanged, it is nevertheless important that we do not hang him for the wrong reason. (I must hasten to say that I do not believe Hicks's theory should be liquidated, much less that eminent economist's person; I am discussing only the application of his theory to one particular cycle.) The proper verdict to render on Hicks's theory in connection with 1948-49 is that it is not very helpful. To paraphrase Schumpeter again, autonomous investment is only a label for a blank space, and if we fill this blank space by some such thing as humps and slumps, we are filling a blank with another blank.

All of this is negative. Let me wind up with two positive suggestions for theory. Part of the rise in the consumption function was due to external events but part—perhaps the larger part—was not. This played a crucial role in heading off a cumulative process. It would be trite to say that the business cycle theory of the future must explain changes in the consumption function. The 1949 experience suggests that rather sudden shifts in the consumption function not only are significant for explaining business cycles but also may themselves have a cyclical origin.

Lastly, the speculative boom, which played a prominent part in some older theories but in recent theories is usually omitted, must be retained and explained. The presence of such a boom in 1919 and its absence in 1946-47 is one of the obvious differences between two cycles so similar in origin and so different in result. Had there been a speculative boom to collapse in 1948, it would have greatly changed subsequent events. The kind of explanation of the occurrence of speculative booms suggested here is old and familiar, running in terms of the recent experience with business cycles of business managers and investors.

BUSINESS FLUCTUATIONS IN THE UNITED STATES SINCE 1951: SELECTED DEVELOPMENTS

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Three fairly distinct economic movements have occurred in the American economy since 1951. First, the sharp rise in gross national product which followed the outbreak of the Korean war was interrupted early in 1951 by an inventory crisis although rising national security expenditures still maintained and gradually increased the gross national product during the period preceding the steel strike in mid-1952. Next, a resurgence in both private investment and consumption spending, beginning in the fall of 1952, carried gross national product and industrial production into new high ground in the spring of 1953. Finally, this peak level performance gave way when inventory and national security expenditures were reduced. The declines in industrial production and gross national product have apparently been checked, however, due largely to the expansion in new construction and consumption expenditures, an increased flow of government transfer payments, and reduced federal taxes.

Detailed examination of each of these periods of major change undoubtedly would be desirable and rewarding. However, time considerations severely limit the scope of the present analysis. As a consequence, the objectives of this paper are confined to three: to digest the basic economic developments from 1950 to 1954; to select, if possible, the crucial factors which contributed to the major economic swings of the period; and to advance some tentative observations concerning the near-term prospects in the economy.

Economic Developments, 1950-54

Gross National Product:

From an annual rate[1] of 264.9 billion dollars in the first quarter of 1950 (hereafter, 1950 I), gross national product, in current dollars, rose by 105 billions, or approximately 40 per cent, to a peak of 369.9 billions in 1953 II. In real terms, the increase was at least one-quarter. Almost 45 per cent of the rise, however, was concentrated in three quarterly periods: 18.5 billion dollars from 1950 II to III; 14.3 billions from 1950 IV to 1951 I; and 14.3 billions from 1952 III to IV. The total increase during the six quarters from 1951 I to 1952 III was only 24.9 billion dollars. Thus the behavior of the post-Korea gross national

[1] Data on the components of gross national product refer to annual rates unless otherwise stated.

product was characterized by sharp spurts interspersed by much slower periods of growth.

Following the peak in 1953 II, gross national product began to decline and at its low in 1954 III had fallen by 3.9 per cent, or about the same percentage as in the 1948-49 recession.

Personal Consumption Expenditures. Turning now to major components of the gross national product, the two waves in consumption expenditures following the outbreak of the Korean war are well known. From 189.1 billion dollars in 1950 II, personal consumption rose to 202.9 billions in III, and then declined to 198.8 billions in IV. Following their peak of 210.0 billions in 1951 I, they fell to 204.4 billions in the next quarter largely because of a 4.4 billion decline in expenditures on consumer durable goods. Consumption expenditures gradually rose thereafter to 218.2 billions in 1952 III. They then increased by almost 6 per cent to 231.2 billions in 1953 III as disposable income continued to grow and consumer credit rose sharply by 4.6 billions. More than one-half of this increase in consumption was from 1952 III to IV. Since declining to 229.7 billions in 1953 IV, consumer expenditures have risen moderately.

Gross Private Domestic Investment. The maximum increases in gross private domestic investment were 9.1 billion dollars from 1950 I to II and 9.2 billions from 1950 III to IV. Producer durable equipment and business inventories were responsible for two-thirds of the first rise while business inventories more than accounted for the second. From the peak of 62.6 billions in 1950 IV, gross private domestic investment fell by almost 25 per cent to 47.2 billions in 1952 II. The explanation is, of course, that investment in business inventories declined from an accumulation of 14.7 billions to minus .9 billions. The inventory problem was particularly acute for dealers in consumer durable goods. In the ensuing boom of 1952-53, private investment, led by business inventories, recovered to 55.9 billions in 1953 II and then fell by one-fifth to a low of 44.5 billions in 1954 I. All but 1.8 billions of this decline may be accounted for by the change in inventories which fell from 5.4 billions to minus 4.2 billions. An important offsetting factor has been construction which has reached new peaks in each quarter of 1954.

Federal Government Purchases of Goods and Services. Federal government purchases of goods and services rose steadily through 1953 II, with the exception of a billion dollar decline from 1952 II to III. At their peak of 62.2 billions in 1953 II, federal government expenditures for goods and services were three times as large as in 1950 II and III. National security items increased even more rapidly, rising from 17.2 billion dollars in the quarter preceding the Korean war to 54.3 bil-

lions in 1953 II, although their rate of increase began to decline early in 1952. Through 1954 III, federal government expenditures have declined by 14.3 billions to 47.9 billions, of which all but 2.2 billlons have been in national security expenditures.

State and Local Government Expenditures on Goods and Services. State and local government expenditures on goods and services have increased each quarter since 1950 II except for a decline of one-tenth of a billion dollars from 1951 II to III and one-half billion dollars from 1953 I to II.

Other Economic Series:

The other economic series which will be reviewed here are personal and disposable personal income, industrial production, prices, and interest rates.

Personal and Disposable Personal Income. Special interest attaches to personal income and particularly to disposable personal income because of their close relationship to the flow of expenditures. Personal incomes rose each quarter from 1949 III to 1953 III but declined moderately through 1954 I. Since then personal incomes have risen slightly.

Disposable personal income has shown even greater strength. With the single exception of 1952 I, disposable personal incomes have either remained unchanged or risen every quarter since 1949 III. Two factors bear primary responsibility for the increase in disposable personal income from 1953 III to 1954 III as gross national product was declining: the rise in government transfer payments from 12.6 to 14.7 billion dollars, and most important, the fall in personal tax and nontax payments from 36.3 to 32.9 billions.

Industrial Production. The Federal Reserve Board index of industrial production provides a relatively sensitive measurement of real production developments. From a low of 94 in the 1949 recession, total industrial production had recovered to a new post-World War II peak of 112 in June immediately preceding the Korean war. Subsequently, the index rose by 10 points, or 9 per cent, to 122 in December, 1950. It remained unchanged during the first five months of 1951 and then declined to 118 where it remained from August through October, 1951. The steel strike is reflected in the decline of industrial production from 121 in March, 1952, to 115 in July, 1952. It recovered rapidly thereafter, rising by 22 points, or almost one-fifth, to 137 in both May and July, 1953. Industrial production then fell by just over 10 per cent, to 123, in March and April, 1954, and was still at that level in August.

Behavior of Prices. Consideration must now be given to price changes. From a low of 100.4 in February, 1950, the consumer price

index rose by 10 per cent, to 110.3, in March, 1951. During the next two and a half years, consumers' prices increased by less than 5 per cent to their peak of 115.4 in October, 1953, and have receded less than 1 per cent since then.

The index of wholesale prices had reached 100.2 in June, 1950. Eight months later it had risen to 116.5. This proved to be the peak as wholesale prices, led by farm prices, fell to 109.4 in April, 1953, and remain at approximately this level today.

Interest Rates. Finally the movements of interest rates reflect the changing demand for and supply of funds and the dear and cheap money policies of the monetary authority. Yields on long-term US Treasury bonds provide a sensitive index of changing monetary policies. From a low of 2.19 per cent in December, 1949, yields rose gradually to 2.39 per cent a year later. Following the accord in March, 1951, yields rose to 2.65 per cent in June, 1951, and finally reached a peak of 2.74 per cent in January, 1952. They fell to 2.57 per cent in May, 1952, and rose gradually during the remainder of the year. The tighter money policy of early 1953 is clearly recorded in the rise of yields by over one-third of a per cent: from 2.75 per cent in December, 1952, to 3.09 per cent in May and June of 1953. Since that time yields have fallen again to a low of 2.47 per cent in April, 1954, and recently have firmed slightly.

Summary:

Summing up the general features of the period, the economy has been relatively stable as compared with pre-World War II cycles. The major fluctuations in gross national product have resulted primarily from alterations in rates of inventory, consumption, and national security expenditures. Producer durable equipment expenditures have been somewhat more stable than in many earlier fluctuations. Construction was also highly stable from the Korean war through 1952, ranging from 22.7 to 24.2 billion dollars. In 1953, however, it averaged 25.5 billions and has risen to a new peak in each quarter of 1954. Of unusual interest is the fact that from 1951 to the middle of 1953, whenever a major component of expenditure began to falter, some offsetting strength appeared. The relative weakness in consumption in 1951 led to an inventory crisis, particularly in durable consumer goods, which nevertheless failed to reduce the gross product because of the rising level of national security expenditures. Then as the rate of expansion in the latter tapered off, private spending revived partly to replenish stocks following the steel strike of 1952 but also facilitated by the expansion in consumer loans. Monetary policy was tightened early in 1953. Shortly thereafter, as consumption weakened again, national

security expenditures also began their decline and inventories started to accumulate. The heavy disinvestment in inventories and declines in national security expenditures were offset, but only in part, by increases in construction, consumption, and government transfer payments, and by tax reductions. As a consequence, the 1953-54 recession was mild, more comparable to the decline of 1927 than other pre-World War II recessions and approximately as severe as that of 1948-49, depending upon the statistical comparisons drawn.

Causal Factors

This is the empirical record. The areas of expansion and contraction have seldom been so clearly evident. As will appear below, however, despite the clarity of the statistical record, caution would still seem to be justified in interpreting the causal relationships, particularly in the latter part of the period.

From 1951 to the Steel Strike of Mid-1952. The inflation of the post-Korean boom was stopped in 1951 when the consumption function shifted downward. It is likewise probable that inventory expenditures declined also both because consumption weakened and because the cessation of the price rises ended the opportunity for further speculative inventory profits. A variety of factors weakened the forces of demand. For one thing, the extremely high level of expenditures had built up consumer stocks which appeared adequate for the time being once it became apparent that the flow of production of consumer goods would not be contracted excessively. Second, data from the Survey Research Center of the University of Michigan indicate that in its June, 1951, surveys more consumers thought it was a poor time to buy because of high prices than in subsequent surveys of 1951 and 1952. Third, merchants had overordered during the period of rapidly rising prices and hastened to restore more normal inventory-sales positions. In the fourth place, the harder money policies suggested that inflation was to be curbed. And, finally, when price ceilings were established consumers were encouraged to believe that the inflation was over.

In spite of the inventory crisis, gross national product continued to rise largely because of the further expansion in federal national security expenditures from 27.4 billions in 1951 I to 50.0 billions in 1952 II, an increase of 22.6 billions while gross national product went up 22.1 billions. Increases in personal consumption, net foreign investment, state and local government expenditures, and other federal expenditures approximately compensated for the drop in gross private domestic investment, leaving the increase in national security expenditures as the major factor in the rise in gross national product.

The Boom of 1952-53: From the Steel Strike Through the Second

Quarter of 1953. During the 1952-53 boom, gross national product rose by 28.5 billion dollars, or 8.3 per cent, from 1952 II to 1953 II. Personal consumption expenditures which rose by 14.1 billions and gross private domestic investment which rose by 8.7 billions (2.2 billions in construction and 6.3 billions in business inventories), together accounted for 80 per cent of the increase in gross national product. The dominant factors in this boom clearly were consumption and gross private investment. The expansion of 4.6 billions in consumer credit and the move to replenish depleted stocks supply the primary explanation for the strength in consumption and gross private investment.

The Recession of 1953-54: From Mid-1953 Through the Third Quarter of 1954. The turning point in 1953, as well as the behavior of the 1953-54 recession, is easily described in terms of the fluctuations in the components of gross national product but the causal relationships are more perplexing. By far the most important changes have been in the rates of government purchases and inventory investment. Government purchases declined by 11.0 billions. National security expenditures declined by 12.2 billions and other expenditures by 2.4 billions but were partially offset by a rise of 3.3 billions in state and local government expenditures. Gross private domestic investment fell by 10.6 billion, of which business inventories amounted to 10.2 billions. Construction and consumption expenditures have been the primary factors in checking the decline. Inventory changes, however, undoubtedly were the major private expenditure force in the turning point of 1953, but the reasons for their behavior seem less obvious.

The relatively high rate of inventory accumulation in 1953 II suggests that some part may have been unintentional. Prices declined slightly during 1952 IV and 1953 I. Therefore there seems to have been no price incentive to accumulate inventories. Some weight perhaps should be given to acceleration forces in explaining the inventory behavior since there was only a modest expansion in personal consumption between 1953 I and III. Personal savings as a percentage of disposable income fell from 8.5 per cent in 1952 III to 7.7 per cent in 1953 I but rose to 8.0 per cent in 1953 III. In the Hicks-Harrod view of turning points this could have been evidence of an approach to the production ceiling or the operation of a weak accelerator. As to the first view, production does not seem to have encountered a ceiling since symptoms of resource strain (inflation, production bottlenecks, etc.) were absent. As to the second, a weakness in consumption might reflect a redistribution of income. If, for example, a short-run shift of income in favor of profit receivers affects significantly the propensity to consume (and this is far from estabished), then the empirical developments of the

spring of 1953 may lend some support to the underconsumption version of the Hicks-Harrod model. Thus profits after taxes, although still considerably below the levels of the first three quarters of the Korean war, averaged 2 billion dollars more during the first three quarters of 1953 than they did from 1951 II through all of 1952.

The one other change in the spring of 1953 which conceivably might have upset business calculations was the establishment of the harder money policies. Interest rates firmed which undoubtedly had some effects on the demand for and supply of funds as well as on general business expectations. In the words of the Council of Economic Advisers in the January, 1954, *Economic Report of the President*: "The volume of new private flotations continued at a very high level but some difficulties were encountered at that time in marketing securities and placing mortgages." Apparently only consumer credit, which rose substantially prior to the initiation of more restrictive money policies, pointed toward the resumption of inflation. But the major inflationary boom itself had terminated almost two years before and prices in fact were declining moderately at the time the decision was made to tighten monetary conditions. In retrospect, therefore, these policies seem neither necessary nor wise.

Near-term Prospects

The present problem is at least threefold: first, to reduce the level of unemployment which is between 4 and 5 per cent of the labor force and to eliminate part-time work; second, to provide 500,000 to 700,000 new jobs annually to take care of the growing labor force; and, third, to secure the normal increase in productivity of between 2 and 3 per cent per year which requires an annual growth in the national product of between 7 and 10 billion dollars.

Although the immediate prospects for achieving all three of these objectives are none too bright, an upturn in general business seems to be under way and is likely to continue for several months at least. National security expenditures have leveled out but other federal government expenditures are planned to increase moderately during the current fiscal year and state and local government expenditures will continue their slow climb. Consumption has been well maintained and is increasing although there is no reason to anticipate a sharp shift upward in the consumption function. In this connection, it is worth noting that at present levels of disposable income personal consumption would change by only 4.5 billion dollars, before any multiplier effects, if personal savings were to fluctuate between their high and low ratio to disposable income since 1951 I. Some help may be expected

from gross private domestic investment if construction gains offset declines in producer durable equipment expenditures and if disinvestment in inventories gives way to inventory accumulation.

This very cursory examination of the prospects for expenditure indicates that a moderate recovery may be forecast although the forces for a substantial upturn are not yet in sight. It should be observed, however, that in many earlier fluctuations unexpected forces have made important contributions to revival and expansion. Nevertheless, the fact remains that unless there is a significant improvement in one of the major components of national expenditures, the gains from increased productivity are likely to be lost in increased unemployment or perhaps disguised unemployment (shorter hours, reduced labor forces, etc.). The mildness of the slump in production during the 1953-54 recession may suggest that the short-term or cyclical problem is giving way to long-run considerations which are to raise the level of product and the general standard of living. Whether this proves true or not, it would still seem desirable to give more attention to the problem of the "long haul"—to analysis of the secular factors which can undergird a progressive economy.

DISCUSSION

JOHN LINTNER: The two papers by Professors Roose and Fels provide a good review of the 1949 recession and subsequent cyclical experience in terms of the observed changes in broad aggregates and indexes. The analysis of these developments in both papers offers much that will be useful in subsequent work. In keeping with the chairman's invitation to serve as a general discussant for the session, I shall address myself briefly to two broad points raised by these papers, leaving detailed comment to the other discussants. While neither of the points is new, both seem sufficiently important to justify comment in the present session.

My first point has to do with the adequacy of analysis in terms of such comprehensive but often inherently heterogeneous aggregates as those used by the present authors and by economists generally in most of their work on business cycles. (The most significant exception to reliance on such aggregates in the present papers is Fels's distinction between the behavior of automobiles and other consumer durables in the 1949 recession.) It is certainly true that most of the major advances in this field over the last generation can be directly attributed to the development of national income accounting and related statements of money flows, on the one hand, and to the formulation and (usually mathematical) exploration of the properties and implications of theoretical methods specifically oriented to such aggregates, on the other. No one would want to question the great significance of the advances which have been made along both these lines, nor even the current usefulness of such efforts and analysis. But serious questions do need to be raised concerning the adequacy of analyses which deal only with comprehensive aggregates to meet the demands increasingly being put upon economists in this area. The development of sounder judgments on prospective business conditions leading to more responsible policy decisions as well as the necessary further scientific progress through tests of existing theories and elaboration of still better theories are going to depend heavily upon much more attention to the detailed components and shifts in the structural composition of important aggregates than has usually been given. The fact that data on many of the important components of our global aggregates are not readily available or that they are not precise enough to support firm conclusions does not lessen the need for more analysis at this level. These considerations simply put an added premium on efforts to develop and refine the empirical data required for such investigations.

Time permits no more than a single illustration. For this purpose, consider the important issue of the existence and extent of non-random shifts in the consumption function over the cycle. Both authors treat the consumption function as a behavioral equation (as it should be), but in keeping with the usual analysis of such issues, both then go on to infer the presence or absence of shifts with behavioral significance from comparative movements in consumption and disposable personal income in spite of the fact that this income aggregate encompasses unincorporated businesses, farms, and other nonpersonal economic units. For brevity, I shall confine my specific comments to Fels's

finding that an upward shift in the consumption function was more important in maintaining a relatively high bottom to the 1949 recession than the combined effects of built-in flexibility, the delayed impact of prior tax reductions, and current increases in government expenditures. His argument runs that since disposable income fell between the fourth quarters of 1948 and 1949 by 1.7 billion dollars, consumption would have fallen a substantial fraction of this amount if the function had not shifted. Since consumption actually rose by 0.7 billions, the function must have shifted upward by 1.6 billions or more. If Fels had assumed a marginal propensity to consume, say, 0.8, the indicated upward shift in the consumption function would have been 2.05 billion dollars. (Differences in the assumed value for the marginal propensity to consume do not significantly affect the estimates given below because of the very small value estimated for the change in disposable income that would be expected to have a net effect upon consumption expenditures.) But between the two fourth quarters, there was a reduction of 1.9 billion dollars in the rate of inventory accumulation in unincorporated businesses and on farms. *Ceteris paribus,* this involved an equal reduction of like amount in both personal income and personal savings. (I am here ignoring the changes in bank debt usually associated with inventory changes, since in this period reduced inventory was accompanied by increased rather than by reduced indebtedness. Data are from Irwin Friend's *Individuals' Savings,* published in 1954 by John Wiley & Sons, especially pages 46 and 47.) With tax payments and accruals virtually constant as they were, such changes have a multiplier of one with no repercussion effects via induced changes in consumption. Exclusive of income simultaneously created by and automatically reinvested in increased noncorporate inventories, disposable income on the basis of the available figures rose by 0.2 billion dollars; at the same time, personal savings other than those "automatically" invested in such inventory increases declined by only 0.5 billions. The resulting shift indicated in the consumption function is less than ¾ billion dollars, which is less than the probable effect of built-in flexibility alone and consequently substantially less than the combined effects of such flexibility, changed tax rates, and increased government spending. Indeed, since the indicated size of the shift is considerably smaller than the margins of error in the data, it is not even clear that the consumption function actually shifted at all. A much more detailed analysis than is appropriate in these remarks—and based on much better and fuller data than are presently at hand —would be required to settle the issue.

In order to avoid unwarranted implications, let me add that, like the authors, I am inclined to believe that there are significant non-random (as well as random) shifts in the consumption function during cyclical fluctuations, but what they are and why they are is still vague pending much more extensive and detailed investigation. At this time, my concern is simply to illustrate the proposition that we must use more detailed breakdowns than those provided in the present national income aggregates to test our hypotheses and develop our insight. A balance must of course be struck at some point. Perfect homogeneity of components is unattainable. Gains in homogeneity and relevance to the actual data, motivations, and processes of de-

cision making must always be weighed against costs in terms of complexity of models, less accuracy in data, and so on. (Budget studies and other cross-sectional analysis can go far toward meeting many of these problems.) But the economic balance between aggregation and disaggregation for cycle analysis surely lies in the direction of more detailed study of the components making up our aggregates than has generally been the practice. This proposition seems clear enough in terms of the economists' traditional effort to push toward the point of equality between incremental gains and losses; it can perhaps be made even more forcefully in terms of seeking minimax solutions in the allocation of our efforts.

My second general observation suggested by the present papers concerns what we may reasonably expect in the foreseeable future in the way of "a satisfactory theory" of business cycles. Useful historical or empirical work necessarily involves major elements of testing hypotheses incorporated in existing or new theories. The implications of the indicated modifications of hypotheses must be explored theoretically and new formulations developed for subsequent testing. This combination of theoretical and empirical work has produced the very real progress toward more satisfactory theories of the cycle which has been achieved over the last quarter century, and continued scientific work along these lines will undoubtedly produce much more satisfactory theories than we now have. Such studies at the micro as well as the sub-aggregate and aggregative levels should throw valuable light on the determinants of different types of saving and consumption and both the real and monetary factors which most importantly influence investment expenditure. Important distinctions and valuable new insights will unquestionably be gained as attention is turned to the separate components of all that is now lumped together vaguely as "autonomous" investment and as allowance is made for the distinctive impact on the rest of the structure of equal absolute or relative shifts in such distinctive types of investment. These developments should be particularly important in relation to the central question of the relation between longer term growth and cyclical fluctuations.

It would be a mistake, however, to hold out the promise that these prospective developments will do any more than narrow the range of differences in judgment and advice respecting policy which economists offer policy-makers. Any economic theory is necessarily partial and subjunctive in character. Even if we had an adequate formulated and verified theory of the operation of what are generally regarded as endogenous factors in the cycle, there would continue to be major differences in judgment regarding the probable future impact of exogenous factors. Since behavioral equations adequate to explain changes in endogenous variables necessarily include exogenous variables (or parameters functionally related to the level or rate of change of such variables), the reaction mechanism of the system will depend on values assigned or assumed for these other factors in each case. At still a different level there would continue to be differences in judgments regarding proper policy because of differences in the relative importance attached to alternative policy goals.

In sum, forseeable developments in the analysis of cycles will undoubtedly improve our understanding of the processes involved. They will consequently

give us a better basis for the judgments underlying our recommendations in particular cases, but significant differences and uncertainties in judgment will undoubtedly remain. Policy recommendations will consequently have to continue to be adapted to such uncertainty, and studies of how this adaption can be made more effective must press forward along with the study of the cyclical process itself.

DANIEL HAMBERG: I should like to devote my discussion to two aspects of Professor Fels's interesting paper. One is the tasks of business cycle theory; the other is the emphasis given the caution of businessmen with respect to inventory accumulation in the postwar period in Fels's explanation of the limited magnitude of the 1949 recession. In the latter connection I shall suggest a hypothesis for expecting inventory recessions per se generally to be limited in magnitude and perhaps become even more so in the future.

A major task of business cycle theory is to explain why prolonged periods of growth are sometimes interrupted only by mild inventory recessions—recessions that do not degenerate into major depressions—while at other times the lapses from growth take the form of major debacles of the magnitude of those of 1872, 1892, or 1929. Closely connected with this task is the correlative one of explaining why some cyclical upturns rapidly cumulate into extended periods of full employment growth (with only minor interruptions), while others, like those of 1892 or 1933, are markedly lacking in vigor and buoyancy, either barely reaching or actually falling short of full employment before the upper turning point.

Many business cycle analysts endeavor to explain these differences by emphasizing a constellation of fortuitous circumstances that may be associated with the individual upper or lower turning points. Among this group may be numbered such writers as Professor Slichter and generally those that have written under the auspices of the NBER.

Along with many other cycle theorists, however, I would disagree with this type of explanation. In the main this disagreement probably stems from an innate desire to look for a more systematic explanation—to look for factors that all inventory recessions, for example, appear to have in common. The same kind of search is associated with the examination of the major downswings as well as with the effort to explain the broad differences in upswings mentioned earlier.

Analysis of the inventory recessions of the past has thus led many cycle theorists, those whom Fels describes as placing the emphasis on "long-run investment opportunities," to the observation that the common denominator of all of these recessions is a surging rate of innovational investment and, almost without exception, a similar behavior on the part of residential construction. (This fact leads me to discount somewhat the significance of the apparent upward shift in the consumption function in 1949 as a major explanation of the mildness of that recession. The recessions of the twenties were equally mild, without any corresponding behavior on the part of consumption.) At the same time, those downturns that cumulate into major depressions appear to coincide with the (temporary) drying up of these outlets

for fixed investment. Invariably, too, the aforementioned differential behavior in upswings following major depressions seems to depend on the presence or absence of major innovations and high volumes of residential construction.

In the realm of systematic explanations of business cycles, also, are the endogenous or self-generating theories, particularly those that employ a multiplier-accelerator mechanism or some variation of it, like the models of Hicks, Goodwin, Clark, Kaldor, and Kalecki. These and virtually all other self-generating theories, however, suffer from the fundamental defect of imputing to capitalism more instability than actually exists, and there is enough already. For in their basic features, as Professor Fels has suggested, they imply that all movements in national income, upwards and downwards, tend to be explosive or antidamped. Except through appendaged obiter dicta that change them in basic ways, these models are incapable of dealing either with inventory recessions or with those periods of temporary stagnation sometimes described as weak cycles.

The lone exception to this statement is the remarkable model of Professor Frisch, developed some twenty years ago. Modified to allow the erratic shocks of autonomous investment to vary in frequency according to the magnitude of the shock (the larger shocks occurring less frequently than the smaller ones), the Frischian model meets the long-standing objection that it lacks the characteristic of systematic fluctuation that seems to have marked the business cycles of the past (cf. M. Kalecki, *Theory of Economic Dynamics*, Chapter 13). The principal posers of this objection have been among those advocating the assumption of antidamped cycles. With the objection disposed of, there is no longer any reason to continue to assume a priori that such cyclical behavior constitutes the underlying tendency of capitalism. When the aforementioned modification of the Frischian model is made, fairly regular cycles are produced by models possessing heavy damping coefficients as well as those with moderately damping coefficients. From the viewpoint of cycle theory, this is an important point, for it is only a model with heavy damping coefficients and kept alive by the irregular behavior of autonomous investment that is able to cope in self-contained fashion with the task of explaining not merely the systematic aspects but also the *differential* behavior of business cycles.

The necessity of a concept of fluctuating autonomous investment, i.e., investment based on other than income change, is apparent. The resistance to such a notion, even conceptually, is strange, since it is clear that certain categories of investment can in no way be related to the sale of goods already being produced. Though such investment can be influenced in its timing by income level, the important thing is that this relation is unsystematic. Not only may autonomous investment not fall during certain downswing phases, thereby limiting the latter to inventory fluctuations, but it may not rise during some periods of cyclical upswing, giving rise to periods of temporary stagnation. At the same time, it may also fall in the face of rising income, leading inevitably (in the absence of appropriate government policy) to the major downswings like those experienced in the past.

If pressed for a generalized explanation of the origins of inventory re-

cessions, I would point to the many possibilities of industry or sector maladjustments that may arise in a free market economy using roundabout methods of production. Over- or underestimation of demand in certain sectors, or, perhaps more important, unforseen shifts in the composition of aggregate demand—all in the face of a certain amount of immobility of resources—these are the most likely candidates in looking for explanations of upper turning points. But when the aggregate economy has strength and resiliency imparted to it by the surging streams of innovational and residential investment, during those irregular periods of growth, it shows itself quite capable of absorbing these sectoral maladjustments within the limits of inventory recessions. When these autonomous investment outlets are more or less absent, the same maladjustments find the bottom dropping out of the private stream of expenditures.

With these remarks as background, I should now like to call your attention to factors that may be expected to make inventory recessions generally mild (so long as long-run investment opportunities are favorable). Professor Fels expresses a feeling of inadequacy with the emphasis given the apparent upward shift in the consumption function and the strength of autonomous investment in his explanation of the mildness of the 1949 recession. To bolster his argument he feels called upon to cite the comparative absence of inventory speculation in the period preceding the recession. His suggested reasons for this state of affairs are that businessmen remembered what happened in 1919-21 when they engaged in inventory speculation, that memories of the depressed thirties added to their caution, and that this caution was furthered by the predictions of postwar depression.

It is difficult to believe that the current crop of decision-makers would be significantly influenced by what happened in an inventory cycle some quarter of a century earlier. Most of them were either nonexistent as businessmen or were probably in very low echelon positions, and it is very likely that their knowledge of what went on at that time is quite dim. I find this suggestion just too implausible.

More seriously may we take the suggestions about the effects of the memories of the depressed thirties and of the predictions of early postwar depression. There is no time to discuss analytically the general merits of these suggestions. On the empirical side, however, the events of the postwar period appear to contradict such notions. In brief, Fels is suggesting that postwar business sales expectations were inelastic. Under these circumstances one should expect to find business investment concentrated in just that area where the turnover is rapid and where therefore funds would not be irretrievably tied up in the event of imminent depression. In turn, this implies that inventory investment should have played a dominant role in total investment, while fixed investment should have played a correspondingly minor role. This is just what occurred in the years 1919-20; but of course, exactly the opposite happened in the post-World War II period. For one observer at least, the latter fact—the fact that so much was spent on fixed capital especially—provides ample testimony that business expectations were quite elastic in the recent postwar period.

To what then can we trace the absence of inventory speculation in the postwar period? In answer, I would reply that this kind of behavior should be regarded as entirely normal, that it is the periods of inventory speculation that are exceptional and require special treatment. On the whole, economists have been prone to exaggerate greatly business propensities to speculate in inventories on future price movements. In contrast to this picture is the real one, which shows that businessmen generally have a strong aversion for inventory speculation. Most think in terms of making profits out of their essential business operations; i.e., their production and sales operations, the appeal of their (differentiated) products. Rather than engage in activity whose sole feature is speculation, businessmen generally prefer to concentrate their energies on affairs over which they believe they have more control, where their training and talents give them a greater feeling of security. In a basic sense, an element of speculation inheres in all roundabout methods of production or purchasing in anticipation of demand. But this is unavoidable. It does not mean that businessmen favor it. In fact, much business activity is best understood if viewed as efforts to reduce risk and uncertainty. By the same token, the implication is that businessmen will not go out of their way in quest of unnecessary risk and speculation, such as is involved in price speculation in inventories.

Good, conservative business practice frowns on pure speculative profits. Proper inventory practice "is said to consist in maintaining such a relation between output or shipments (or sales) as is required to support the normal functions of production and selling." (Abramovitz, *Inventories and Business Cycles*, page 184.) The reasons why businessmen look askance upon inventory speculation are manifold. For one thing, guesses as to future price behavior may prove wrong as well as right; the possibility of inventory losses may therefore stand on an equal footing with that of inventory profits. Inventories tie up cash balances; other things equal, they make a business firm less liquid and leave it more exposed to disaster through illiquidity. The threat of bankruptcy because of illiquidity is something to which every businessman is keenly sensitive. Moreover, if he has to borrow from a bank to finance inventory speculation, his banker will be suspicious and most likely uncooperative. It will be made clear to him that he is no longer regarded as a solid, conservative businessman, that his credit is no longer as good as it formerly was.

In addition, the businessman—manufacturer, wholesaler, or retailer—who engages in speculative inventory purchases must face the added costs incident to storage, handling, obsolescence, depreciation, interest, insurance, and possibly property taxes. His storage space may be absolutely limited.

These remarks are not intended to imply that business firms never speculate to any degree in inventories. Thus when sales are rapidly advancing, individual business firms may often be found to be building up inventories somewhat faster than the growing sales. Even this action, however, is likely to be motivated principally by the desire to ensure maintenance of the share of future markets; that is, protect against slow deliveries, poor selection, etc. At times, also, salesmen may tip off buyers of an impending price increase,

and the latter may respond by stocking up heavily in advance. Again, however, this may not reflect pure inventory speculation so much as it represents an effort to protect the business firm's competitive position. Pressures in this direction are likely to be especially strong if one's competitors are known to possess large stocks purchased at the lower prices.

Granting these possibilities as to "speculation," as a rule such cases do not represent the commonality of experience, such as might be expected to generate cyclical movements in GNP. The only important exception to this rule arises in connection with sudden developments like the Korean war or the wave of speculative buying following World Wars I and II. Under such circumstances, past experience, rightly or wrongly, may lead businessmen into efforts aimed at large-scale inventory build-up in anticipation of later shortages or high sales levels. However, the motivating force in this situation, too, is likely to be less a desire to cash in on high future prices than a fear of just getting stuck with little or nothing to sell. In any event, these circumstances may be assumed exceptional enough to warrant separate explanation. The behavior of businessmen under these conditions is not characteristic of their usual peacetime practice and will tend to give rise to mistaken results if applied in an analysis of inventory fluctuations of the kind experienced in the distant past, 1948-49, or even 1953-54.

The implications of the foregoing remarks should be clear. They suggest that "normally" inventory cycles as such may be expected to be mild. Violent inventory fluctuations like that of 1919-21 must be explained in terms of special circumstances. Thus Professor Fels's emphasis on the caution of businessmen as regards inventory investment during the postwar period must be taken to be misleading, for such caution is the usual earmark of business attitudes toward this category of investment. Instead, he might well rest his case on the stabilizing role of autonomous investment behavior, aided and abetted by the fortuitous upward shift in the consumption function.

In closing, I should like to offer a hypothesis that not only may inventory recessions per se normally be expected to be mild in character, for the reasons elaborated above, but that there is some reason to expect a secular growth in mildness; i.e., diminution in both amplitude and duration. Examination of the data on inventory-sales ratios leaves one with the strong impression of a secular decline in these ratios; the only important exception to this trend seems to be in certain areas of durable manufactures. A priori, there are many good reasons that can be offered in explanation of this apparent trend. One is the movement toward industrial integration—a movement that recently seems to have developed renewed impetus. Vertical integration may be expected to reduce inventory-sales or output ratios by lessening the instability of supply taking the form of variations in delivery rates and prices of materials. Horizontal integration may also have the same effect by reducing the amount of inventories held as safety allowances against unforeseen variations in supply and demand, "for deficiencies in one plant may be compensated for by excesses in other plants" (Whitin, *Theory of Inventory Management*, page 149).

Perhaps even more important in explaining the downward trend in inventory

ratios are the widespread improvements in transportation, communications, production methods, accounting control techniques such as IBM methods, and standardization of product. There is every reason to expect continued improvements in technology, etc., with their resulting tendencies to reduce inventory-output or sales ratios.

Now the general consensus on the role of inventories in business cycles is that on the whole they are destabilizing, even when cognizance is taken of their stabilizing function as shock absorbers. Much of the destabilizing effects of inventory investment may be associated with the inventory accelerator; i.e., the desired ratio of stocks to output or sales. Few, at any rate, are likely to quarrel with the statement that, *ceteris paribus*, the duration and amplitude of inventory cycles are likely to vary directly with the value of the inventory accelerator. Whence it follows that the secular decline in this accelerator may be expected to have a general damping tendency on inventory cycles.

But this is not all. Most analyses of the role of the inventory accelerator have been based on the assumption of a linear accelerator. Recently, however, modern inventory control methods have shown that optimal inventory policy dictates that inventories should vary less than proportionately with changes in sales (see Whitin, *op. cit.*, Chapter 3). Introduction of such a variable accelerator also has a decisive damping effect on inventory fluctuations (see *ibid.*, pages 121-124). Contrary to the opinions of most economists on this matter, there is some evidence that many businessmen at present plan their inventories to behave in this manner, though for a variety of reasons. In part, this may account for the mildness of some of the recent inventory recessions. Beyond this, however, as modern inventory control methods attain wider use and more and more businessmen see the wisdom of varying inventories less than proportionately with sales, there is a further basis for expecting future damping in inventory cycles.

Finally, I should like to draw attention to what seems to be a trend away from FIFO in favor of LIFO accounting systems. To the extent that there exist any propensities to speculate in inventories, this switch should tend to reduce them. For the profits on inventory that arise during periods of advancing prices are wiped out under LIFO bookkeeping methods.

One final word in self-defense. My statement, quoted by Professor Fels, to the effect that the inventory recessions of the past testify to the stability of capitalism should not be misconstrued. It was made with those models in mind that indicate explosive behavior following all turning points. From the point of view of policy recommendations, I quite agree with Professor Fels that there is no basis for inferring from the 1949 and other similar experiences that major downswings are a thing of the past, as there is evidently some tendency to do currently. This view, however, is perfectly consistent with the notion that not all upper turning points may be expected to degenerate into major downswings—so long as there continues to exist an important category of investment that is independent of income change. The link between these seemingly opposing views lies in the fact that we do not as yet have tools that enable us to know in advance when there is a sufficient volume of "long-run

investment" outlets to limit downswings to the magnitude of inventory recessions and when there is not. But this is a problem of forecasting, not theory. Nonetheless, as long as our forecasting techniques remain weak, I for one would recommend policy based on the cautious assumption that any upper turning point poses a threat of major depression.

EARL C. HALD: Professor Roose has given us a concise summary of business cycle developments in the United States over the past several years. Much of of his analysis and most of his conclusions would seem to require nothing from me but expressions of hearty approval. I particularly welcome the note of caution he employs in surveying the future—a note that needs to be sounded for the benefit of those who, dazzled by the gyrations of the stock market and delighted to find that the production of steel at 75 per cent of capacity is still a highly profitable operation, are saying that the 1954 recession has been only a slight hesitation in the onward march of prosperity and high employment. Apart from, and yet affecting, the outlook for the future, certain aspects of the 1952-53 boom and of the 1953-54 slump may be worthy of more comment and analysis than could be brought to bear by Professor Roose within the limitations of his paper.

Pointing out that consumption rose by 14.1 billion dollars (at annual rates) from the second quarter of 1952 to the second quarter of 1953, and that investment rose by 8.7 billion dollars during the same period, Professor Roose says: "The dominant factors in this boom clearly were consumption and gross private investment." Although the importance of the increases mentioned was obviously great, one may question the omission of a reference to government spending for goods and services, which rose by 8.5 billion dollars during the same period, or almost as much as investment gained. It seems clear that this increase in the government portion of the spending stream substantially affected the character of the boom, just as the fall in government spending during 1954-55 has been of significant, if not dominant, importance during this latter period. As for the rise in consumption in 1952-53, it should be pointed out that consumption as a percentage of disposable income actually fell during the 1952-53 boom, personal saving having risen from 7.3 per cent of disposable income in the second quarter of 1952 to 7.8 per cent in the same quarter of 1953. (It is interesting to note that the proportion of disposable personal income saved continued to rise throughout the third and fourth quarters of 1953 but has fallen rather sharply in the second and third quarters of this year, coincident with the leveling out of the slump.) On the basis of these data it would perhaps be more accurate to say that the character of the 1952-53 boom was shaped primarily by government spending and private investment and much less by consumption spending.

Professor Roose comments only in passing on what is to me one of the most interesting aspects of the 1953 downturn; namely, the part played by monetary policy, as reinforced by Treasury interest rate policy, early in 1953. The focus on monetary policy has been sharpened by statements made lately, on the one hand, by its defenders to the effect that the easing of credit conditions beginning in May, 1953, saved the economy from a more severe depres-

sion and, on the other hand, by its critics that the tight money policy of early 1953 caused all of our troubles, and therefore the monetary authorities should not now be reminding us of how fortunate we are that conditions did not become worse than they actually proved to be! On this subject, Professor Roose merely points out that, judged by the behavior of the price level, the major inflationary boom had terminated well before 1953 and that therefore the decision to permit credit tightness to continue and even to be accentuated early in that year seems "neither necessary nor wise."

Needless to say, the part played by monetary policy in shaping the course of events during any period is extremely difficult to assess. This is partly because the real impact of monetary policy may not be manifest in the behavior of interest rates, which can be observed, but may appear in that obscure and indefinable area we call the availability of credit. Moreover, we must think of monetary policy also in terms of its indirect influence on the expectations of both entrepeneurs and consumers—not to mention the expectations of bankers, which may be of equal importance. In assessing the reasonableness of monetary policy early in 1953, however, we may begin by pointing out that, although the rise in wholesale prices had reached a peak early in 1951 and there had been some decline through June of 1952, the picture during the remainder of 1952 and early 1953 was by no means a clear one, and both wholesale and retail prices showed stability and at times even a rising tendency during the first three months of 1953. In view of all the indicators at hand, perhaps the conclusion of the monetary authorities that the inflation potential was still present is understandable.

There is little evidence that Federal Reserve authorities actively desired to tighten money rates in the spring of 1953. It is true that the rediscount rate was moved up from 1¾ per cent to 2 per cent in January, but this may be explained in part by the fact that the lower rate had been in effect for more than two years during a period of rising market rates and that the Federal Reserve authorities simply felt it was getting too far out of line with market rates. Moreover, the announcement of the rate increase just before the accession of the new administration to power implies perhaps more than anything a desire to dissociate this change, which had been contemplated for some time, from the policies of that new administration, which had already been revealed to be in favor of higher money market rates. After January, Federal Reserve ownership of government securities remained virtually unchanged until May, when an active program of open market purchases was inaugurated. Reserve requirements were permitted to remain at the levels which had prevailed for two years. It would appear proper to describe the policy followed by the monetary authorities throughout February, March, and April of 1953 as one of watchful waiting, after which a decision was taken to embark upon a policy of definitely easing credit. Meanwhile, the increasingly strong demand for credit continued to push market interest rates upward, the peak as measured by the prevailing rate on long-term government securities being reached in the first week of June.

How large a part was played by Treasury policy in the rise of interest rates between January and June is of course impossible to say. It is only fair to

point out, however, that there was a great deal of talk, both publicly and privately, to the effect that Treasury officials of the new administration were determined to convert at least part of the maturing debt into securities of much longer term, and that they were aware that this would require an interest rate structure higher than had heretofore prevailed. It is my belief that this announced policy had a good deal to do with the reaction of the money market, and that to the extent that the interest rate pattern of this period was due to government policy action, the blame or credit, as the case may be, must attach to the actions of the Terasury as much or more than to the actions of the monetary authorities. One might even go so far as to suggest that the tendency of the Treasury to "run with the ball," so to speak, resulted in such a sharp rise in interest rates that the monetary authorities soon came to the conclusion that credit stringency was developing too far and too fast. I have a strong impression that this course of events had much to do with the decision by the monetary authorities to reverse the trend of credit developments earlier than might otherwise have been the case. It is ironic that the Federal Reserve authorities, having striven for a full seven years to inaugurate a more restrictive interest rate policy in the face of determined and for the most part effective Treasury opposition, suddenly became faced with the opposite situation: a Treasury policy which now became too restrictive too rapidly. The result, thus largely fortuitous, was that the monetary authorities were able to establish a record of reversing monetary policy at the peak of the boom much earlier than they had succeeded in doing at any other point in history.

Unfortunately, much of this commentary must remain in the realm of speculation. The only thing that we can say definitely from the facts as they emerged is that the monetary authorities avoided overtly adding to credit stringency during a three-months period near the very end of the boom, and substituted for this essentially neutral policy a policy of active credit ease well in advance of any real evidence that the downturn was at hand. It may well be that the least fortunate result of the general rise in interest rates during the first half of the year was that growing scarcity of funds available for federally guaranteed mortgages resulted in a rise in interest rates in this area. This not only confused the picture as to whether the government was seeking to raise interest rates generally or to lower them, but may represent a substantial long-term cost to such borrowers, since the higher rate has remained in effect in spite of easier money market conditions. From the standpoint of its impact upon spending as a whole, however, the effects of the change in mortgage rates do not appear to have been serious, since construction has remained remarkably strong.

Monetary policy since mid-1953 requires little comment, the Federal Reserve System having followed an active policy of easing credit by means of reduction of reserve requirements, purchases in the open market, and two reductions in the rediscount rate. The extent to which these policies have contributed directly to the leveling out of the economic decline since earlier this year is of course uncertain; but the actions taken have at least been conducive to recovery.

With regard to the longer run, it appears to me that Professor Roose's questions concerning our ability to maintain high level employment, in view of our increasing labor force and of improvements in productivity, are well founded. Those who look to private investment for the necessary increases in the spending level are likely to be disappointed. Aside from inventory changes, gross private domestic investment has been remarkedly stable over the past three years—and remarkably high. Although we have been experiencing an unusual period of rapid technological advance and very high construction, it does not seem reasonable to expect these factors to expand further rapidly enough to take up the slack. Consumption, as Professor Roose has pointed out, could not be expected to provide much of the required increase, even if we assume a substantial drop in the rate of saving. With these points in mind, one might raise a serious question as to the efficacy of this year's tax reduction program, so largely based on the "trickle down" theory of demand stimulation.

The remaining factor is therefore government spending, whose sharp decline in the past year has so vitally affected aggregate demand. To the extent that world conditions permit a reduction in defense expenditures, it might be useful to think in terms of government spending for such items as highways rather than a further reduction in over-all government expenditures. Under present conditions with respect to revenue, this would raise the familiar question of the trend of public debt—surely too complex a question for our present limited discussion.

DEBT MANAGEMENT AND MONETARY POLICY

FEDERAL DEBT MANAGEMENT: CIVIL WAR, WORLD WAR I, AND WORLD WAR II

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The tools of public debt management used in each of our major wars fall into three broad categories. First, the government has created securities in a variety of forms. The interest rates, maturities, conditions for call or redemption, and other special provisions (including taxability) have been adjusted to fit various requirements. Second, the government has, to some extent, manipulated the market. In some cases this has simply been an advertising program, but more frequently it has been a high-pressure campaign playing on the emotional tension of the times. More important in recent years have been the somewhat mechanical techniques for rigging and underwriting the market. Third, the government has used powers of compulsion ranging from the thinly veiled threats of quotas to the declaration that certain of its securities are legal tender (i.e., money).

A variety of objectives have guided the use of these tools. Most economists have urged the debt authority to use them primarily as weapons for the control of inflation.[1] But the debt authority is also asked to finance large deficits without paying excessive interest costs, without establishing "unmanageable" postwar debt structures, without treading too harshly on our concepts of free markets, and without injuring the future ability of the government to borrow. The result has generally been a series of compromises, with service first to one then to another objective. As a result none of the goals has been fully achieved, nor completely disregarded.

The Civil War, World War I, and World War II did not impose equal burdens on the debt authority. Some indication of the different magnitudes of the financing problem is shown by Table 1. In terms of the diversion of resources to war, World War II was about four times as costly as World War I. The share of war costs financed by taxation in the Civil War was about half that of World War II. Furthermore, variations in the length of the war emergency, the degree of popular support for the war, the characteristics of the monetary system, and the distribution of the tax burden also influenced the magnitude of the

[1] The lack of unanimity among economists on this issue probably stems largely from disagreement on the desirability of direct controls. Faith in the effectiveness of direct controls for controlling inflation (plus, in some instances, a hope for a moderate degree of inflation) encouraged emphasis of other objectives, especially in World War II.

task of the debt authority. Nevertheless, in each of the wars the Treasury was faced with the task of channeling to itself large amounts of money in short periods of time. This pressure to maintain a rapid flow of funds is a distinguishing characteristic of wartime debt management. The following discussion of debt management practices examines some of the things the Treasury has done to meet this pressure.

Marketing the Securities. In each of the wars advertising campaigns stressed the patriotic duty of citizens to buy bonds, and in many cases the appeal to unsophisticated investors was based on a presumed need for funds for specific purposes ($18.75 will buy a grenade for our boys). The organization developed by Jay Cooke during the Civil War established the general pattern used in subsequent campaigns. Relying primarily on banks and other financial institutions as the focal points for local committees, he brought the sales message to every community. With the development of mass communication media and more scientific methods of calculating quotas, sales policies became more centralized but the core of the sales program has remained a voluntary "grass-roots" approach.

In wartime, the need to sell government securities to groups unfamiliar with investing requires that efforts be made to simplify the process. "Three will get you four in ten years" was one way to simplify the idea of compound interest. It was probably only exceeded in its simplicity by the "two cents a day" slogan which was used to sell the 7.3 per cent notes of the Civil War. Similarly, smaller denomination

TABLE 1

FEDERAL WAR EXPENDITURES, DEBTS, AND RELATED DATA

(Amounts in billions)

	Total Federal Wartime Expenditure	Percentage of Federal Expenditure Financed by Taxation	Increase in Federal Debt Over Last Prewar Year		Estimated Gross National Product for Years Shown*	Percentage of Gross National Product*		
			Amount	Percentage		Total Federal War Expenditures	Wartime Debt Increase	Interest Charge on Federal Debt
Civil War....	$3.3	21	$ 2.6	4000	$ 6 (1865)	54	43	2.3
World War I..	33	30	24.3	2000	77 (1918)	43	31	1.4
World War II.	368	42	229.2	550	215 (1945)	171	107	2.3

* GNP estimates for 1918 and 1865, designed to be comparable with the Department of Commerce concept used for 1945, were made with the assistance of Daniel Holland and Daniel Creamer, of the National Bureau of Economic Research, and are based on unpublished estimates made at the National Bureau of Economic Research.

SOURCE: *Annual Reports of the Secretary of the Treasury.*

securities have been offered to persons with low incomes. The development of the saving stamp scheme during World War I was the logical development of these efforts. The "withholding" program of the payroll savings plan of World War II was another such step designed to simplify and widen the distribution of government securities.

Preventing the Postponement of Bond Purchases. One obstacle to the sale of bonds during wartime is the reluctance of persons to invest because they expect bond prices to fall as the war continues. In October, 1942, this attitude apparently contributed to the near failure of at least one bond offering. But for the rest of the war the problem was eliminated by the clear understanding that the Federal Reserve System would protect the yield curve existing at the beginning of the war.[2] In World War I the Treasury had used the conversion privilege on the first two Liberty Bonds, saying, in effect: if bond prices fall, holders will be able to turn in their old bonds at par for any new bonds with higher coupon rates. By placing certain time limitations on the right to convert, by changing the tax-exemption privilege, and by naming the last loan a note instead of a bond, it restrained some of the conversion. Nevertheless, two-thirds of the convertible bonds eventually ended up in a form which dissipated the potential savings of the early low interest rates.

The Interest Rate "Trade." When upward pressure on interest rates becomes strong, the Treasury can relieve it by monetizing a portion of the debt. Later comments will show that the Treasury has made considerable use of this safety valve. But this is not the full extent of its efforts. It can also vary the security package by offering other types of yields.

Tax exemption has been the nearest alternative to the reward of higher interest rates. It was not used during World War II—except for the *de facto* exemption given to some holdings of savings bonds.[3] In the Civil War, tax exemption was probably not especially effective in in-

[2] The serious problem of postponed bond subscriptions was eliminated, although not everyone fully believed that the pegs would hold. The doubters were a good market for short-term, low-yield securities and, as Paul Samuelson put it, "the gullibility of investors" yielded a benefit to taxpayers. See his, "Turn of the Screw," *Am. Econ. Rev.*, September, 1945, pp. 674-676. Without this uncertainty the pegs probably would not have held. An interesting discussion of the disequilibrating effect of certainty in a supported market is found in Elinor Harris, "Money Demand and the Interest Rate Level," *Quart. Jour. Econ.*, February, 1950, pp. 105-124; also "Comment" and "Reply," *Quart. Jour. Econ.*, May, 1951, pp. 284-289.

[3] Since the large mass of holders of E-Bonds probably do not report the interest in their income tax return they may be considered, in effect, tax-exempt issues. For the same reason many savings deposits, savings and loan shares, and other widely distributed financial assets may also be considered tax exempt. However, it seems doubtful that this type of exemption was important to the people (small investors) who received it. Of course, if the Treasury had, at the time of sale, pledged a stronger enforcement of tax reporting for E-Bonds than other exempt assets, the attraction of E-Bonds would have been weakened.

creasing the demand for securities. The maximum income tax rate was only 5 per cent; so that at best the difference in yield was minor. More important, though, the exemption feature was not especially attractive because it was written into the revenue law instead of the security itself: it was only as good as the current revenue law.

Tax exemption was an important variable during the first World War. After the First Liberty Loan (which was fully exempt except for estate and inheritance taxes), a variety of exemption provisions were used. In the second and third loans, the Treasury increased the rate of interest and reduced the degree of tax exemption—partly in order to obtain a wider distribution of bonds with persons of moderate means and partly to prevent complete conversion of the first Liberty bonds. In the Fourth and Fifth Liberty Loans, the Treasury, attempting to halt the increase in interest rates, used one of the most interesting tax-exemption features known. This was the carry-back tax exemption through which, depending on the amount of fourth or fifth Liberty bonds held, additional tax exemption could be obtained on other Liberty bonds held. In other words, by purchasing the new bonds, investors could get tax exemptions for some of the previously taxable securities.[4]

In all of our wars, the government has attempted to substitute the appeal of patriotism for the appeal of high yields, but in none has there been as frank a recognition of this trade between better yields and patriotism as in World War I. Indeed, the excesses committed in the name of the war effort during World War I probably account in large part for the somewhat more relaxed approach used during World War

[4] The sequence was as follows (variations in exemptions applied only to graduated income and profits taxes):

First Liberties (3½%)—Exempt in unlimited amounts.

Second Liberties (4%)—Interest from $5,000 principal exempt.

Third Liberties (4¼%)—Same as Second Liberties but total exemption from Second and Third Liberties could not exceed $5,000.

Fourth Liberties (4¼%)—Same as Third Liberties but total exemption from Second, Third, and Fourth Liberties could not exceed $5,000.

Except: (1) $30,000 of Fourth Liberties were exempt until two years after the war and (2) an additional amount of Second and Third Liberties equal to 1½ times the holdings of Fourth Liberties (but not over $45,000) were exempt for the same period.

Fifth (Victory) Notes

4¾%—No tax exemption.

3¾%—Exempt in unlimited amount.

In addition, Second, Third and Fourth Liberty bonds equal to three times the holdings of Victory Notes (but not over $20,000) were exempt.

Thus, in addition to the exemption of unlimited amounts of the First and Fifth (3¾ per cent series) loans, purchasers could also obtain exemption on $100,000 of Second, Third, and Fourth Liberty bonds, largely by the carry-back exemption. For a short description of the carry-back exemption, see Robert A. Love, *Federal Financing* (Columbia University Press, 1931), pp. 178-180; some Treasury reasons for this type of exemption are given in a letter reproduced in the *Annual Report of the Secretary of the Treasury* (1918), pp. 14-17.

II. The social pressure of patriotism coupled with quotas can be a ruthless and discriminatory device—especially for the economically insecure.[5] The official disapproval of coercive patriotism during World War II can be contrasted with the statement, attributed to Charles G. Dawes, then chairman of the regional sales campaign, when someone expressed doubt that a 3½ per cent interest rate was high enough: "Anybody who declines to subscribe for that reason, knock him down."[6]

But if patriotism as a substitute for interest payments was deemphasized in World War II, its place was more than filled by another feature: liquidity. If money is defined in terms of the two attributes of standard of value and medium of exchange, there is a clear demarkation between money and debt. However, when stress is put on the first attribute and its subcharacteristics—a store of value and a bearer of options (the stress indicated for contingency and speculative balances)—the distinction between money and debt becomes hazy. Assets of the hazy type are ordinarily labeled liquid assets; their price in terms of the accounting unit is regarded as given but they cannot be spent. Debt management during World War II resulted in the creation of a large volume of such assets. In addition, assets were created which, though they lacked contractual stability in price, offered only minor prospects of variation and therefore only slightly less liquidity.

The nearness of the federal debt to money was fostered not only by the support program and the issuance of redeemable securities but also by emphasis on marketable short-term securities. Thus investors who were unconvinced of the tenacity of the support program and who were legally limited to a given amount of savings bonds could still find an ample volume of liquid securities in the short-term market. Table 2 shows this greater emphasis on short-term issues during World War II.

With an upward sloping yield curve, the stress placed on short-term securities resulted in a substantial interest savings. In other words, the Treasury also "played the pattern of rates." But this source of Treasury economy does not appear to have been available in our earlier

[5] See Henry C. Murphy, *National Debt in War and Transition* (New York, 1950), pp. 86-87. Murphy notes that there was probably much coercion in bond sales during World War II. But the effect on the bondholder was, at worst, only temporary because he could get back at his oppressor by selling or redeeming his bond without a loss. In this he was better off than the bondholder in World War I, who in most cases would have had to sell his bond at a loss.

[6] Quoted in C. J. Childs, *Concerning U. S. Government Securities* (Lakeside Press, 1947), pp. 119-120. The idea of a trade between interest rates and patriotism runs through many Treasury releases during this period. For instance, referring to the last Liberty Loan, Secretary Glass said: "Some students of the situation believed it would be impossible again to appeal to the patriotism of the American people. The Treasury held no such view. Furthermore, it was impossible to float the loan strictly on a commercial basis." *Annual Report of the Secretary of the Treasury* (1919), p. 33.

wars. The yield curves during World War I and the Civil War are not clearly defined, but they seem to have been virtually flat throughout the war; hence little interest saving was to be gained by using short-term issues.[7]

Although short-term obligations have been used in all of the wars, they were not generally considered respectable before World War II. In World War I, Treasury certificates were generally issued in anticipation of taxes or bond subscriptions. They were used for later pay-

TABLE 2

MATURITY DISTRIBUTION OF INTEREST-BEARING DIRECT FEDERAL DEBT*

(JUNE 30, 1865, 1919, DECEMBER 31, 1945)

Term to Redemption or Maturity	Percentage of Total		
	1865	1919	1945
Demand†	4	4	22
Less than 1 year	7	14	27
1 year and less than 5 years	40	0	8
5 years and over	49	82	43
	100	100	100
	Amount in Millions		
Total interest-bearing direct federal debt*	$2,217	$25,234	$237,545

* Excluding special issues held in Treasury trust accounts.

† Includes issues which possess a definite maturity date, e.g., savings bonds or notes, but which are payble upon demand of holder.

ments to the government and were never really considered a permanent part of the debt. In the Civil War, the substantial use of three-year notes (and bonds callable in five years) was rationalized by the Treasury as a means of retaining control of the debt. Interest rates were expected to decline and the government was expected to retire some of the debt in the postwar period; hence it was deemed desirable that a portion of the debt be subject to refunding. Nevertheless, the objective at the time was still to refund the short-terms into long- and this was done shortly after the war. On the other hand, the greater emphasis during the thirties of the liquidity needs of financial institutions and nonfinancial corporations led to greater acceptance of short-terms and

[7] In World War I, the basic yield curve for high-grade corporate securities was flat in the first year of the war and slightly downward sloping to the right in 1918 and 1919. See David Durand, *Basic Yields of Corporate Bonds 1900-1942* (National Bureau of Economic Research, Technical Paper, No. 3, 1942), p. 5. Since the spread between corporate and government securities probably varied from time to time and for different maturities, this yield curve is only a rough approximation of the government's curve. A sampling of government security yields during the Civil War suggests the same thing: a flat curve with a slight tendency toward higher yields for short-terms.

permitted them to play a permanent part in the "tailoring" program of World War II.

Maintaining the Prices of Government Securities. The problem of maintaining the prices of outstanding securities can be evaded through the use of fixed price, demand obligations such as the Series E bonds used during World War II, the War Savings Certificates (which were redeemable upon ten days' notice) used during World War I, and the Temporary Deposit Certificates (redeemable on ten days' notice) used during the Civil War.[8] Such demand obligations were only used to a minor degree in World War I and the Civil War. But in financing World War II, against a background of uncertainty caused by the depression, it apparently seemed unlikely that anything less than a completely safe security could attract a large volume of small savers' funds. Thus fixed price obligations accounted for over one-fifth of the World War II debt.

The efforts of the debt authority to maintain prices of government securities in the market have received considerably more attention than these contractual agreements to pay a given amount on demand. Manipulation of the prices of outstanding securities is a familiar technique of bond sellers—and the Treasury is no exception. When the Treasury is able to hold the market prices of existing securities at or above their issue price, two results are achieved. First, it prevents the investor from finding better, cut-rate securities on the open market, and, second, it indicates to the public that the purchase of a government security will not involve a direct capital loss. In this regard the support program of World War II was clearly a success.

In contrast to World War II, the support operations of our earlier wars were crude and ineffective. In the Civil War, the Treasury commissioned its agent to enter the market on several occasions to purchase issues which, because of their low prices, were discouraging the sales of current offerings. The largest of these support operations involved only 10 million dollars and apparently took only the biggest bargains off the market. They were solely emergency operations designed to deal with specific market situations and the authority to purchase securities was never continued for more than a month or two at a time.[9]

[8] The Temporary Deposit loan of the Civil War was an interesting excursion by the Treasury into the banking business. These deposits, made at the subtreasuries, paid 4, 5, and 6 per cent interest at various times. Virtually on a demand basis, they were attractive to banks and other financial institutions for investment of short-term funds. The certificates ultimately came to be used by city banks for settling clearing house balances. In the type of funds they attracted and the way they were used they were similar to the Treasury bills of World War II.

[9] An interesting source of price support existed with the gold bonds of the Civil War. Since the market price of gold rose at almost the same rate as the general price level, bondholders receiving interest in gold and a pledge of redemption in gold had what amounted to a "purchasing power bond." The price of gold bonds reflected this by stay-

During the first World War, several devices were used directly to support the prices of government securities. Beginning with the Third Liberty Loan, the Treasury was authorized to set aside 5 per cent of the proceeds of new bond issues for the purpose of repurchasing securities, the maximum price to be paid for these securities was par plus accrued interest, and the securities purchased were to be canceled and considered retired. In the first six months in which this program was in effect the Treasury purchased close to 250 million dollars worth of Liberty bonds and by November, 1919, it had purchased 953 millions.

This type of support operation was, in effect, simply a preliminary repayment of the portion of the loan which appeared to be excessive (excessive being defined as that marginal amount of the loan which tended to depress the market price below some predetermined level). It meant that the Treasury had to borrow more funds in other loan drives or concurrently sell some other type of security. In a period of rising interest rates, it meant that the Treasury was in effect refunding a portion of the outstanding debt at higher interest rates.

Repurchase of Treasury securities was also made possible in World War I by the establishment of the War Finance Corporation (WFC). This agency obtained most of its funds from the Treasury and purchased government securities for its own account and then later sold them to the Treasury. Since WFC held 487 millions in November, 1919, the total amount purchased by the Treasury by that date was 1.43 billion, or 5.8 per cent of the war debt.

This "by its own bootstraps" operation, in which the Treasury used mainly borrowed funds for the purpose of buying its own securities, was not wholly successful if the maintenance of prices at par is taken as the criterion. Except for the First Liberty Loan, each of the Liberty bonds fell below par from the first time they were traded and gradually moved down in price for several years. But, obviously, par need not be taken as the sole measure of success, since we have no way of knowing how far prices might have fallen without these operations. It is interesting to speculate, however, whether the government would have

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ing above par, but the full impact of the inflation never showed in bond prices. If it had, bond prices should have increased with increases in the price of gold. (To maintain a constant yield the price of a gold perpetuity would be a constant multiple of the price of gold.) Since increases in the price of gold were also interpreted as indications that the government might not continue to pay in gold, a prospective purchaser could not view the bond as a pure purchasing power bond. Nevertheless, for those persons who bought them at par and held them through the inflation they were almost as good. Interesting comments on this aspect of the Civil War gold bonds are found in Wesley Clair Mitchell, *History of the Greenbacks* (University of Chicago Press, 1903), pp. 275-279, 377-378; also, Frederick R. Macaulay, *The Movements of Interest Rates, Bond Yields, and Stock Prices in the United States Since 1856* (National Bureau of Economic Research, 1938), pp. 201-203.

helped itself more if it had sold 1.5 billion dollars fewer bonds and forgotten all about trying to support prices.

The pressure of declining security prices intensified as the war progressed and in the initial version of the Fourth Liberty Loan Act the ultimate solution was offered: a provision giving the President the power to prohibit the transfer of bonds on the market. After much debate, the provision was softened so that the President could simply investigate and regulate sales. Apparently scant use was made of the power to subpoena books and testimony, but there is ample evidence that the Treasury employed its powers of persuasion in the market place. Treasury Department statements made it quite clear that the government considered bond buyers as patriots and people who sold their bonds were frequently the opposite—although they were never quite called traitors. In contrast with these sometimes tortured devices of World War I, the simplicity of the support program of World War II seems almost benign.

Monetization of Debt. In a sense, the amount of the debt which spills over into the monetary system is a measure of the effectiveness of all the rest of the debt management policies. That is, the monetized debt may be considered a residual: what is left over after other sales efforts had been used. The public record clearly shows that our wartime debt authorities have regarded monetization of the debt in this way; they considered monetization to be undesirable and they attempted to avoid it. But it is important to recognize that the other residual market (remaining nonbank funds) still existed and could be tapped with the residual tools—greater sales efforts, higher interest rates, more tailoring, or further effort with any of the other flexible sales weapons. The fact that these other tools were not used suggests that monetization was considered a more flexible, more certain, or less costly tool than the others.

In World War II the approach was fairly simple: the objective was to sell as many securities as possible, with the weapons used, outside of the banking system and then to sell the balance to the banks.[10] In order to perpetuate this arrangement, the Federal Reserve System and the Treasury developed the support program. And to further implement the program they employed a new tool of war finance: the yield curve. With this ingenious device, deviations from

[10] In a formal sense, only purchases by the central bank are inflationary. Sales of government securities to commercial banks do monetize the government debt, but if the supply of reserves is given, government debt is simply substituted for private debt. However, when the central bank keys its purchases (or advances) to commercial bank purchases, i.e., more commercial bank purchases result in more central bank credit, then sales to banks are inflationary. Since commercial banks had and used the privilege of shifting securities to the central bank during each of the world wars, sales to them are considered inflationary here.

the normal yield curve were the trigger mechanism for the creation of new bank reserves. From the standpoint of simplifying the task of debt management, this program was a major advance in war finance. It permitted the Treasury to develop a basket of securities which bore certain well-defined characteristics and to use these same securities with minor changes for the duration of the war. In contrast, the securities of our other wars had been continuously sprinkled with a variety of special "this time only" provisions.

As shown in Table 3, the Reserve banks purchased close to 9.6 per cent of the debt of World War II. In World War I they had purchased only about 1 per cent. But whereas Reserve bank loans secured by government securities were only 0.1 per cent of the new debt in World War II, they were equal to 6½ per cent of the debt of World War I. Thus Reserve bank credit on account of government securities amounted to 9.7 per cent of the World War II debt and 7.4 per cent of the World War I debt.

In World War I, a preferential discount rate on notes secured by government securities was the major device employed to expand Federal Reserve credit; in World War II open market operations accomplished the same thing while at the same time registering distinct price quotations in the government security market. These means used to expand Federal Reserve credit reflect the prevailing theories of monetary controls. In 1917 the discount rate was the core of Federal

TABLE 3

BANK CREDIT AND FEDERAL DEBT, WORLD WARS I AND II

(Amounts in billions of dollars)

	World War I June 1916–June 1919		World War II June 1941–Dec. 1945	
	Amount	Percentage of Total War Debt	Amount	Percentage of Total War Debt
Total war debt	$24.26		$229.15	
(1) Increase in government securities owned by Federal Reserve banks	.24	1.0	22.01	9.6
(2) Increase in government securities owned by commercial banks	4.39	18.1	70.70	30.8
(3) Increase in Federal Reserve loans secured by government securities	1.55	6.4	0.22	0.1
(4) Increase in commercial bank loans secured by government securities	3.0*	12.2	6.0*	2.6
War debt financed by bank credit (1+2+4)	7.6	31.3	98.7	43.0

* Estimated on the basis of reporting member banks.

SOURCE: Board of Governors of the Federal Reserve System, *Bulletins, Banking and Monetary Statistics, Annual Reports.*

Reserve policy; open market operations as a means of influencing the volume of credit were still to be invented. In 1941 the situation was reversed; open market policy was considered paramount and the discount rate was regarded largely as a psychological tool.[11]

About one-third of the debt created during each of the world wars was financed by commercial bank credit. In the second World War the banks purchased 31 per cent of the new debt for their own portfolios and carried an estimated 3 per cent by loans to customers. In World War I, the banks purchased only 18 per cent on their own account and extended loans to customers for approximately 12 per cent more of the war debt.[12] Financing of the deficit by commercial bank credit can be roughly set then at 30 per cent in World War I and 34 per cent in World War II.

In World War I a greater share of the new reserves was used to extend credit to customers under the "borrow and buy" program. This, of course, was more in keeping with the then prevailing ideas of banking. The customer loan was considered more liquid than the security which it purchased. By World War II, banks had become accustomed to larger government portfolios; new means of liquidating securities had been developed; a bank could afford, therefore, to invest more heavily in governments. The fact that the Treasury provided a stable market and a host of varying maturities, enabling a bank to pick its liquidity risk, was clearly more conducive to direct holdings than the emphasis on illiquid long-term securities of World War I.

A comparison of the monetization of the debt in the world wars with that of the Civil War shows the contrast between types of monetary systems. The first Civil War loan, which was taken by a group of large banks, contributed to the suspension of gold payments by the banks and eventually by the Treasury. Into this monetary vacuum the Treasury issued its first series of greenbacks: legal tender notes.

[11] Anna Youngman suggests a number of factors which may account for the shift in techniques: the development of the tradition against borrowing and Federal Reserve disapproval of borrowing as a long-run source of reserves, the excess reserve episode of the thirties, and bankers fear of being trapped, as they were in 1920, by rising discount rates. See her *Federal Reserve System in Wartime* (National Bureau of Economic Research, Occasional Paper No. 21, 1945), pp. 26-29. Of course, banks were trapped in 1920 by a reversal in Federal Reserve policy. And the same thing could have happened following World War II if open market policy had been reversed.

[12] Two points have been raised about the customer loan data: (1) some customer loans secured by government securities may have been used to finance purchases other than government securities; (2) some government securities may have been purchased with loans secured by personal notes or other collateral. Regarding the first, such borrowers had a choice of selling their bonds or borrowing new funds; since they chose to borrow, banks did, in effect, finance their bondholdings. As for the second, there may have been a small amount of such lending, especially where customers already had established a line of credit; but the preferential borrowing rate generally accorded to loans secured by governments undoubtedly kept most such bank credit in the "secured by government obligations" classification.

Subsequently, the issues of greenbacks, demand notes, and fractional currency rose to nearly 460 million dollars, or about 17 per cent of the total war deficit. In addition, the Treasury also issued over 225 million dollars of one-, two-, and three-year legal tender notes which paid 5 per cent and 6 per cent compound interest. The 5 per cent notes bore coupons which could be clipped, eliminating any interest accrual.[13] They were therefore widely used as currency. The three-year compound interest notes were an attempt to correct this situation by allowing interest to accrue for the whole period to maturity (after three years a $100 note had a redemption value of $119.40). Approximately 8 per cent of the war deficit was financed by this interest-bearing legal tender.[14] In addition, by June 30, 1865, the national banks held 392 billion dollars in government securities of which 236 million was held on deposit with the Treasurer of the United States. Data on state banks' holdings of government securities are not complete but they probably did not exceed 25 million dollars by the end of the war. Altogether, then, it appears that national and state commercial banks absorbed about 15 per cent of the war debt.

These three categories of Treasury issues—legal tender notes, interest-bearing legal tender notes, and securities sold to the commercial banks—account for about 40 per cent of the wartime deficit. Thus in terms of monetization of debt, the Civil War falls in between the two world wars. However, because Civil War taxing policy was less vigorous, the monetized debt played a larger role in over-all fiscal management than in the two later wars.

Conclusions. The debt authorities during World War II employed more potentially inflationary devices than were used in either of the previous wars. Pegged securities, short-term securities, and redeemable securities were the core of the World War II program. This strong emphasis on liquid securities must be partly explained as a heritage of the depression. Hard-won low interest rates apparently could not easily be cast aside by the people who made them, especially since the same people also believed that the economy would return to prewar conditions. Thus time and again the Treasury explained its low interest rate policy by stressing the importance of low rates as a contribution to postwar full employment. But perhaps equally important was the fact that by 1942 a host of direct controls had been invented

[13] We can get a rough measure of the value to the Treasury of the legal tender provision at the time of issue by noting that these one- and two-year 5 per cent legal tender notes were issued at par at the same time that one-year 6 per cent certificates of indebtedness were selling at around 95.

[14] Of course the whole 225 million dollars did not circulate as money for the entire time it was outstanding. It seems likely that more of the compound interest notes circulated in their first year (1865) than later. Since the notes were spendable and had the legal status of money, they are included here as part of the monetized debt, though it is clear that only a fine line separates them from some of the nonmarketable issues sold during World War II.

and accepted. The use of direct wage and price controls and rationing removed a major obstacle to the development of the full-scale support program. These two factors—the presumption that inflation would not be a major problem after the war and, just in case, the development of tools to keep inflation under control—help explain the greater emphasis on liquidity in World War II debt policy. Subsequent events made this calculated risk wrong.

But if the calculated risk proved to be wrong, some qualitative gains were still made. Tax exemption with its resultant discrimination in favor of the wealthy was discarded (a difficult weapon to discard if the Treasury is to rely largely on the natural forces of the market). Vigilante subscriptions which act like a tax, without the careful equity considerations generally found in a tax, were virtually eliminated. And, although as it developed in World War II the total effect of the program of tailoring the securities to meet the needs of investors was inflationary, the principle of differentiated markets suggests a more equitable debt structure than the policy of fitting most investors to Liberty bonds.

Broadly speaking, the government is faced with two alternatives in financing a given wartime deficit. First, it can use money and near-money and then rely on a variety of direct controls to restrain inflation. This was the emphasis of the policy followed during World War II and when controls were discarded following the war the inflationary potential was revealed. Second, by using forms of compulsion and/or securities bearing sufficiently high interest rates the Treasury can induce or force the public to give up liquidity. This policy was stressed more in the debt management of World War I. The first alternative would be used where low interest rates, stable bond prices, moderate sales efforts, and direct controls are considered desirable or at least unobjectionable. The second would be used where control of inflation is considered the paramount objective and where continued direct controls are considered objectionable or politically unobtainable.

The central bank may reverse the illiquidity implicit in the second approach when the threat of deflation looms. Open market purchases should be sufficient. But the ability of the central bank to remove the liquidity implicit in the first approach may be severely limited by the composition of the debt. Contractual near-money, such as redeemable bonds and short-term securities, cannot easily be destroyed by the monetary authority. Indeed, there is the possibility that attempts to restrain the "moneyness" of a portion of the debt (say, by dropping pegs) would result in the creation of even more money.[15] The implica-

[15] Of course, if the central bank is willing to completely cut loose from the Treasury, contraction would be possible. But seemingly the more the debt is floating, the more the

tion seems clear that if the debt is to aid in the control of inflation it must consist of securities which are subject to attack by the monetary authority (ideally, consols). In this regard the debt management of World War I was considerably more successful than that of World War II.

But before approving a future war program based primarily on consol-like obligations, several familiar objections must be raised. Could such instruments be sold? Would it be desirable to attempt to distribute such securities widely and permit the repercussions of changes in monetary policy to bear directly on small bondholders as well as large? Would wide distribution of central bank ammunition in this kind of a political system constitute a practical restraint on the use of contractive monetary policy? Answers to these questions are far from obvious (nor do these questions cover all the problems of using consols) but they all suggest one point for consideration; namely, that nonmarketable securities should have a place in a program for financing a future deficit. However, if such securities are used, it would be desirable to restrict the degree of redeemability considerably more than was done during World War II.

If during wartime the general appeal of nonmarketable securities with restricted redemption privileges would preclude a wide distribution, then some form of compulsory program may be necessary. A compulsory lending program tied to income or social security taxes and using, perhaps, a cost-of-living index to determine interest payments would if applied to a portion of the debt have the advantage of leaving the balance of the consol-like debt free to monetary manipulation. In this context of compulsion, even a security reserve program for banks and other financial institutions (and perhaps even nonfinancial concerns) might not appear inappropriate.

The assumption behind these comments is that the lessons of earlier debt management have demonstrated that the primary goal of wartime debt management should be to aid, not hinder, the control of inflation. Even more basically, they assume that debt management should help to distribute the cost of war in a more equitable manner than by inflation. But a "fight inflation at any cost" approach to debt management can, as we have seen, have its own harsh and inequitable aspects. The first task, then, in formulating a program for the management of future war deficits will be to make a clear-cut decision of what the objectives of wartime debt management really are. Once these objectives are established, the demonstrated inventiveness of the monetary and debt authorities should be adequate for the task.

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central bank may be considered responsible for it. See Robert Roosa, "Integrating Debt Management and Open Market Operations," AEA *Papers and Proceedings*, May, 1952, pp. 214-235.

MONETARY POLICY IN RECENT YEARS

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Four years ago monetary policy was reinstated as a major measure of government policy for combating booms and depressions. Since then flexible monetary action has played a classical role in dealing with a complete business cycle.

This has also been a period of rediscovery and adaptation of the instruments of monetary policy. From long disuse, the workings of the instruments for general credit regulation had become unfamiliar to the financial and business community. In certain respects, moreover, changed circumstances called for changes in the techniques of their use, although in general old concepts and principles have proved remarkably appropriate to the postwar world. Debt management policies and techniques have also been worked out which help to keep the financing requirements of the federal government from tying down monetary policy and to reinforce monetary policy with debt management actions.

It is the primary purpose of this paper to consider how the three major instruments of general credit policy were actually used in a co-ordinated program looking alternatively toward credit neutrality, restraint, and ease over the period since March, 1951. Open market operations and, in a supplemental way, changes in reserve requirements are the principle means available to the monetary authorities for making available the reserve funds needed to support economic growth while maintaining whatever degree of pressure on bank reserve positions may be considered appropriate to the prevailing economic situation. Reserves not supplied by these instruments are available at the initiative of individual member banks through borrowing at the Reserve banks. When reserve funds are obtained this way, however, banks are obliged to repay and tend to restrict their credit extensions in order to do so.

During most of the period under review, the degree of pressure on member bank reserve positions seems to be most usefully indicated by the volume of member bank borrowing. Another indicator—the volume of unborrowed excess reserves or free reserves—might perhaps be used more appropriately in periods of extreme credit ease because the volume of borrowing cannot become negative. For purposes of this discussion, however, I shall keep to a single indicator: the volume of borrowing.

Revival of Discount Function after the Accord. With the Treasury-Federal Reserve accord in March, 1951, a major restrictive step was

taken in permitting prices of government securities to find their own levels in the market, without open market action by the Federal Reserve to establish them or directly affect them. This action stripped excess liquidity from the economy. It reinstated in the market a distinction between shorter and longer term securities; that is, between securities of little market risk and those of greater market risk. It made it desirable, in fact, to permit a fairly sizable growth in the volume of bank credit and money over the ensuing year or more in order to cushion the force of the policy change.

During this interval general techniques of monetary management which had been dormant for two decades were brought back into use. Most important was the resumption of co-ordinate use of open market operations and the discount mechanism to achieve whatever conditions of supply, availability, and cost of credit were considered appropriate to the developing economic situation. Open market operations became again the instrument for influencing on a current basis the over-all volume of unborrowed reserves of member banks. When policy-making officials believed there was need for restraint on bank credit and monetary expansion, less reserves were made available on an unborrowed basis, thereby obliging banks as a whole to increase their borrowing if they were to expand credit. When easier credit conditions were desired, open market operations were used to reduce the pressure of indebtedness on banks by increasing the volume of reserves available from sources other than borrowing.

The extent of member bank borrowing affects bank lending and investing because individual banks are generally reluctant to remain indebted to the Federal Reserve for more than a temporary period. Indebted banks endeavor, therefore, to make adjustments in their total loans and investments to whatever extent may be necessary for the repayment of their borrowings. When indebted banks make these adjustments, they of course shift at least part of the reserve deficiency to another bank, and that bank will in turn need to borrow and make subsequent asset adjustments to repay.

This shifting among banks of the burden of borrowing at the Federal Reserve tends to make the Federal Reserve discount rate effective. At such times the discount rate becomes the pivotal rate in the money and capital market, and money rates tend to array themselves in relation to the discount rate. In a technical sense, this relationship tells banks whether it is cheaper to borrow or sell securities for a short-period adjustment. When through open market operations member bank reserve positions have been put under pressure (or allowed to get under pressure as bank credit and deposits expanded), money rates tend to range higher in relationship to the discount rate. As bank re-

serve positions ease, market rates will be lower in relation to that rate.

In the postwar world the liquidity positions of banks and of other institutional lenders have been largely involved in the government securities market. In the short- and intermediate-term sector of that market, banks are a major, and generally the dominant, factor. When banks have developed reserve deficiencies and are obliged to borrow, they commonly seek to repay borrowing initially by sales of short-term government securities. When such selling pressure is increasing, yields on short-term government securities tend to rise. Banks under these conditions may also become sellers of intermediate-term and longer issues, or at least will be less interested in buying, and these yields will also tend to go up. Such a development then will affect not only the liquidity of banks but also the liquid position of savings institutions of various kinds because of the reduced value of their sizable holdings of intermediate- and longer-term government securities. Pressure or ease in bank reserve positions is thus reflected rather promptly in the tone of the entire credit market.

When open market operations are being used so as to develop restraint on credit expansion, it will become appropriate at some phase to supplement the action by an increase in the discount rate. This move will strengthen the reluctance of member banks to remain indebted to the Reserve banks by making current borrowing more expensive and probably by lending support to expectations of discount increases yet to come. Conversely, when credit conditions are being eased through open market actions designed to reduce the volume of member bank borrowing, a reduction in the discount rate will help relieve banks of the pressure to make even temporary adjustments in reserve positions in the short-term market rather than by use of borrowing at the Reserve banks. By changing this pivotal rate, moreover, the Federal Reserve exerts a strong influence on the cost of most other kinds of credit, since a rate change is an official recognition of a change in supply and availability conditions for credit that is likely to continue for a time.

In the four-year period since the accord, the interrelationship of the volume of member bank indebtedness, the general tone in credit markets, and the level of market interest rates in comparison with the discount rate have followed the pattern just outlined.

General Credit Measures, Mid-1951 to Mid-1952. During the first year after the accord, monetary policy was relatively neutral. There was a fair balance in the demand and supply of credit, given the credit and monetary expansion the Federal Reserve regarded as reasonable in view of the changed liquidity situation of the economy. Over the period, member bank borrowing at the Reserve banks tended to average

under 400 million dollars, except for the seasonal peak in December. Few, if any, individual banks remained long in debt to the System, but a number were alternately in and out of debt. With this general level of borrowing, the Treasury bill rate remained somewhat under the discount rate of 1¾ per cent, the yield on three- to five-year Treasury securities hovered around 2 per cent, and the yield on twenty-year Treasury bonds fluctuated under 2¾ per cent. Corporate and municipal security yields showed comparable stability and moved within a fairly narrow range.

Actually, during this period of about one year the stability in the general level of interest rates (although not of course in the interrelationship of yields on various types of credit) reflected largely the fact that banks obtained enough reserves from all sources other than by borrowing at Reserve banks to support a substantial bank credit and monetary expansion. This bank credit, added to savings available from nonbank sources, was sufficient to meet at stable interest rates the large credit demands coming to the money and capital market.

Credit Restraint, Mid-1952 to Mid-1953. Pressures of credit demand began to accelerate, however, in the second quarter of 1952 and over the following year, particularly with termination of controls over consumer credit and with the surge of activity that developed in the fall and winter. A vigorous expansion in private spending and in private credit demands developed just as defense outlays were reaching a peak and federal financing needs became exceptionally heavy. The rapidly rising volume of capital expenditures, inventory accumulation, and consumer durable goods outlays—to a large extent financed by credit—were contributing to overtime operations in industry and threatened to develop an unsustainable boom. Credit and monetary policy were turned toward curbing somewhat the expansion of credit and money, with the objective of preventing the volume of nonfederal spending from forming an extraordinary bulge in a relatively short period of time.

The Federal Reserve was not willing under these conditions to provide through open market operations the volume of bank reserves that banks wanted to support their lending activities. With increasing frequency individual member banks were obliged to go to their Reserve banks for temporary borrowing accommodation. As the volume of borrowing increased, member banks intensified their efforts to adjust asset positions so as to be able to repay. Growth in the volume of bank credit thus came under more and more restraint.

Increasing reliance by member banks on borrowing in the second half of 1952 was reflected in a rise in interest rates. Rates on three-month Treasury bills, which had been below the discount rate except

for an end-of-1951 peak, rose above it after mid-1952. Intermediate- and long-term rates also went up fairly steadily over the period. Early in 1953 the Reserve banks increased their discount rates. The credit market had partly anticipated this move in its prior rise, but the rate structure rearranged itself upward somewhat further in adjusting to the new discount rate of 2 per cent.

The volume of bank borrowing continued very large (about 1¼ billion dollars) during the early months of 1953, a season when bank reserve positions tend normally to be easy. Under this pressure, banks redoubled efforts to bring their reserve positions into line, and the rise in interest rates accelerated.

The rise in interest rates in early 1953 and the general tightening in credit availability did not, it should be noted, reflect an actual decline in the total amount of new credit available. It was due, rather, to an extraordinary bulge in the demand for a number of types of credit and the fact that bank credit was not permitted by the monetary authorities to increase by as much as would have been necessary to meet fully this increase in demand.

Credit Easing, Mid-1953 to Mid-1954. By midspring of 1953 monetary measures had made their contribution to preventing an excessive boom. Further monetary tightening was not needed from this standpoint, and developments in money and capital markets and in the economy generally indicated that if further tightening did occur it might cause unnecessary adjustment difficulties in coming months. Credit demands remained intense, but part of the demand was not for immediate use but in anticipation of needs by borrowers who wished to be certain their future requirements would be covered. Accordingly, measures were taken in May and early June to prevent the prevailing intense demand for credit from being reflected unduly in further tightening in the cost and availability of credit and further decline in the capitalization factors of the economy.

Open market purchases were made at that time in sufficient volume to reduce materially the volume of member bank borrowing at the Reserve banks. Reserve requirements were also reduced at midyear. Bank borrowing at the Reserve banks, which in the first four and a half months of the year averaged over 1 billion dollars, was reduced about one-half by the latter part of May and in June and July averaged just over 400 million dollars.

With this easing in the volume of member bank borrowing, both short-term and long-term interest rates declined sharply. By August, Treasury bill yields had declined to the discount rate. Comparable declines from June peaks occurred in yields on intermediate- and long-term securities.

Thereafter, as evidences of slackening in business activity began to accumulate, additional actions were taken to ease the credit situation. Over the summer and fall of 1953, in a succession of steps, credit became cheaper and more readily available. Member bank borrowing continued over the second half of 1953 to average between 400 and 500 million dollars. Interest rates declined steadily over the period.

In January, 1954, and in subsequent months, open market operations were so conducted as to eliminate virtually all member bank indebtedness at the Federal Reserve banks. This condition was maintained over most of 1954 by a succession of open market actions supplemented in midyear by another reduction in reserve requirements. In addition to the decline in borrowing, excess reserves of member banks rose somewhat through the spring and summer following a period of a number of months of stability at around 700 million dollars.

Reflecting the easy reserve position for member banks, market yields went down further and short-term government yields largely lost contact with the discount rate. Reductions were made in the discount rate in February and April, but these probably had little immediate impact in the market since the need for borrowing seemed remote for most banks. More recently, however, with the advent of seasonal credit and money pressures, borrowing has been somewhat more important, and short-term yields have moved into closer touch with the discount rate.

Throughout the year 1954, demand for long-term credit has continued very strong, reflecting large increases in mortgage and state and local government financing which more than offset some declines in the sales of new securities by corporations. Demand for short-term credit by businesses and consumers has been slack. Nevertheless, there has been a substantial growth in bank credit and deposits, due principally to expansion in bank holdings of US government and state and municipal securities.

Monetary Policy and Debt Management. For co-ordinated use of open market and discount policy in an anticyclical program, certain debt management conditions need to be fulfilled. Above all, open market operations must be free to operate as needed to offset changes in the various factors affecting bank reserves. Since there is no reason to believe that these operations will coincide in timing or direction with Treasury refinancing periods and considerations related to these activities, it is important that Treasury refinancing operations be handled in a way that will not require assistance from open market operations. By experimentation over the two or so years following the accord, refunding techniques were worked out that have proved highly successful from the standpoint of both the Treasury and the Federal Reserve.

The effectiveness of monetary policy is improved when the maturity of the federal debt is well spaced over a span of years. When this is the case, credit tightening and credit easing actions will be transmitted more smoothly and promptly throughout the credit market as these actions play on the liquidity of government security portfolios of banks and other lenders and investors. At the same time, however, there should be a large enough volume of short-term issues to support a broad money market adequate to meet the basic liquidity needs of the Federal Reserve System, banks, other financial institutions, and businesses. Progress has also been made in these aspects of debt management.

Summary. In summary, it may be said that during the past four years reserve banking techniques which were developed in this country during the twenties have been revived and used in more or less the fashion of that earlier period to achieve a contracyclical credit and monetary policy. In some respects, however, adaptations have been needed in these techniques to fit them into the present-day institutional framework. The major new fact in the postwar credit world is the existence of a huge government debt. Contrary to widespread fears, however, the size of the debt has not proved an unsurmountable obstacle to a flexible credit and monetary policy and in important respects has been helpful in implementing that policy. In general, the old instruments and principles of reserve banking have again proved themselves in use to be effective and sound.

DISCUSSION

ALBERT G. HART: Professor Robinson's paper affords a very interesting review of wartime debt management. As he points out, such a retrospect is useful above all as background for stand-by planning of economic mobilization—which like all such planning contemplates the possibility that a future war (which could start only because somebody hoped for a *Blitzkrieg*) might again become an endurance contest. Rather than blur the effect of his paper by dwelling on its rather minor flaws, I propose to sketch some of the contingent problems of any future war finance program.

Perhaps the chief lesson from the history of war finance is the tendency of a borrowing program to get diverted from its true aim of stimulating saving into the scoring of purely token triumphs, with distribution of kudos to the manipulators of empty symbols. Imposing bond subscriptions are financed painlessly (even with "free-riding" profit) by people who put themselves in funds by selling previously-bought securities to banks. Savings bond purchases are designed to pick up funds the buyers do not aim to spend, and the savings-stimulating effect of any underestimate of future spending is offset by easy redemption. Tax exemption is made available as a reward for switching assets. In any event (under the pattern of World War II), the incentive effect of tax exemption is frittered away. By failing to promise tax exemption but yet not setting up any efficient machinery to collect taxes on income from savings bonds, we managed to incur the costs of exemption *ex post*, without having it taken into account in savings decisions.

In any future inflationary squeeze (whether due to war or to the more probable event of another sudden bulge in defense spending short of war), a major handicap to policy will be the existing mass of liquid assets. We seem to have inflated ourselves up to the existing mass of currency and checking deposits. But either bond drives or compulsory lending programs will be ineffective if savings bond redemption and bond market support enable asset-switching to pass for saving.

Against this difficulty there are two possible remedies. One, as shown by Shaw and his associates, is to generate saving by drastic rationing of the privilege of spending, cutting existing liquid assets off from the market by some such device as issuing a new "ration currency" and freezing assets. If such a purchasing power rationing system were thoroughly applied, it might both generate the needed current savings and permit the immobilization of those savings. But the struggle to capture scarce goods by making side-payments out of supposedly immobilized assets would be intense. In enacting such a novel program, Congress would instinctively create major loopholes—particularly in a short-of-war situation.

The more orthodox remedy would be to overcome excess liquidity by monetary policy. To reduce the switching problem to manageable proportions, debt policy can work itself into a position where the bulk of nonbank holdings are in long-term marketable bonds. This can be done even in a recession if the authorities are willing to incur interest costs. True, to put more of a refunding into bonds has some deflationary effect; but not more than could

be offset by monetizing a minor fraction to give banks excess reserves. A mixture of bonds plus a little cash, in short, would be much the same in the short run as a mixture of notes and certificates. But in a future inflationary emergency, holders could be frozen into bonds much more effectively than into notes and certificates by permitting yields to rise somewhat. Thus the much-maligned policy of lengthening maturities, if carried out with resolution, could set the stage for a restrictive policy without having short-run restrictive effects.

In case an inflationary emergency develops, monetary policy could do much more than in World War II. In the nature of things, a rationing system would be necessary and would do much of the immediate job of generating savings. Monetary policy could help by encouraging installment buying of bonds (as distinct from lump-sum loans designed to be liquidated by selling the securities). Savings bonds could be given much less liquid redemption pledges. It would be much better after the emergency to have 50 billion dollars of genuine long-term bonds outstanding rather than 60 billions of nominally long-term bonds whose holders could and would redeem them freely. (Our so-called "ten-year" savings bonds have worked out on the average more like three-year notes.) Such a policy would involve higher interest costs and greater discounts on marketable bonds than that of World War II; but it would be much more conducive to saving during and after the emergency. If by any chance there were a tendency to stagnation later on, the government could always restore more liquidity than it had contracted for.

To use less polite language, debt management tends to find excuses for a pussyfooting policy. A good corrective for these excuses is Henry Simons' slogan that the justification of interest is to compensate people for accepting illiquidity, and it is never justifiable to give them interest and liquidity, too—at least *ex ante*. Let me urge more candor in dealing with these problems.

Turning to Mr. Youngdahl's paper, I am inclined to share in the rejoicing over the revival of monetary policy and to agree that Federal Reserve policy has been unusually successful since 1951. But a few reservations should perhaps be registered.

To begin with monetary policy in 1951, this looks better by hindsight than it did at the time. Chairman Martin, two months after the accord, still pictured inflationary pressure as critical in his statement before the House Banking and Currency Committee on the extension of credit powers granted in 1950; and six months after the accord, the *Federal Reserve Bulletin* was still insisting on the need of a "vigorous program of fiscal and credit restraint." Yet under the terms of the accord, the Federal Reserve did not stop increasing its holdings of government securities until October, 1951, and the deposit-supporting power of owned reserves (total reserves less rediscounts) was 5 per cent greater in the fourth than in the first quarter of 1951. Certainly the Federal Reserve was doing at least fairly well by any standards. But speaking as an outsider, without benefit of the files Mr. Youngdahl has evidently reviewed, it seems to me that the Federal Reserve was running a risk, and that

an ideal monetary policy would not have ridden with quite so slack a rein.

By the test of the deposit-supporting power of owned reserves, the Federal Reserve began to tighten in 1952 only in the autumn and turned around in mid-May of 1953. Since the downturn of 1953 is pretty clearly dated in July, this record is a little too good as a response to the weakening of business; and inventories (one of the major criteria invoked by Mr. Youngdahl) were still rising fast. As Mr. Youngdahl intimates, the Federal Reserve was reacting to money market symptoms more than to the general situation. After refusing for some months to respond to the Federal Reserve's mild credit restriction measures, most interest rates began to firm in March—presumably because of uncertainty induced by Treasury financing—and the suddenness of the rise made it natural to ease the market.

As I remarked in relation to Mr. Robinson's paper, I feel some regret that the Treasury and Federal Reserve backed off so easily from the program of lengthening debt maturities. It might have been better to have eased the market enough earlier than mid-May to give the new bond issue a favorable launching and to have taken the occasion to feel out the effect of substituting a mixture of bonds and cash for intermediate-length maturities.

Mr. Youngdahl is scrupulous not to claim too much for monetary policy in relation to the recession that started in 1953. This is a case where it would be easy to prove too much. The Federal Reserve took measures calculated to lower the cost and increase the availability of credit. In the field where we have most to hope from lower rates of interest and relaxed capital rationing—namely, construction—a powerful support for activity has developed. The effect seems a bit too dramatic for the apparent cause. The fact that an increasing share of the flow of savings is channeled through institutions that insist on placing it in securities and mortgages must be a factor in the situation. But it is no mere coincidence that easy money has been accompanied by a strong security market, facilitating corporate and municipal flotations, and pushing institutional investors toward mortgages. Monetary policy may not be the source of all economic blessings; but it is proving more helpful than most of us would have guessed a few years ago—undoubtedly largely because of skillful handling.

EARL R. ROLPH: Professor Robinson has traced for us some of the patterns of war finance that have recurred through the past three major conflicts this country has waged. War results in substantial increases in government expenditures for goods and services and in large deficits. Together these create the inflation potential that explodes into large increases in private expenditures and, except during World War II, into large increases in prices. The government's large financial requirements during wartime lead to an apparent shortage of money—a situation which Secretaries of the Treasury manage to overcome by technically brilliant financial maneuvers. These maneuvers have taken different forms but they are alike in resulting in large increases in the amount of money in private hands. Secretary Chase was driven to the "unsound" procedure of openly printing greenbacks. So ashamed was he of this breach of financial decorum that later, as Supreme

Court justice, he declared his own actions unconstitutional. Nothing so embarrassing was required of the Secretaries during the last two great wars. Money was created, of course, and on a large scale. The Treasury first printed the debt and pursuaded others to create the money.

A recurring theme of war finance stressed by Professor Robinson is the adoption of the tactics of high-pressure salesmanship to sell the debt. Chase was clearly outclassed by Glass and Morgenthau on this score, not that he was niggardly in his efforts. Here we find no great change in the economic ideas of Treasury officials. Success of a bond campaign was judged and is still judged by the test of the volume of business. Failure means that some of the Treasury's merchandise remains on the shelves. So strong was this conception of success that, during World War II, the Treasury consistently underpriced its securities, creating a spread between the sales prices and the support price. The enterprising souls who stood in line to accept these gifts from the Treasury were called "free-riders"; they were also called other names.

The condensed survey that Professor Robinson has just related reveals some change in official ideas, too. In every major war in this and other countries, finance officials have been plagued by the reluctance of some people to buy bonds immediately, preferring to wait for what they hoped would be a better deal. Whether officials should allow this consideration to bother them is a nice question that has a definite answer only if it is assumed that the sale of more debt is better than the sale of less. The corrective technique discovered was, as everyone knows, the practice of having the central bank support the prices of government securities. Secretary Chase had no central bank to perform this operation for him, and he thereby probably spared the country from even more inflation than did occur. Secretary Glass had recourse to the device of supporting the prices of earlier issues by fixing their rates of conversion into later issues—a somewhat clumsy technique even in the most skillful hands.

The fundamental question of debt management during wartime under modern conditions concerns what economic function it is supposed to perform. Finance officials have been almost obessed with the idea of raising money. But of course a sovereign government always has access to unlimited financial means. Presumably the economic purpose is to force or persuade people to buy debt instead of buying goods and services. But the apparatus of direct controls has made this purpose largely obsolete. Scarcely anyone can be found who believes that the 2 per cent yield of World War II made any important difference. In any event, there exists a much simpler method of preventing private borrowers from taking advantage of low rates of interest. A tax could be imposed on borrowers in amounts equal to, say, 10 per cent of the proceeds of any loan and collectible at the source by the Federal Reserve banks. We can have the advantages of a 10 per cent war without the Treasury troubling itself to sell debt at all. The gains of such a system are many. Not the least would be that of sparing the country the exhortations of bond salesmen. Another would be the reduction in the vast amount of paper work involved in turning the Treasury into a savings bank. Professor

Robinson apparently takes the view that debt management has a role to play in an all-out military effort. I think he is mistaken. The best system of debt management during such a period is no debt management at all. If it is objected that the resulting flood of money would create postwar problems, the answer is that there exists no method, except much heavier taxation than any country has seen fit to impose, to avoid postwar problems. With no wartime debt management, it is unlikely that we would again handcuff ourselves because of fears of postwar deflations.

Mr. Youngdahl has presented an interesting abbreviated history of monetary policy since the accord. I find myself somewhat out of sympathy with the tone of Mr. Youngdahl's remarks—I mean with his interpretations of the evidence, not with his account of the evidence. I detect a note of misplaced complacency. We are told that "the old instruments and principles of reserve banking have again proved themselves in use to be effective and sound." The word "again" should be sufficient to give anyone pause. When was it that the old instruments proved themselves so effective? The history of the Federal Reserve is not one of striking successes. Even if we limit our attention to the postwar era, there have been failures. It should be recalled that it was the Federal Reserve officials who decided to offset the deflationary effects of the Treasury surplus during the immediate postwar years. It is customary but not convincing to place the responsibility for these policies solely upon the Treasury. The policy of supporting the structure of yields on federal securities was vigorously supported by officials of the Federal Reserve System before Congressional committees and in the public print. Not even the Treasury can be charged with the incredible policy of supporting the bond market in reverse during the recession of 1949. But let us be careful about blaming anyone but ourselves—professional economists—for these policies. We failed to speak with a clear voice when it mattered.

The restoration of the old tools of monetary management since the accord have consisted, according to Mr. Youngdahl's account, of restrictions on bank borrowing, supplemented by open market operations and changes in legal reserve requirements. Instead of supporting the prices of Treasury obligations, the Federal Reserve has supported the prices of bankers' obligations. To a simple-minded person like myself, there seems to be no vast difference between these and former practices. Let us be clear that bank borrowing from the Reserve banks is inflationary; it is another way that banks obtain reserves. Youngdahl stresses the point that banks are reluctant to borrow. When circumstances beyond their control force them into debt, they stop increasing, or is it that they merely increase less rapidly, the quantity of money in private hands. No doubt it is true that banks are reluctant to borrow, but like many ordinary persons, bankers allow their reluctance to be overcome by more attractive alternatives. Mr. Youngdahl has pointed out that the yields on some classes of short-term securities move together with the discount rate. This fact suggests that the reluctance to borrow is conditional; it depends upon the foregone gain of other methods of obtaining reserves. The technique of relying upon some automatic tendency on the part of banks to adjust their asset positions to the level of their indebtedness seems to me to be

dangerous. It permits the public and the banks to determine monetary policy, and this, I think, is the fundamental objection to the earlier policy of supporting the government bond market. The elimination altogether of the privilege of banks to borrow from the Reserve banks seems to me to be a desirable reform.

During the mild recession we have been experiencing, the Reserve officials have not exhibited the aggressiveness that someone who believes in vigorous monetary policy would like to see. One could even debate whether the Federal Reserve System has allowed the expansionary effects of the federal deficit to exercise its full influence on private expenditures during the period since the fall of 1953, when the evidence was reasonably clear that the economy was in a decline. Imagine, if you will, what the situation might have been if the Treasury had possessed a very large cash balance at that time and had, instead of selling debt, allowed its cash balance to fall by the amount of the deficit. Imagine further that the Federal Reserve had simply allowed this cash to flow into the hands of the public, leaving reserve requirements and the discount rate unchanged, and had engaged in no open market operations. Such a policy is neutral in the sense that monetary management exerts no independent influence on private expenditures. This criterion of neutrality rests upon the belief that the Federal Reserve cannot take responsibility for the automatic flexibility of the tax system nor for other features of fiscal policy. Would the effect on the spending behavior of private groups have been less stimulating in this event than that of the policies actually pursued? From this point of view, it is generous, I think, to hold that the Federal Reserve has adopted even a neutral policy to offset the recession. It is true to be sure that the policies adopted have not been as restrictive in fact as they might have been.

On the subject of economic stabilization, there is nothing much more dangerous than to suppose that we have at long last in this country a body of monetary and fiscal devices adequate to the task of preventing large swings in private expenditures. We do not as yet have even a deliberate synchronization of fiscal and monetary policies. Mr. Youngdahl notwithstanding, the old principles of monetary management are no longer good enough.

TAXATION AND INCOME DISTRIBUTION

THE DIFFERENTIAL TAX BURDEN ON STOCKHOLDERS

By DANIEL M. HOLLAND
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Introduction

This is a report on some of the findings of a study I have been undertaking at the National Bureau of Economic Research.[1] I shall place major emphasis on the conceptual framework of my project and its findings; the discussion of assumptions and procedures will, perforce, be cursory, but I must warn you that the findings are highly qualified—valid only within the premises and techniques used in their derivation. An analysis of our method and its limitations will be found in the full report now in preparation at the Bureau. Undoubtedly, too, many of the qualifications will be brought out by the discussants. The primary limitation, however—our assumption that the corporation income tax is not shifted—deserves specific mention at the outset.

My investigation is concerned with estimating the income tax load on stockholders and the degree of differential taxation to which they have been subject. This is a matter that aroused great controversy and about which there appeared to be much misunderstanding in the discussions of the relief provision of the Internal Revenue Code of 1954.[2]

[1] This study was initiated under the guidance of the late Robert Murray Haig, to whom I am deeply grateful for both advice and encouragement. It has been the beneficiary of searching critical readings, at one stage or another, by Arthur F. Burns, Lawrence Seltzer, Carl Shoup, Richard B. Goode, W. Leonard Crum, Geoffrey Moore, Richard Slitor, and Thor Hultgren. In addition an earlier version of the study has been read by Leo Wolman, Simon Kuznets, Dan Smith, Gerhard Colm, William S. Vickrey, A. G. Hart, C. Lowell Harriss, Maurice E. Peloubet, Marshall Robinson, and C. Harry Kahn. To these men my deepest thanks. It should be understood that this does not mean to imply that they supported my point of view or my procedures. Quite the contrary in a number of cases. Nor do I mean to "socialize" the blame. All errors and oversights are mine alone. For helpful advice and computation assistance in the earlier stages of the project, I wish to thank Bella Shapiro, Gloria Moskowitz, and Fred Stewart. Finally I am most grateful for the critical insight of Doris Eiseman and Mary Ann Zimmerman, who, in addition to doing most of the computations, helped me clarify a number of conceptual problems.

[2] The author does not claim, of course, either to have cleared up all the ambiguities or difficulties of this problem or to have blazed a trail in isolating the relevant considerations. A number of investigators have worked in this area. Among the most substantial contributions are: Richard B. Goode, *The Corporation Income Tax* (John Wiley and Sons, 1951) and *The Postwar Corporation Tax Structure* (Treasury Department, Division of Tax Research, 1946); W. L. Crum, "The Taxation of Stockholders," *Quart. Jour. Econ.* (February, 1950); also, the *Preliminary Report* and *The Final Report* of the Committee on the Federal Corporate Net Income Tax, Harold M. Groves, Chairman. The *Preliminary Report* was made in 1949 and the *Final Report* in 1950 to the National Tax Association, which had established the Committee, at the Association's annual meetings.

How Heavily Are Stockholders Taxed?

While it is generally agreed, if the incidence of the corporation income tax is taken to be on profits, that stockholders compared with other persons are subject to a differential tax load, there are divergent points of view about the nature and relative weight of this differential burden. Some critics, placing the emphasis on distributed corporate earnings, point to the fact that they are taxed first at the corporation level when earned and again at the personal level when distributed and contend, therefore, that this source of income is "overtaxed." This was one of the reasons cited by President Eisenhower for the relief provision recently incorporated in the Internal Revenue Code of 1954. (*Congressional Record*, Volume 100, No. 11, Eighty-third Congress, 2nd Session, page 549.)

Others, however, concentrating on the undistributed segment of corporate earnings emphasize that this income source escapes its fair share of taxation, because it is not subject to the high marginal rates of personal income tax that would have applied had it been distributed to stockholders. Such was the emphasis of President Roosevelt's initial proposal of an undistributed profits tax in 1936. (*Congressional Record*, Volume 80, Part 3, Seventy-fourth Congress, 2nd Session, page 3146.)

Clearly, the considerations raised by both points of view are relevant in the determination of the differential tax load on stockholders; the final answer rests on the relative strength of these two factors. Either contention—i.e., primarily overtaxation or primarily undertaxation—can be proved by an appropriate set of illustrative figures. This being the case, it seemed sensible to use the most realistic data available: the data of *Statistics of Income*, the annual Bureau of Internal Revenue tabulations from tax returns.

Our Procedure

Basic to our "standard" method is the assumption that the corporation income tax rests on profits. While no unanimity of opinon exists on this score, the view that the tax is not shifted, particularly in the short run, is still most widely held. Moreover, it is the view germane to any contention that stockholders are differentially taxed. (Not to be dogmatic, however, we present below the results from an alternative calculation that assumes half the tax is shifted.)

Central to our procedure, also, is the assumption that in all income classes, stockholders' share of corporate earnings before tax can be determined by a proportionate allocation based on dividend receipts. So from the aggregate data on pretax corporate earnings we can obtain a ratio by which to "blow up" dividend receipts to pro rata shares of net corporate earnings. (Hereafter, unless otherwise specified, the

phrase net corporate earnings refers to corporate income before taxes; i.e., the sum of dividends, corporate savings, and corporation income taxes with corporate savings taken net of deficits.)

Then from a cross-classification for dividend recipients (stockholders) of income class and dividend size class we break up the array of stockholders into some 225 cells. For example, from this array we get an entry for stockholders with adjusted gross income of $4,000 and under $5,000 with dividend receipts of more than zero and less than $100, another for those at this same income level with dividend receipts of between $100 and under $200, etc. To the average amount of dividends in each of these cells we apply the corporate earning multiplier and add this product to stockholders' average adjusted gross income. We call the result imputed gross income. It includes all of net corporate earnings rather than dividends as the measure of personal income from corporate activity.

Next, stockholders and the data relevant to them are rearrayed in new income classes based on the size of their imputed gross income. We strike averages for these classes, plot them, and obtain a schedule from which to read off the relevant data for a number of selected imputed gross income levels from $1,000 to $500,000. To measure the degree of differential taxation we break down net corporate earnings into two components: earnings for distribution (i.e., the sum that had to be earned, in view of the corporate tax, to enable the actual amount of corporate dividend payments) and earnings for retention defined, similarly, as the pretax counterpart of retained earnings.

We address ourselves first to the double taxation of distributed earnings (earnings for distribution). In this connection the "extra burden" on stockholders is measured by the excess of the sum of the corporate tax on earnings for distribution and the personal tax on dividends over the potential personal income tax on earnings for distribution. The extra burden expressed as a fraction of its base we call the differential, and in this case it is equal to $C\ (1 - P)$, where C equals the corporate rate and P the relevant personal marginal rate. Since P rises with stockholders' income but never reaches 100 per cent, the differential against the earnings for distribution segment of the stockholder's share of net corporate earnings falls as the level of stockholder income rises, but always remains positive.

But this is only part of the story. What about the undistributed segment? Here the extra burden equals the corporate tax on earnings for retention minus the potential personal tax on this same base. Expressed as a fraction of earnings for retention this comes to $C - P$. (This P is, of course, higher than the P in the earnings for distribution formula; the C is, also, because earnings for retention are taken net

of deficits.) In this case, depending on the relative heights of C and P, the differential can be positive, negative, or zero. With C invariant with stockholders' income and P a rising function thereof, the differential will decline as stockholder income rises, and after a point, if P is high enough it will (and in most of the years of the decade of the forties did) become negative, turning into a tax "benefit."

A combination of the two measures we have just discussed provides the composite or net result: the differential against net corporate earnings, which is a weighted average of the differential against each of the components of net corporate earnings.

Let D be earnings for distribution and R earnings for retention. Then the differential against net corporate earnings equals:

$$C\ (1-P)\frac{(\ D\ )}{(D+R)} + (C-P)\frac{(\ R\ )}{(D+R)}$$

It will be a declining function of stockholders' income. Further, after a point the differential against earnings for retention can (and in most years of the last decade did) weigh so heavily that the extra burden on net corporate earnings will become negative. In other words, at incomes above some high level an income tax differential in favor of net corporate earnings will prevail.

By this measure we can answer the question: How much more (or less) heavily were corporate earnings actually taxed than they would have been had they been subject in full to the personal income tax alone?

We have used one more measure in our analysis. By relating the extra burden to the total income of stockholders, we obtain the differential against (or in favor of) stockholders. With this measure we answer the question: How much more heavily, measured in terms of effective rates, were stockholders actually taxed by the combined corporate-personal income tax system than they would have been with the corporate tax abolished and their pro rata share of net corporate earnings subject fully and promptly to the personal income tax?

Let O be stockholders income from sources other than corporate earnings, then the differential against stockholders equals:

$$C\ (1-P)\frac{(\ D\ )}{(D+R+O)} + (C-P)\frac{(\ R\ )}{(D+R+O)}$$

One further point about our measures. It can be argued that some portion, at least, of retained earnings would show up as capital gains, and that some of these gains, in turn, would be realized in taxable form, and thus sometime in the future an additional personal income

tax liability would be incurred. In measuring the actual tax load on earnings for retention, therefore, we added a term representing the estimated present value of this future capital gains tax liability. Needless to say, the adjustment is not very precise. In any event, it is relatively minor.

So much for the conceptual framework. Now what have we found out?

The Findings for a Selected Year—1950[3]

Our basic data—the tabulations in *Statistics of Income*—come out with a lag of several years; 1950 is as contemporaneous as we can get. How heavy was the differential taxation of net earnings and stockholders in this year? The answer is summarized on Chart I.

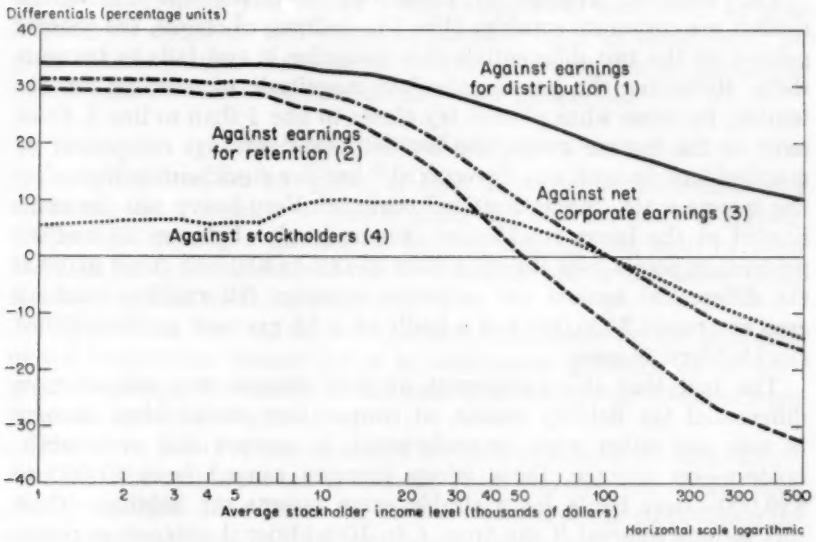

CHART I. DIFFERENTIALS, 1950

From an examination of line 1 it appears that the double taxation of distributed earnings was substantial. At the bottom of the taxable stockholder income scale, for example, earnings made for distribution to stockholders were subject to a tax liability over 34 percentage points higher than would have been their due under the personal income tax

[3] The findings presented here and in what follows are preliminary. They are the result of numerous calculations all of which have not yet been checked. However, it is unlikely that the arithmetic errors that will be uncovered will change our results substantially. The entries in Chart I and Tables 1, 2, and 5 are weighted averages of the data for joint and separate returns.

alone. At the $25,000 stockholder income level the net extra burden averaged about 29 percentage points, and at the top of the stockholder income range plotted on the chart ($500,000) it came to 10 per cent. The higher the stockholder income level the lower the differential against earnings for distribution.

While the differential against earnings for retention follows the same general pattern, the magnitudes involved are smaller and its fall is sharper. This is most apparent over the upper portion of the stockholder income array. Starting at 30 per cent for the lowest income stockholders, it fell very rapidly, so that by the $25,000 mark it was only 15 per cent; the fall continued, the burden changed to a benefit, and at the top of the stockholder income scale we found a differential of −33 per cent.

The weighted average of these two measures—the differential against net corporate earnings (line 3)—follows, of course, the general pattern of the two differentials that comprise it and falls in between them. Reflecting the greater absolute magnitude of earnings for retention, its value when plotted lay closer to line 2 than to line 1. Over most of the income range, the net corporate earnings component of stockholders' income was "overtaxed," but for stockholders higher up the income scale, "undertaxation" occurred. Very heavy was the extra burden at the lower stockholder income levels—between 31 and 29 percentage points over the span from $1,000 to $10,000. After $10,000 the differential against net corporate earnings fell rapidly, reaching zero at around $100,000 and a nadir of −16 per cent at $500,000 of stockholders' income.

The fact that this component of their income was subject to a differential tax liability means, of course, that stockholders' income *in toto* was either over- or undertaxed. It appears that most stockholders—on average, those whose incomes ranged from $1,000 to $50,000—were liable for a sizable extra income tax liability. (Over this income interval it ran from 6 to 10 additional percentage points of tax.) But as we examine the stockholders near the top of the income scale a different picture emerges, for the differential declined very rapidly after the $50,000 point. Again, as an average result, stockholders with incomes higher than $100,000 enjoyed a tax benefit that became relatively more important the larger their income.

Our findings are for average stockholders; so we have no way of telling precisely how many fell in the over- and undertaxed categories. For 1950, however, we can furnish a reasonable figure on this score, because we had to examine the tax liability in each stockholder cell for our "partnership" method revenue estimate reported on below. Of the 3.4 million double-taxed stockholders, about 3.3 million, or 97

per cent, paid a higher combined corporate-personal income tax liability than would have been due under the personal income tax alone; they were, therefore, overtaxed. On the other hand, some 3 per cent, about 100,000, were undertaxed. While small as a proportion of all stockholders, the undertaxed group assumes greater importance when we measure its share of all double-taxed net corporate earnings. Here, because the distribution of net corporate earnings is highly concentrated, we found that about 46 per cent of net corporate earnings imputable to personal income taxpaying dividend recipients was undertaxed. (In deriving these figures we did not make the future capital gains tax liability adjustment alluded to earlier. However, this does not seriously affect their relevance for the purpose at hand.)

The same measures elaborated for 1950 have been undertaken for all the intervening years since 1940, except 1942 and 1943. It goes beyond the scope of this paper to examine them in detail here. In general their pattern is similar to the fifties, but, of course, the height of the differentials varied considerably over the period.

Two Alternative Assumptions

Our procedure, we recognize, is open to numerous objections and criticisms. At a number of points along the line we made a specific choice from among a number of possible assumptions and, in some cases, at least, strong support can be found for one or another of those alternatives that we failed to select.

In particular, had we chosen a variant assumption on the incidence of the corporation income tax or an alternative definition of net corporate earnings, our results would have been noticeably different from those we have just reported on.

Therefore, for 1947, we measured the differentials on the assumption that half the corporate tax is shifted. From this test one simple conclusion emerges. (See Table 1.) To the extent that the corporation income tax is shifted, our findings based on the standard assumption involve an overstatement of the extra burden on net corporate earnings and stockholders.

Similar accounting conventions are used in defining income for the personal and corporation income taxes. Since we are comparing the actual tax of one system with the potential tax of the other, we did not feel it necessary conceptually to redefine income to take account of the effects of rapid and sharp increases in the price level. However, as a matter of some interest and out of deference to a substantial body of opinion which holds that the BIR definition of income is seriously inappropriate when price levels change rapidly, we undertook an additional alternative calculation for purposes of which we lowered the

TABLE 1

COMPARISON OF DIFFERENTIALS UNDER STANDARD METHOD AND FIFTY PER CENT SHIFTING ASSUMPTION, 1947

Imputed Gross Income Level (000)	Differential Against Net Corporate Earnings			Differential Against Stockholders		
	Standard Method	Shifting Assumption	2–1	Standard Method	Shifting Assumption	5–4
	1	2	3	4	5	6
$ 1.....	24.0	11.2	−12.8	5.9	2.4	−3.5
3.....	23.8	10.9	−12.9	4.8	2.6	−2.2
5.....	22.4	9.6	−12.8	5.2	2.4	−2.8
10.....	17.9	4.8	−13.1	6.5	1.6	−4.9
25.....	2.0	−10.0	−12.0	1.0	− 4.5	−5.5
50.....	− 7.6	−18.1	−10.5	− 4.8	−10.9	−6.1
100.....	−16.2	−26.3	−10.1	−11.9	−18.4	−6.5
250.....	−24.5	−33.9	− 9.4	−19.7	−26.6	−6.9
500.....	−25.8	−34.9	− 9.1	−23.0	−31.3	−8.3

total of corporate earnings imputed to stockholders by the inventory valuation adjustment and the estimated excess of current over historical cost depreciation. The results are summarized and compared with our standard method in Table 2.

Our conclusion from this test is that for years of rising price levels, if it is considered sensible and relevant to allow for current costs of inventory maintenance and depreciable asset replacement in defining taxable income, our standard case understates the differential against

TABLE 2

COMPARISON OF DIFFERENTIALS UNDER STANDARD METHOD AND CURRENT COST ASSUMPTION, 1947

Imputed Gross Income Level (000)	Differential Against Net Corporate Earnings			Differential Against Stockholders		
	Standard Method	Current Cost Assumption	2–1	Standard Method	Current Cost Assumption	5–4
	1	2	3	4	5	6
$ 1.....	24.0	39.7	15.7	5.9	8.1	2.2
3.....	23.8	39.4	15.6	4.8	9.8	5.0
5.....	22.4	38.0	15.6	5.2	9.4	4.2
10.....	17.9	33.4	15.5	6.5	11.0	4.5
25.....	2.0	18.3	16.3	1.0	7.3	6.3
50.....	− 7.6	10.0	17.6	− 4.8	5.5	10.3
100.....	−16.2	1.6	17.8	−11.9	1.0	12.9
250.....	−24.5	−5.3	19.2	−19.7	−4.2	15.5
500.....	−25.8	−5.6	20.2	−23.0	−5.2	17.8

stockholders and net corporate earnings. But the basic pattern of our findings remains. Under the current cost alternative, too, the differentials fall as stockholders' income rises, and, after a point, they turn in favor of net corporate earnings and stockholders.

Revenue Aspects of the "Partnership" Method

Were shareholders to be taxed on their corporate earnings as members of a partnership are taxed on their share of its income, the corporate entity as such would drop out of the picture. Since stockholders would be held accountable under the personal income tax for their pro rata share of corporate earnings, all such earnings would be promptly and fully called to account and subject to only one income tax. Over, under, or unequal taxation of corporate earnings would cease to exist. In what follows it should be clearly understood that we neither support nor condemn the partnership method of taxing corporate earnings, nor do we raise the question as to whether it is in fact a practical alternative. (Some students who admit its desirability in principle question its administrative feasibility.) In addition, we assume, as we have for the most part earlier, that the incidence of the corporation income tax is on profits, and that the same ratio of dividends to net corporate earnings characterizes each of our income class dividend size cells.

For several years—1947, 1949, and 1950—we have estimated the revenue loss or, possibly, gain that would ensue upon the institution of the partnership method. These estimates are of interest in and of themselves, for in a period of heavy federal government revenue requirements, among the relevant criteria for evaluating a proposed change in the tax structure is its revenue effect. More particularly, in the context of our study, these estimates constitute a measure of the aggregate extent of the unequal taxation of stockholders, thereby complementing the differentials which are average measures of experience at selected income levels and, therefore, furnish no idea of the total amount of differential taxation of all double-taxed stockholders.

Our starting point in these estimates was to consider the corporate tax abolished and the revenue it raised lost to the Treasury. How far would the increased personal income tax collections due to full imputation of net corporate earnings go toward recouping this revenue loss? To answer this question we estimated for each of our stockholder cells (i.e., the cells in the cross-classification of income and dividend size class) the increase in personal income tax liability that would follow full imputation of stockholders' pro rata share of net corporate

earnings. (Numerous details are involved in our procedure. All we do here is present its basic features.) We did the same for taxable estates and trusts. The difference between the corporate tax liability sacrificed by shifting to the partnership method and the increase in personal income tax liability constitutes the net revenue loss on a current basis; i.e., that portion of the revenue loss that would occur in the year of the shift to the partnership method.

But this is not the end of the story. A further loss in revenue is to be expected. For, following the treatment now accorded partnership shares, the basis of valuation for the purposes of determining capital gains would be reduced by the full amount of imputed retained earnings. On this score, future capital gains and the tax liability thereon would be lower because of the switch to the partnership method. It goes without saying that the lack of any real evidence germane to this item causes this portion of our estimate to be characterized by a much lower order of accuracy than the current revenue loss figures.

Quite varied are the results for our selected years (see Table 3). In 1947, for example, the Treasury would probably have suffered no revenue loss by a shift to the partnership method, currently. Over the indefinite future, however, tax collections would have totaled between 1 and 1.5 billion dollars less because of the imputed retained earnings of 1947. In 1949, a more sizable current revenue loss would have developed. For 1950, our estimate of the loss runs at a still higher level. The current revenue loss of around 3.5 billions represents a little under 10 per cent of total corporate and personal income tax liabilities. (As tabulated in *Statistics of Income,* the corporation income tax liability in 1950 came to 17.3 billion and personal to 18.6 billions.) To recoup it, something like a 3.2 percentage point upward shift in the personal income tax schedule would have been necessary. Moreover, we estimate a future revenue loss of around 1.4 billion dollars. For 1953, a more substantial revenue loss is indicated: 5.7 billions currently and 1 billion more in the future. To maintain the estimated 1953 level of tax receipts, a rise of four points in personal income tax rates would have been required.

The difference in the revenue loss among these several years is to be explained by variations in the relative heights of the corporate and personal income tax, the magnitude of net corporate earnings, and the proportions represented by dividends and retained earnings.

It seems safe to conclude that only in a prosperous year, with the corporate rate under 40 per cent, the personal rate structure effectively as high as it was in 1947, and a high proportion of corporate earnings retained, would a switch to the partnership method not involve a current loss of revenue to the Treasury.

TABLE 3

PARTNERSHIP METHOD REVENUE LOSS*

(Dollar Amounts in Billions)

Year	Current	Future	Total	Percentage Point Rise in Tax Rates Required to Recoup Current Revenue Loss
1947.....	0.2†	1.2	1.0	−0.2
1949.....	2.2	0.9	3.1	2.5
1950.....	3.5	1.4	4.9	3.2
1953.....	5.7	1.1	6.8	4.0

* The 1953 values are considerably less reliable than those for the other three years, for no detailed data pertaining to this year are yet available. Despite this, however, the figures appear in the table because of the interest that attaches to the most recent experience.

† Revenue gain.

The Aggregate Extra Burden

The data assembled in connection with the partnership method revenue loss estimates bear on another problem discussed earlier in this paper. We have already examined the net extra burden of the corporation income tax on average stockholders at selected income levels. Now we can approach it somewhat differently, concentrating on aggregates rather than averages. How heavy an extra burden did the corporation income tax represent in the aggregate? In what follows, this query is answered only in terms of what we have called above the current increase or decrease in tax liability under the partnership method. Unlike the differentials presented earlier, these data take no account of the future capital gains tax liability.

Table 4 presents the relevant information. In every year analyzed, for stockholders in the upper portion of the income array, the partnership method would have meant a heavier tax liability than they actually were subject to. These stockholders, then, in the aggregate, even though double-taxed, were undertaxed. On the other hand, stockholders in the lower portion of the distribution were overtaxed. Had a switch to the partnership method been made, their income tax liability would have been lower. By far the larger portion of stockholders were overtaxed, but a not insignificant number in some classes were undertaxed. (See column 6.) Much larger, however, is the proportion of net corporate earnings falling in the undertaxed category. In 1950, for example, as already noted, this came to about 46 per cent.

The Relief Provisions of the Internal Revenue Code of 1954

Very recently what has been described by its proponents as a

TABLE 4

A COMPARISON OF THE TAX LIABILITY OF STOCKHOLDERS UNDER THE CORPORATION-PERSONAL INCOME TAX SYSTEM AND THE PARTNERSHIP METHOD, 1947, 1949 AND 1950*

Imputed Gross Income Class (thousands)	Tax Liability (million)		3 as a Percentage of 2	Number of Stockholders with Partnership Method Liability Higher than Corporate-Personal Income Tax Liability	Percentage of Total Returns in Class "Undertaxed"
	Corporate-Personal Tax System	Partnership Method			
1	2	3	4	5	6
			1947		
Under $2	$ 57.7	$ 36.7	64	—	—
2 under 4	470.7	314.4	67	—	—
4 under 5	253.8	184.3	73	—	—
5 under 7	600.1	409.9	68	—	—
7 under 10	749.1	542.9	72	—	—
10 under 25	2,607.4	2,248.8	86	—	—
25 under 50	2,342.9	2,468.5	105	134,311	79.8
50 under 100	2,588.4	3,243.0	125	75,718	100.0
100 under 500	2,938.2	4,177.6	142	28,955	100.0
500 under 1,000	668.4	1,045.3	156	1,791	100.0
1,000 and over	808.7	1,282.4	159	649	100.0
Total	14,085.4	15,953.8	113	241,424	7.4
			1949		
Under $2	27.0	14.9	55	—	—
2 under 4	278.8	171.6	62	—	—
4 under 5	233.4	148.0	63	—	—
5 under 7	634.5	400.3	63	—	—
7 under 10	719.4	449.6	62	—	—
10 under 25	2,415.9	1,664.4	69	—	—
25 under 50	1,665.6	1,350.8	81	10,219	6.5
50 under 100	2,257.9	2,114.4	94	28,378	32.2
100 under 500	3,010.8	3,482.1	116	37,455	99.5
500 under 1,000	705.9	942.1	133	2,321	100.0
1,000 and over	857.5	1,148.3	134	801	100.0
Total	12,806.5	11,886.3	93	79,174	2.5
			1950		
Under $2	23.3	13.2	57	—	—
2 under 4	230.8	135.0	58	—	—
4 under 5	297.9	181.0	61	—	—
5 under 7	738.3	474.9	64	—	—
7 under 10	706.4	398.9	56	—	—
10 under 25	3,102.1	2,028.9	65	—	—
25 under 50	3,002.9	2,365.7	79	12,252	5.2
50 under 100	3,776.3	3,660.5	97	37,945	33.3
100 under 500	5,192.7	6,150.6	118	50,788	99.8
500 under 1,000	1,241.2	1,683.0	136	3,149	100.0
1,000 and over	1,747.0	2,328.6	133	1,213	100.0
Total	20,058.9	19,420.3	97	105,347	3.2

* The fact that in 1947 taxable dividend recipients would have probably had a higher income tax liability under the partnership method than under the combined corporate personal income tax system is not inconsistent with our findings that, on a current basis, no increase in revenue would have accompanied a change to the partnership method. Nor are the virtually identical tax liabilities for taxable stockholders under the two systems reported in Table 4 for 1950 a contradiction of our conclusion that in that year a sizable revenue loss would have occurred immediately upon the introduction of the partnership method. For not all dividends show up on taxable returns. For these two years, we were able to trace only between 82 and 83 per cent of net corporate dividend payments to the taxable returns of individuals and estates and trusts. For a variety of reasons—provisions of the tax law, oversight, dishonesty, etc.—the remaining 18 per cent does not get into the personal income tax mill. So the partnership method tax base was only 82 per cent of the corporate tax base.

"significant step in the right direction"[4] has been taken to afford stockholders some relief from the double taxation of dividends. Under the provisions of the Internal Revenue Code of 1954, the first $50 of dividends received from domestic corporations (the first $100 for joint returns) is excluded from taxable income, and further, stockholders are given a credit against personal income tax equal to 4 per cent of the remainder of their dividend receipts. (Internal Revenue Code of 1954, Public Law 591, Chapter 736, Sections 34 and 116. This is considerably less generous than President Eisenhower's original proposal which called for exclusions double the size finally settled on and a tax credit ultimately to reach 15 per cent.)

Thus relief is provided only for distributed earnings and it therefore deals with only a portion of the problem of the differential taxation of corporate earnings. (Apparently, also, no relief is afforded dividend recipients who are not subject to the personal income tax.) But even when viewed more narrowly within their own framework, i.e., from the point of view of distributed earnings only, these relief provisions do not appear to be a particularly satisfactory "step in the right direction."

We have pointed out above that as regards earnings made for distribution to stockholders, the corporation income tax constitutes a net extra burden equal to $C\ (1 - P)$. The higher the stockholders' income, the lower the extra burden of the corporation income tax. The relief provisions of the Internal Revenue Code of 1954, however, are not geared to this relation. The tax credit, by providing a constant amount of relief at all income levels, operates in such a way that a greater degree of relief is obtained the higher the stockholders' income level, and the relief provided by the exclusion is a rising function of stockholders' income.

More specifically, the credit is equal to 4 per cent of dividends. Since the extra burden falls as stockholder income increases and the credit is the same absolute amount per dollar of dividends at all income levels, the higher the stockholders' income, the greater the proportion of the extra burden removed by the tax credit. As for the exclusion, the relief it affords ranges under 1954 rates from 20 cents to 91 cents per dollar of dividends, the amount of relief increasing with the marginal rate of personal income tax.

So far our discussion of the relief provisions has run in terms of marginal dollars of earnings for distribution. The over-all result, however, is a more complicated story, because it depends on how heavily each relief provision is weighted in the total relief granted, and this,

[4] Remarks by Marion B. Folsom, Under Secretary of the Treasury, before the American Management Association, Hotel Statler, New York City, August 19, 1954.

in turn, depends on the absolute size of the dividend (or earnings for distribution) component of stockholders' income. Per dollar of dividends, the exclusion gives greater relief at all income levels. However, its absolute amount is limited.

To determine how much relief, all things considered, will be provided by the Internal Revenue code of 1954, we applied its provisions to our "average" stockholder data for 1950, this being the most recent year for which the necessary tabulations are available. Our findings are presented in Table 5. (The reader is reminded that stockholder incomes include the retained as well as the distributed portion of net corporate earnings.) The relation between the degree of relief and stockholders' income is U-shaped. Proportionately the greatest relief occurs at the bottom and top of the income scale, with a lesser degree of amelioration

TABLE 5

ESTIMATED REDUCTION OF NET EXTRA BURDEN ON EARNINGS FOR DISTRIBUTION DUE TO RELIEF PROVISIONS OF INTERNAL REVENUE CODE OF 1954, USING DATA FOR 1950

Imputed Gross Income Level (thousands)	Differential Against Earnings for Distribution		Reduction in Differential Due to Relief	
	Before Relief	After Relief	Absolute (percentage points)	Relative (per cent)
1	2	3	4	5
$ 1	34.1	24.1	10.0	29.3
2	34.1	26.4	7.7	22.6
3	34.1	27.6	6.5	19.1
4	33.7	27.4	6.3	18.7
5	33.8	28.3	5.5	16.3
6	33.9	28.8	5.1	15.0
8	33.7	29.9	3.8	11.3
10	32.8	29.3	3.5	10.7
12	32.6	29.3	3.3	10.1
15	32.1	29.1	3.0	9.3
20	30.8	27.9	2.9	9.4
25	29.2	26.3	2.9	9.9
50	24.9	22.3	2.6	10.4
75	21.7	19.2	2.5	11.5
100	19.1	16.6	2.5	13.1
150	16.7	14.2	2.5	15.0
200	14.6	12.2	2.4	16.4
250	13.2	10.8	2.4	18.2
500	10.0	7.6	2.4	24.0

in between. (See column 5.) For the lower average stockholder incomes where the absolute amount of earnings for distribution is small, the exclusion, which gives more relief per dollar of dividends, far outweighs the credit in importance. Hence the relatively high degree of relief observed. Near the top of the income scale the strength of the relief is explained by the fact that the differential against earnings for distribu-

tion becomes steadily smaller as the size of stockholder income increases, while the absolute amount of relief is substantially the same as at lower income levels. (See columns 2 and 4.)

In summary, our major conclusion is that the relief provisions of the Internal Revenue Code of 1954 are inappropriate measures for the hardship they seek to alleviate. They are not geared directly to the basic feature of this hardship; viz., that it becomes increasingly less severe the higher the marginal personal income tax rate to which stockholders are subject. It is true that because of the relative weights assigned the exclusion and tax credit, we do find that the absolute amount of relief (measured in percentage points) is greatest at the smallest incomes and least at the top of the income scale, and that for "representative stockholders" in the lower portion of the income range the relative degree of relief is higher the greater the need for it. But, over the rest of the income scale, from about the $15,000 point on up, the opposite condition exists: the less stockholders need it, the greater the degree of relief they obtain.

One final comment. Were Congress, at some time in the future, to continue to step in the right direction and increase the exclusion and/or tax credit, complete removal of the differential burden could be accomplished for stockholders at only one income level. Those below this point would remain overtaxed; those above this income level would be undertaxed. As a corollary point, it must be recognized that if the relief afforded by a given credit and exclusion level is deemed satisfactory and just, any change in the corporate income tax rate or personal income tax rate schedule would disturb this equilibrium and necessitate a readjustment of the size of the exclusion and credit.

THE INDIVIDUAL INCOME TAX AND INCOME DISTRIBUTION

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The effects of the individual income tax upon income distribution have been made the subject of increasing attention in recent years. Pechman's recent study and the detailed analysis of the upper income groups by Kuznets have substantially increased our knowledge in this area. Musgrave and others have made useful estimates of the distribution of the tax burden among income classes. In almost all these studies[1] emphasis has been placed upon the effect of the individual income tax upon the aggregate income distribution. In this paper I propose to consider some of the effects of the income tax upon individuals with equal incomes and the consequences of the uneven impact of the tax upon both the final income distribution and the equity with which the progressive rates apply. Although equity, in the sense of equal treatment of persons with equal incomes, is generally conceded to be an objective of the income tax, the bias toward more equal income distribution has tended to minimize the attention given the gross discrimination possible under the law as it now stands. It is the contention of this paper that current practices are so bad as to seriously weaken the income tax as a means of income redistribution and to threaten its future as the largest single source of federal revenue.

The individual income tax has been long considered one of the most effective instruments of taxation because, in addition to its use as a means of equalizing incomes, it has the advantages of countercyclical flexibility, relatively certain incidence and the ease with which it can be adjusted to take account of family size, and other considerations which seem to bear upon taxable capacity. As a result, the increased use of this source of federal revenue has been generally favored. In the relatively short period since 1913, the yield of this tax has increased so that it now produces approximately 45 per cent of all federal revenues. This has been accomplished by raising the rates applicable to individual incomes and greatly increasing the tax base by drastically lowering the value of the individual exemptions. The importance of the reduction in exemption levels is illustrated by reference to the increase in number of returns from approximately four million in 1934 to

[1] Joseph A. Pechman, "Distribution of Income Before and After Federal Income Taxes, 1941 and 1947," *Studies in Income and Wealth*, Vol. 13 (New York, 1951); Simon Kuznets, *Shares of Upper Income Groups in Income and Savings* (New York, 1953); R. A. Musgrave, J. J. Carroll, L. D. Cook, and L. Frane, "Distribution of Tax Payments by Income Groups: A Case Study for 1948," *Nat. Tax Jour.*, March, 1951, pp. 1-53.

approximately fifty million in recent years. It can now be said that there are relatively few gainfully employed who do not have to pay some income tax.

A superficial review of the change in the relative role of the individual income tax suggests to many that substantial progress has been made in achieving justice in the distribution of the tax burden as among equals. Although there is a very vocal group which insists that the equalization aspects of the income tax have been carried too far and have led to the destruction of work incentives and investment incentives, no objective analysis of this question can produce evidence to support their contentions.[2] Some qualifications about the adverse long-run implications of continued high rates upon incentives have been made but the extraordinary prosperity and investment boom which has characterized the period since the income tax schedules have reached record levels is evidence that the fears of the opponents of the tax are exaggerated to say the least.

What *is* of concern is that at the same time that the individual income tax has grown to play such an important role in our revenue system it has been made increasingly arbitrary and erratic in its application. As a result, the progressivity of the tax is more apparent than real. Thus, although much is said about the severity of the marginal rates, the recent Harvard studies of top executives bring out the fact that when net capital gains are included in income no one of them paid as much as 50 per cent of his true income in taxation in 1946 and that progression stops at approximately the $50,000 level.[3] It should be noted that these computations are made from Treasury tabulations and do not allow for such items as tax-exempt interest, percentage depletion in excess of cost, and personal consumption expenditures deducted as business expense.

An analysis of the reasons for the difference between the apparent severity of the tax and its actual impact will be considered below. It is important to realize that as now applied the individual income tax is not as effective an instrument of income redistribution as generally believed and that the results of the uneven application of the tax may seriously distort the way in which business is organized and the channels into which investment funds are directed. (J. Keith Butters, *op. cit.*) If these trends toward the emasculation of the income tax continue, there is danger that the cumulative reaction against the arbitrary and capricious application of the individual income tax will destroy its effectiveness. One danger is that there will be a marked increase in

[2] J. Keith Butters, "Taxation, Incentives and Financial Capacity," AEA *Papers and Proceedings*, May, 1954, pp. 504-519.

[3] J. Keith Butters, Lawrence E. Thompson, and Lynn L. Bollinger, *Effects of Taxation, Investments by Individuals* (Boston, 1953), pp. 85-89.

evasion and noncompliance, further aggravating the difficulties we now face. Another danger is that the current tendency of Congress to extend the avenues of avoidance and legal escape will be accelerated.

Before the extent of the deficiencies of the individual income tax are described in detail, the distinction between this analysis and that usually found in general discussions of the effect of the income tax upon income distribution must be made clear. In the usual study, the sole objective is to describe factually the manner in which incomes are distributed before and after taxes. Although an attempt is usually made to adjust the income concept used to take account of certain limitations of the tax law, averages for each income class are used and no attempt is made to indicate the range of tax liabilities to which individuals of the same income class may be subject. There is usually no attempt to measure the extent of avoidance of income tax liabilities. Most studies exclude net capital gains from income so that the data will conform to the concept of income used for purposes of national income measurement. In no case do estimates adequately take account of the influence of accrued income—particularly realizable but yet unrealized capital gains and losses! Similar deficiencies exist in the estimates of net income due to questionable use of expense allowances and other deductions from gross receipts. Thus if, as is the contention of this paper, the effect of existing tax law and administration is to encourage taxpayers to find means of reducing taxable income without substantially reducing their real income, all estimates of changes in income distribution in recent years must be qualified to the degree this has been true. If, as also is the contention of this paper, the possibilities of avoidance of taxation are more extensive in the upper income brackets than in the low, studies based upon income tax returns will show an artificial income distribution both before and after taxes.

An example of the problem that must be resolved is the finding of Richard Goode that the corporation income tax, which he assumes to rest on the stockholder, is progressively distributed because dividend income is concentrated in the upper income brackets (*The Corporation Income Tax* [New York, 1951], pages 73-97). This is true and will result in a more equal income distribution pyramid after taxes. The point here emphasized is that the effect of the corporation tax will still be of concern to the low-income individual who has dividend income and of little concern to the high-income individual if his income does not include dividends. The general tendency of a tax to equalize incomes does not relieve nor in any way satisfy the individual in low brackets subject to a heavy burden while his neighbor may be escaping the burden entirely. Both the general progressivity of the corporate tax and its

possible discriminatory effects are significant but one does not cancel out the other.

Sources of Discrimination. There are two major sources of discrimination in the distribution of the individual income tax burden. First is the discrimination which results from the uneven enforcement of the tax law. This is a problem at all income levels and can be related to source of income and occupation more certainly than income level. Thus while the wage earner and salaried worker subject to deduction at the source pay on almost all their income, the farmer, dividend recipient, professional man, and small businessman are all evaders of the income tax to a much greater extent.

Both available data and the well-known emphasis of the Bureau of Internal Revenue upon the auditing of the returns in the upper income brackets suggest, however, that this problem is greatest at the middle- and low-income levels. Few taxpayers in the top brackets can consistently fail to report reasonably accurately their total taxable income. The more complete records and accounting systems which are maintained by both large-scale business and the upper bracket taxpayer are also conducive to better reporting and enforcement by the Bureau.[4] We may conclude that the underreporting of income for tax purposes, although serious and not entirely confined to any one income level, does not seriously change the impact of progression of the income tax. On the other hand, the sharp discrepancies between the compliance of the farmer, the small businessman, and other self-employed and professional workers and wage and salaried workers can be just as disturbing and seriously threaten the effectiveness of the income tax. To add insult to injury, the use of the standard deduction of 10 per cent tends to make the income tax a gross tax for wage earners with the sole advantage provided by personal exemptions.

The second and more important source of discrimination reduces both the progressivity of the tax and the equity of its application. In this category are the exclusions from income and differential rates of taxation of income from certain sources. Certain of these features of the law are of advantage to only top-income brackets or individuals with substantial property income. The effect in these cases is to reduce the extent and severity of progression at the same time that equity is being violated. In other cases the opportunity to realize the possibilities of tax avoidance is a function of the type of employment or employer, the industry, or even the section of the country in which one lives. Some—and in recent years an increasing number of possibilities—are

[4] Selma Goldsmith, "Appraisal of Basic Data Available for Constructing Income Size Distributions," *Studies in Income and wealth*, Vol. 13 (New York, 1951), pp. 266-327; Simon Kuznets, *op. cit.*, pp. 435-468.

restricted to the aged or related to marital status. The list of possibilities is well known and at this time of year brought to one's attention by tax advisers, investment analysts, and others trying to share some part of the special privileges open to the taxpayers.

Among the important means of reducing the tax burden in the top brackets are the following: tax exemption of state and local bonds, the maximum rate of 25 per cent applied to capital gains realized after six months' holding, percentage depletion, income splitting for married couples, family partnerships and trusts, differed compensation plans and pension plans, stock options, and, finally, the expense account. All income levels can benefit from the following means although the bias in favor of the wealthy is less clear. In every case the possibility is open to only those taxpayers fortunate enough to have income or property in certain forms. The possibilities include: tax exemption of most income in kind with special emphasis upon the rental value of owner-occupied homes, the deduction of mortgage interest, pension and health plans paid for by the employer, fringe benefits if they are not realizable in cash, and, finally, the growing favors extended to those over sixty-five.

As Stanley Surrey has pointed out, "we have a large number of separate tax rates on the books which in combination produces effective rates of tax which are nowhere near what you would expect when looking at our surtax tables." And, also, that "the executive has his stock option, his pension arrangement, his deferred compensation arrangement, and his expense account. The investor has the capital gains rate for his securities and even tax exempt securities along with percentage depletion for his tax exempt oil holdings and capital gains for his coal royalties. The farmer has his livestock sales. Certain partners have splitting of income with their minor children."[5]

Although each of these avenues of tax relief deserves detailed analysis, the limits of this paper prevent more than a cursory treatment. Perhaps the most important quantitatively is capital gains, as it has the double-barreled effect of both reducing the tax on realized gains and permitting the permanent and complete avoidance of any taxation on gains that are never realized but become a part of one's estate. It is here assumed and asserted that capital gains should be included in the concept of taxable income. L. H. Seltzer has presented the most compelling evidence upon this point in his monumental study of the question (*The Nature and Tax Treatment of Capital Gains and Losses*, [New York], 1951). Even if some reservations are still maintained

[5] Statement as discussant at Congress of Industrial Organizations *Conference on Taxation*, Washington, D.C., October 16, 1953, pp. 42-44; and "Policy Issues in Federal Taxation," *Current Business Studies*, No. 10 (Trade and Industry Law Institute, Inc., 1951), pp. 20-29.

on this point, the general acceptance of the market and the professional tax adviser of the fact of many possibilities of conversion of ordinary income into capital gains makes the issue one of major concern for the purposes of this paper. The general assertion of the national income specialists that capital gains cannot be included in economic income must also be questioned on these grounds.

Along with capital gains must also be included undistributed profits of corporations. These have grown in recent years to proportions unheard of in previous periods—in part, as the result of the advantage to top-bracket taxpayers. In the case of the family corporation the possibilities of tax avoidance are most clearly found. Although the law provides for penalties if the retention of earnings is for purpose of tax avoidance or is unreasonable, the record indicates that this has not proved to be an effective method of preventing the widespread use of the corporation as a means of tax minimization. The importance of the family corporation may decline as a result of the explicit legalization of the family partnership, but the large corporation will probably continue to find it acceptable to its shareholders to retain large sums.

Both effects have been subject to some analysis and the results indicate that a signficant variation in the distribution of income before and after tax will result if either one or both are included within the concept of income. As George Garvy has pointed out ("Functional and Size Distributions of Income and Their Meaning," AEA *Papers and Proceedings,* May, 1954, page 246), Kuznets' estimate of the reduction in the share of the total income going to the top 1 per cent between 1939 and 1946 is nearly halved if realized capital gains are included within the income concept (reduced from 2.82 to 1.60 percentage points) and the decline in the top 5 per cent is reduced by one-third (from 5.25 to 3.63 percentage points). It is therefore true that the sums involved are significant for both total revenue realized and its effects upon income distribution. A large and unknown quantity of gains never realized but accruing to the taxpayer must also be mentioned. Due to the failure of the income tax law to require a final settlement of accrued gains on death, these gains can never be accounted for in spite of the fact that the assets involved are valued for estate tax purposes. The donee uses the estate tax valuation as his basis for future gains and of course may transfer the assets again on his death without payment of tax. When the differentials become so attractive as they now are between the capital gains maximum of 25 per cent and the marginal rates applicable to normal income, the continuance of this form of tax avoidance can be expected. Further opportunities have been offered the taxpayer in certain selected cases. These include the granting of capital gains treatment to coal mine

royalties, to farmers on the sale of livestock held for breeding purposes, and to certain stock dividend arrangements.

The extension of the means available to minimize one's tax burden has been a slow but steady process. Not only do the wealthy find means of escape but organized labor, the farmer and the aged are fast catching up. The recent study of J. H. Guttentag, E. D. Leonard, and W. Y. Rodewald of the taxation of fringe benefits indicates the growing importance of this avenue of escape as well as the complexities of dealing with the problem ("Federal Income Taxation of Fringe Benefits: A Specific Proposal," *National Tax Journal,* Vol. VI, No. 3, September, 1953, pages 250-272). Garvy estimates that employer contributions to health and welfare plans alone have increased from 156 million in 1939 to nearly 3.5 billion dollars in 1952. He states that, "together with employer contributions to social insurance funds which are excluded from personal income, they now exceed the monetary interest and the monetary rent components of personal income." (*Op. cit.*, page 243.) When deferred compensation of executive personnel and the growing use of pensions as a measure of tax reduction are considered, we must conclude with Garvy that "the question might be raised to what extent the drift toward equality, which the size distributions for this country show unmistakably, represents changes in the timing of the compensation for services rendered rather than changes in the earning capacity of factors of production." (*Ibid.*, page 244.)

Other students of the trend toward the relief of certain taxpayers have come to conclusions similar to mine. L. H. Seltzer believes that "few persons like to see a baseball game in which there are no runs, no hits, and no errors; or a football game in which no one makes a touchdown. Many congressmen and other persons have a similar feeling about the tax system and the chances of achieving outstanding success. They do not want an airtight tax system. They want to preserve the opportunity for a man to make a financial home run, a touchdown, a killing. The preferential treatment of capital gains has the virtue, in their minds, of offering just such an opportunity." ("Capital Gains and the Income Tax," AEA *Papers and Proceedings,* May, 1950, page 378.) Surrey generalizes somewhat more in his conclusion that "Congress introduces these various low rates maybe because Congress doesn't really accept the principle of progression in the upper brackets. . . . The result is, however, a complete distortion equity-wise among the upper bracket taxpayers. Some do pay these high rates and some don't, even though the average is less than a 50% effective rate." (Congress of Industrial Organizations, *Conference on Taxation,* October 16, 1953, page 43.) Colm finds that "the high rates are largely ineffective because our system has shown its ingenuity in legal tax

evasion and shifts a great deal of luxury comforts of businessmen to expense accounts, thereby making ineffective some of the high rates." (*Ibid.*, page 5.) Walter Heller sums up the indictment neatly as follows: "But growing erosion and corrosion of the income tax base is increasingly raising questions of how far we can extend our reliance on this form of taxation." (*Ibid.*, page 23.)

A final point must be made with regard to income splitting. This feature of the income tax law, introduced in 1948, was designed to prevent a serious and growing inequity in the treatment of married couples in community property and noncommunity property states. The problem became acute when Pennsylvania and some of the other eastern states changed their property law to open this tax haven to their residents. As a result, we now give to all wives a capital value. If any in my audience are still single, I commend to you the computations of Ludwig S. Hellborn. He has estimated the value of the dowry brought to every husband by his wife or, in economic terms, her cash value under the terms of the 1951 law. If your annual income is $5,000, she is worth $11,720. If you have a $10,000 income, she brings you a reduction in annual tax which can be capitalized at $41,360. At $50,000, she becomes worth over a million dollars and may be worth as much as $11,457,000 if your income is $309,000. After this point her value decreases steadily until she becomes worthless, for tax purposes, to a man earning over $1,224,000. ("Uncle Sam's Dowry," *National Tax Association Proceedings*, 1951, pages 310-314.)

This problem is not easily resolved, but the present discrimination against those who choose to remain single has not been solved by either the extension of income splitting advantages to heads of households or the recent and perhaps not unconnected increase in the proportions of the population who have chosen the joys of marriage over the costly freedom of single life.

The Effects of the 1954 Code. Much has been said about the 1954 Revenue Act which for the first time in many years attempted to recodify the basic tax legislation of the nation. This commendable objective was completed within record time but with unfortunate results in the area under investigation. Perhaps the objective of the act was not to promote greater equity in the taxation of equals but it must be judged on these grounds as well as the results achieved in clarification of the law. It is clear that the chemistry of the process of working out the new tax code between the Treasury and Congress resulted in a low valuation of equity in the sense used in this paper. A quick review of the new act indicates that none of the major devices for minimization of taxation has been removed—in many cases they have been extended—and the law has opened some entirely new avenues of escape.

Some of the major sins of commission will be discussed below. The first is the much debated dividend credit which permits the first $50 to be excluded and 4 per cent of the balance of dividend income to be deducted from the tax after the inclusion of all dividend income in the tax base. Although small, this provision gives tax relief of equal value to all brackets. However, as Daniel Holland points out in his paper, the discrimination arising out of the "double taxation of corporate income," if indeed there is real discrimination, declines as the individual reaches higher brackets. One can also complain that although the flat amount deductible may be justified on grounds of administrative convenience and realistic enforcement criteria, the sum available to husband and wife is $100 and is increasingly valuable as family income rises.

The provision of extra exemptions for taxpayers over sixty-five is continued in the new law, and an additional tax allowance of $240 is granted to sweeten the pot. Thus a married couple over sixty-five now receive $2,400 exemptions—a tax credit of $240 above this and the usual standard exemption. In other words, they will not pay any income tax if their retirement income does not exceed $4,000. The advantages of age are further extended by complete exemption of social security payments, and other special provisions for certain types of retirement income. Further relief is granted by extending annuity deductions to recipients who live beyond the period when their capital has been returned. Although this again has the rationale of eliminating a provision of the law confusing and difficult to enforce, the new provisions are even more complex than the old. I find it difficult to justify this trend when the contrast between the position of the older group and those just starting out in life is made. The latter group has little stock to draw upon and cannot wisely expend what little capital they may have. The aged, on the other hand, may well have complete furnishings of a home, adequate clothes to last their lifetime, and can and do expend their capital by purchase of annuities and other means. To claim that the aged have less taxpaying capacity than the young with equal income is a gross miscarriage of justice. The rule of expediency is the only possible justification for the special favors granted. The desire of those who are forced to retire to live more nearly as they had been able to before their reduction in earning power explains the appeal to the aged. But all of us wish to live better or have larger incomes than we do, and it is my guess that if the intensity of the yearning for better things could be measured the young could out-yearn the aged any day. The aging of the population—which is now certain as the result of increased life expectancy and the maturing of the high birth crops of past years—makes this development one of growing significance.

A sore spot for many years past has been further extended by the action of the Congress in the area of percentage depletion. The extension of this feature of the law in 1951 has been continued by both more specific inclusions of certain extractive industries in the high percentage categories and especially the transfer of uranium and a list of some thirty-five domestically mined metals and materials from the 15 per cent to the 23 per cent category along with the long-privileged sulfur. The base for computation of percentage depletion is also somewhat extended, to include as a part of gross income certain treatment processes for talc, magnizite, coal, and phosphate rock. The final extension includes the raising of deductible mine exploration expenses from $75,000 to $100,000. It is of some interest to note that in the case of the granting of the 23 per cent rate to new areas and raising of the exploration expense allowance it was the Senate that took the initiative after the bill had passed the House of Representatives. The Texas plutocracy is still with us, and it looks as though they might be able to dig in and make several more millions before their favored position is brought to an end by public reaction.

Not only mining interests have been able to win special favor. Lumber operators are given capital-gains privileges on net proceeds and the royalties from coal are also given this favor. In the case of coal, the basis for computation of gains is increased in the new code by permitting certain expenses to be included, thus reducing the tax.

We may conclude that in spite of the real accomplishments of the new code in the area of clarification of existing provisions and the prevention of certain abuses due to the lack of clarity in the law, the net effect has been to extend, aggravate, or otherwise enhance the opportunities for tax avoidance.

In all fairness, one must remember that Rome was not built in a day, nor the income tax law destroyed by a single Congress. Most of the difficulties cited above are the responsibility of other Congresses under the leadership of other administrations. Nevertheless, the current outlook has been made more pessimistic by virtue of the failure of the new legislation to correct any of the major shortcomings of the law. When so much emphasis was placed upon the complete revision of the tax code and the desire to develop an integrated law, the results cannot but be disappointing.

Conclusions. It is my conviction that many of the difficulties and inequities of the law as now applied cannot be removed without new and rather radically revised leadership from those who really favor the achievement of an equitable, progressive tax system. I believe that we must face the fact that confiscatory rates of taxation are not acceptable. It is probably true that as long as nominal rates of 70 per cent

and up are continued and they apply to any large number of taxpayers, Congress will continue to provide means of escape. When the possible inequities and real incentives to change business methods which follow from current practices are taken into account, the case for a major drive to clear up this situation is made compelling. It is the suggestion of many—which I share—that if the law were revised to prevent wholesale avoidance, a substantial reduction in top-bracket rates could be accepted in return. This position is further supported by the realization that expenditures on welfare programs can more effectively reduce the sources of inequality of income than can the income tax and at the same time help to increase the total national product.[6] Let us not kid ourselves about the effectiveness of the top-bracket rates. Let us remember that a truly effective income tax with more modest pretensions is worth more than one riddled and weakened for political reasons. It seems to me that the choice now before us is between the salvage of the income tax and the return to a greater dependence upon indirect taxation. The continued threat of a new mobilization program or even the outbreak of war makes immediate action most desirable. Our financial position will be greatly strengthened if these problems were resolved before a new emergency arises. No tax law can be perfect. It is clear, however, that few can be as bad as the current income tax law and still command the respect of any intelligent citizen or economist.

[6] Allan G. B. Fisher, "Alternative Techniques for Promoting Equality in a Capitalist Society," AEA *Papers and Proceedings*, May, 1950, pp. 356-368.

DISCUSSION

WALTER W. HELLER: It is not often that one encounters on one program two papers of such a basically different nature as the two we have enjoyed. Mr. Holland's is essentially a report of findings of painstaking statistical research, supplemented by an appraisal of the new dividend credit in terms of those findings. Mr. Strayer's, in contrast, is a broad-gauged survey of the personal income tax, based on analysis, intuition, and previous research findings. From it, also, policy implications are drawn. It is noteworthy that at the single point of intersection of the two papers, namely, the dividend credit, the two speakers join forces. Both conclude that it is ill-suited to overcome the inequities of our present system of taxing corporate earnings.

Mr. Strayer puts his misgivings about the dividend credit in a broad context of concern, not to say alarm, over the growing inroads on our federal income tax base. I am sure that many of us share his alarm and welcome his well-focused statement of the issues involved. His review suggests ample cause for action to prevent further deterioration and to restore pieces of the base previously chipped away. However, before engaging the forces which are undermining the income tax base, one might be well advised to test not merely their strength but also the rationale of the concessions they seek. Granted, preferential tax treatment offends the dogma of impartiality—"the equal treatment of equals"—which we of the public finance brotherhood hold dear (and rightfully so). But perhaps there are other admissible, even supervening, social and economic ends to be served by the income tax which conflict with our dogma.

Reviewing the whole array of discriminatory features that Mr. Strayer has placed before us, we find that our federal income tax rates, in practice, run from zero on tax-exempt bond interest and imputed income, to preferential rates on capital gains and income from exploitation of mineral resources, to full rates on wage and salary income. To the extent that differentials are the result of disparities in administration (the income tax appears to be about 95 per cent effective on wage and salary income, 75 per cent effective on dividends, and considerably less than that on interest, rentals, and farm income), the discrimination is irrational and indefensible. Rationality begins to enter when we encounter exclusions of imputed income, substitution of standard for actual deductions, special rules on dependents' incomes, and so on. Here, equity stands aside for simplicity of compliance and administration. A desire to stimulate or protect certain types of production and investment—e.g., in enterprises in foreign countries, in oil and gas exploitation, and in risky undertakings generally—motivates other deviations from the rule of equal treatment of equals. Broad social considerations motivate tax differentials between persons above and below the age of sixty-five (through extra exemptions and retirement credits) and between two single people and a married couple with identical incomes (through income splitting).

In reflecting on these departures from "impartiality," each of us would probably admit into his own "ideal" income tax structure some tax differentials

arising out of administrative, economic, and social considerations. The moment we do, of course, we acknowledge the fact that we are in the realm, not of absolute standards, but of a value consensus. Our dogma must make room for careful evaluation of the ends sought by preferential tax treatment, the likelihood that such treatment will achieve these ends, and the price paid in violations of "horizontal equity." In the case of tax concessions to stimulate desired economic activity, for example, one might ask: How large is the marginal social benefit, if any, and what special tax benefits have to be conferred on the favored group, and at whose expense, to achieve it? Where the tax concession involves a large subsidy to special-interest groups but only small third-party benefits, the path of virtue is clear. But where tax differentials do in fact influence resource allocation or income distribution in accord with accepted economic and social objectives, the decision may be more difficult. Granting, then, that the cause for action to restore the income tax base is compelling, one would do well to proceed only after a careful weighing and balancing of benefits and costs.

Turning to the findings of the Holland paper, one may examine them in terms of what they contribute to our knowledge on the taxation of corporate earnings and to the resolution of policy problems and conflicts in this field.

On the first point, previous studies like Richard Goode's 1946 survey for the US Treasury, *The Post-War Corporation Tax Structure,* have shown by arithmetic how the unintegrated corporate-individual income tax overtaxes those in the lower and middle income brackets and undertaxes those in the higher brackets. Illustrations have been developed to demonstrate the impact of varying tax rates and percentages of corporate earnings retention on the amount and pattern of differential taxation. In some cases, *Statistics of Income* data have been used to provide rough approximations of actual distributions and inequalities.

Mr. Holland's excellent study substitutes statistical measurement and precision (within the confines of his assumptions) for arithmetic and approximation. Adopting the central assumption that the corporation income tax rests on profits, limiting his study to taxable dividend recipients, and using the partnership treatment as the criterion of differential taxation, he tells us the following important things about taxes on stockholders: (1) who, by income brackets, was overtaxed and who undertaxed and by how much on the average; (2) the changes in the "crossover income-points" (from overtaxation to undertaxation) during the 1940-50 decade under the impact of shifting relationships (not summarized by Holland in his paper, unfortunately) between corporate and individual tax levels and between retained and distributed earnings; (3) the total number overtaxed (96.8 per cent in 1950) and undertaxed (3.2 per cent in 1950) and what percentage of corporate earnings fell in each category (54 per cent overtaxed; 46 per cent undertaxed); (4) the net overtaxation of stockholders in the aggregate as measured by the revenue that would be lost in shifting to the partnership method (for 1953, 5.7 billion dollars currently plus 1.1 billions in the future); and (5) how the 1954 dividend relief provision affects the pattern of differential taxation.

In short, Holland has greatly strengthened one link in our data chain by quantifying and verifying the tax differentials between dividend recipients and other taxpayers. There are, of course, many other links that must be strengthened before the full dimensions of the problem can be measured as a basis for policy decisions. Three of the important questions that might submit to analysis and statistical inquiry—at least in greater degree than they have to date—are the following: (1) What is the incidence of the corporate income tax; i.e., how much of it is passed forward to consumers and backward to productive agents or absorbed via capitalization by the present owners of stock, thus diminishing the controversial "double burden" on stockholders? (2) What is the amount and pattern of the greatest relative overtaxation of corporate earnings; namely, that which falls on tax-exempt individuals and institutions? (3) What are the actual burdens that fall on undistributed earnings via capital-gains taxes on stockholders?

The first and third of these are exceedingly tough research nuts to crack. Perhaps we lack the tools needed to crack them. But they do raise this question about allocation of available research resources: do we gain more by turning a bright, well-focused light on a segment of the problem that is already fairly well illumined, or would it be better to shed even a little light on the dark, murky corners of the problem that we now submerge in our assumptions? This raises the basic question of what criteria should guide us in allocating research resources. To achieve the optimum allocation requires an accurate balancing of net research products against opportunity costs of various projects. Even with the benefit of hindsight, this balancing process is bound to be inexact. The Holland contribution will undoubtedly be cited as authoritative in its area of inquiry for years to come. Perhaps one should ask no more.

When one turns to the second question (that of what research of this kind can do to resolve the controversy and clear up the misunderstandings to which Mr. Holland refers at the outset of his paper), one finds little basis for optimism.

To resolve controversy in this area requires nothing less than agreement not only on fundamental value judgments concerning the definition of horizontal equity and preferences as to vertical equity, or progressivity, but also on the relative role of incentives and markets in tax policy. These are differences which will not submit to analysis and statistics.

Even without entering this arena of inescapable differences in value preferances and economic persuasions, one is dismayed to find how sharply authorities can differ in their readings of a given set of facts. Thus, Holland concludes: "The tax credit, by providing a constant amount of relief at all income levels, operates in such a way that a greater degree of relief is obtained the higher the stockholders' income level. . . ." Yet Dan Throop Smith, speaking to the National Tax Association last September as a Treasury representative, concluded: "The adoption of relief in the form of a credit . . . gave greater proportionate relief to lower bracket taxpayers." Each has looked into his mirror on the wall, asked which is the fairest of them all, and emerged with answers bearing no resemblance to one another.

Apart from disagreements as to the meaning of available data, one finds

many dissenters from that interpretation of horizontal equity which regards the partnership treatment of corporate income as constituting equal treatment of equals. Those, for example, who regard corporations as more privileged by law than partnerships (because of limited liability, with the implicit residual cost it imposes on society as a whole) may consider differential taxation of corporate earnings as quite consistent with horizontal equity. This would in no sense invalidate Holland's careful work, but would require relocation of his crossover income points to fit individual convictions as to justifiable differentials.

A desire to remove differential taxation may, of course, be quite secondary to distributional and incentive considerations. In recommending a 15 per cent dividend credit, President Eisenhower stated: "This will promote investment which in turn means business expansion and more production and jobs." By identifying more reliably the beneficiaries of dividend tax relief, the Holland study provides a better basis for forming judgments as to possible impacts on investment. But, like most tax research, it is designed to inform the policy-maker rather than to make decisions for him.

This point applies even more forcefully to interpretations of vertical equity. Many of us would agree with Paul Strayer that the largely artificial rates above, say, 70 per cent involve a net loss in terms of distorting incentive effects, stimuli to avoidance and evasion, and unnecessary increments to tax lawyers' incomes. One may safely surmise that this view is shared, probably with some intensity, by the Treasury team of today. Yet, even though the revenue loss from a cut-off at 70 per cent would be less than a half a billion dollars, a frontal assault on the unrealistic top brackets is politically ruled out. ("Only the Republicans can recognize Red China and only the Democrats can adopt a sales tax.") If this be true, an attractive "second best" is to reduce the tax on those components of income which are most unequally distributed. Very considerable progress has already been made along these lines in the capital-gains area. Dividends would clearly be next in line in carrying out such a policy.

This is not to say that lessened progressivity was the prime object of dividend tax relief in 1954. But the action recommended was not inconsistent with this aim. And since methods which are inconsistent with it were rejected even though they would have done a superior job of removing tax differentials (e.g., the withholding method or the corporate dividend credit), one may reasonably conclude that distributional preferences played a considerable role.

Research is indeed indispensable to policy. But the paths from sound statistical research to sound economic policy are seldom straight and narrow.

RICHARD E. SLITOR: Dr. Holland has given us a very thoughtful and lucid preview of his National Bureau of Economic Research study on the combined burden under the corporate-individual tax structure. His findings and analytic contributions will be of real interest to students of taxation, particularly those concerned with the problem presented by an unintegrated corporation-individual income tax system. In the development of its conceptual frame-

work and in its technical craftsmanship, his paper reflects penetrating analysis and skill in an unusually complex and difficult area of applied economics.

Before proceeding further, I wish to make clear that my comments on his paper will be technical and philosophic in character and are not intended in any way to assert or defend a particular policy view on corporate taxation or the double-taxation problem. For reasons which will readily be understood, including the degree of circumspection proper to a civil servant, I am not in a position to examine the rather direct criticisms and recommendations respecting practical tax policy which appear in Dr. Holland's discussion. I can only observe that there seems to be some inconsistency between the careful caveats and reservations at the beginning of his paper where he states flatly that his findings are highly qualified, "valid only within the premises and technique used in their derivation," and the subsequent free-wheeling conclusions where—if he does not throw caution to the winds—he indulges in some fairly uninhibited policy judgments.

My comments on Dr. Holland's paper will deal with three separate points: (1) the major thesis or objective of measuring the combined corporate individual tax burden viewed as a tax on stockholders, which establishes the theoretical framework of the study, (2) the underlying assumptions about incidence, imputation, and investor behavior which go into the development of his findings, and (3) certain procedural aspects.

His study derives its basic inspiration from the idea that the corporate income tax is a kind of personal withholding tax at source on corporate profits. This in itself is a kind of assumption which involves related questions of the rationale of the tax, the economic separateness of the corporation and its stockholders, as well as matters of shifting, incidence, and imputation. Of course, the study may be regarded as a mere pursuit of an extreme set of assumptions to their logical conclusion. But, in practice, the treatment given it, including the author's readiness to draw practical policy conclusions from the results, suggests that the study is more than an exercise in the systematic exploration of certain postulates and premises, but is considered to be based in substantial degree on the most realistic and fruitful analytic approach and assumptions.

Personally, I have always felt that the role of the corporate tax, historically and conceptually, goes beyond that of an ancillary individual income tax. It is unnecessary to recapitulate here the reasons in support of this view, including the traditional concept of the tax on a separate corporate entity recently illustrated by Colm's "partnership in enterprise" principle, as well as what I have referred to as the special role of profits in a high-level economy bolstered by a government commitment to maintain high employment and economic activity and elements of deliberate partial insulation of business earnings from the full impact of the progressive individual rates. Suffice it to say that, to the extent Dr. Holland's study does not embody a realistic concept of the role of corporate taxation, it produces results which, however interesting on other grounds, are not entirely relevant to an appraisal of the actual performance of the corporate tax.

The assumed economic identity of the corporation and its stockholders

involves one of the fundamental unresolved questions not only of taxation but also of other branches of economics. Economists are divided into camps and occupy various intermediate zones of no man's land, depending on whether, or the extent to which, they see through the corporate veil. The particular position taken in the study, therefore, must be taken as a semiarbitrary selection of a particular premise.

It may be answered that, regardless of the conceptual role of corporate taxation, the differential burden with respect to stockholders may be measured on certain assumptions of incidence and imputation of retained earnings. But, in fact, the conceptual role and the assumptions are merely opposite sides of the same coin.

Holland handles the difficult question of incidence with alternative assumptions (except in his partnership-method revenue estimates, where an alternative to 100 per cent incidence on stockholders would modify his conclusions). Certainly he deserves great credit for his refusal to be bemused by apparently conflicting, chaotic views on the incidence problem, judgment on which has threatened to become, in his words, "a matter of taste, not economic analysis." Both from his preference for the standard arguments against shifting and from other indications, I have the impression, however, that he oversimplifies the situation by implying that there is a single, unchanging rule of incidence if it could only be discovered, although opinions on it in the present imperfect state of knowledge differ widely. It seems fairer to recognize that the nature of corporate tax shifting and incidence, or what is left of the concept in the modern macroeconomic setting, varies from situation to situation, with the varying shades of competitive or semimonopoly conditions, and from time to time over different phases of the economic cycle.

If this is true, Holland's assumption of 100 per cent incidence on the stockholder should be treated as a logical extreme and the alternative of 50 per cent incidence at best as a conglomerate average. Tax experts are frequently compelled to furnish a practical, clear-cut view on incidence. On such occasions they mutter darkly, perform certain suitable incantations, and come up with some rule of thumb, such as a specified fraction of shifting, backward or forward. These generalities are sometimes useful, but there is a world of difference between their expression as desperate generalizations and their systematic application to the task of detailed burden measurement in the manner of Holland's study or the more general study by Musgrave and others several years ago.

The imputation of retained earnings to stockholders, which is equally germane to Holland's major thesis, is one which he places on a basis which I consider unrealistic. Such imputation is especially unrealistic in the case of shareholders in large publicly-held corporations, whose control over corporate distribution policy is generally ineffective. Market values of stock not only reflect retained earnings imperfectly, but there may actually be an inverse relationship between retention and market value.

Holland presumably defends 100 per cent imputation on the theory that income to the stockholders is what could be distributed each year without impairing the capital of the enterprise. For this purpose, he takes the accepted

tax accounting concept of income, although he outlines alternative calculations based on current as distinct from historical costs in measuring depreciation and inventory adjustments.

The theory that the earnings of a corporation which may be imputed annually to the stockholders comprises the amount which could be paid out if the firm were liquidated at the end of the year disregards the heavy costs which would be involved in such liquidation. It ignores the possibility that retained earnings may be a cost of competitive survival. Whatever the implications of this possibility with respect to the taxable capacity of the corporation, it suggests reasons why retentions are so imperfectly reflected in market values in either the long or short run. The real values may not be accumulated but eroded in the process of competitive attrition of capital.

Even to the extent there are real accumulations of investment capital over the long run, their allocation among a shifting population of stockholders is uncertain and erratic as a basis for annual income measurement.

All these considerations add up to my conclusion that the imputation of retained earnings along with an allocable share of corporate tax on a dollar-for-dollar basis to stockholders is a misleading basis for measuring the differential burden of the corporate tax. If serious doubt is thrown on this imputation assumption, a difficult question arises in regard to the economic incidence of the corporate tax as between dividends and retained earnings.

Recognition is given in Dr. Holland's paper to the ambiguity of imputing retained earnings to the stockholders, in view of the fact that they could not be used for purposes other than reinvestment in the particular corporation unless distributed or realized subject to further tax payment. He outlines an approximate adjustment for the present discounted value of estimated future capital-gains tax liability based on the assumption that seventy-two cents is added to share prices for each dollar of undistributed earnings. This adjustment adds to the tax on retained earnings but does not scale down the "earnings for retention" themselves for purposes of imputation. There is an apparent inconsistency between dollar-for-dollar imputation for one purpose and settling for seventy-two cents on the dollar for capital-gains tax purposes.

In view of the numerous complications, adjustments, and refinements which Dr. Holland has already patiently taken into account, I feel apologetic in mentioning others which he has apparently neglected. One is the process variously referred to as discounting, amortization, or reverse capitalization of pre-existing corporate tax by current investors in stock. To the extent the existing body of stockholders have taken account of the tax in acquiring their stocks, some observers regard them as not actually paying the tax. Of course, current buyers may have been investors in other corporate equity holdings at some previous time when part or all of a given level of corporate tax was imposed and thus have sustained the tax in that guise.

Another neglected complication has to do with the status of preferred stockholders, especially in nonparticipating issues. While the incidence in this situation is confused and controversial, the tax on the earnings dedicated to the dividends on such issues is sometimes considered to be sustained by the common stockholders.

A further complication is the possibility of spreading or diffusion of the differential burden on common stocks over other investments through the equalization or realignment of yields. To the extent such diffusion has occurred, the double-taxation differential exists but is difficult to isolate or quantify. The resulting general lowering of yields on bonds, real estate, and other assets as well as stocks due to the tax would have become a ubiquitous datum, and the impact of double taxation inextricably interwoven with other complex forces determining the market equilibrium.

Turning from the conceptual framework and assumptions to procedural aspects, the effective rates of combined corporate-personal income tax burden as measured in Dr. Holland's study are the composite resultant of a number of factors. These include: (1) the element of withholding or taxation at source at rates substantially above the lower bracket individual rates, (2) the cushioning of the net differential impact of double taxation as income and marginal personal rate increase (the corporate tax "costs" the dividend recipient less the higher his income since his net after personal tax would be correspondingly less if it were distributed), (3) the insulation or short-circuiting of retained earnings from current application of the individual income tax, (4) the retention ratios of corporations, (5) the configuration of dividend distributions in relation to the income level and applicable personal tax rates of the stockholder, (6) the effects of deficit corporations and dividend distributions in excess of current earnings, and (7) the relative levels of the corporate and individual income tax rate structures.

Throwing together all these factors and grinding out the composite averaged results tend both to blur the focus of analysis and to conceal significant diversities. The equity and economic impact of particular tax arrangements must be judged in terms of the range of diverse possibilities and particular cases, as well as the average results. This comment, I am aware, is like criticizing a tiger for its stripes. These are inherent characteristics of the method of Dr. Holland's study. Nevertheless, they indicate its limitations and inadequacies, if it is to be judged as more than a statistical measurement of tax burden distribution on the stockholder group on the basis of certain limited or extreme assumptions.

In outlining his methodology and explaining the major factors controlling his results, Holland provides two guides for more exact determination of the role of each of the variables entering into the impact of the unintegrated corporate-individual tax structure which prevailed in the years 1940-50. The formulas developed in his paper provide the groundwork for a more schematic analysis and are one of the valuable contributions of the study. The breakdown of the composite differential against stockholders into the major component differentials which, together with the related distribution of dividends and other incomes, produce the final results is another helpful interpretative guide.

Still further light might be thrown on the subject by a further schematic analysis showing, for example, the effect of different retention ratios at different stockholder income levels, varying imputation assumptions, and the varying

net differential attributable to the corporate tax under different levels of individual tax and alternative patterns of rate graduation.

Perhaps because of its limited reliance on the schematic approach, Holland's study fails, on the one hand, to make clear certain relationships which comparatively simple computations would effectively bring out. On the other, it goes to laborious lengths to bring out some broad conclusions which are inherent in, and no more valid than, its assumptions. Some of its conclusions reflect familiar relationships which, on the assumptions, could be predicted without great difficulty in advance.

I am aware that some of my comments have gone beyond the immediate scope of Dr. Holland's study and probably reflect some of my own special, and possibly crotchety, views and preoccupations. Certainly they are intended to be at least partially constructive and do not detract from the basic excellence and substantial interest of Dr. Holland's study.

CONCEPTS OF COMPETITION AND MONOPOLY

COMPETITION: STATIC MODELS AND DYNAMIC ASPECTS

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I. *Introduction*

This paper is an attempt at a contribution to the endless effort to reduce or bridge the gap between theory and reality; and in particular the gap between theories of cost-price equilibrium and theories of production, growth, and development.[1] Theory must always be the department of oversimplification; but dynamic simplifications are at least different from static. They include the tendencies toward equilibrium. But because these tendencies never reach their static limits, dynamic theory cannot use any features which are needed only to enable a model to attain this impossibly precise completeness—especially if these features are incompatible with the conditions of progress.

It follows that fully dynamic theory is bound to lack certain characteristics which are, to many theorists, the essential earmarks of theory. It must accept indeterminateness, with some margin of individual discretion in business action, and uncertainties of different kinds from those they are accustomed to dispose of by assuming a premium for uninsurable risk. Some of these uncertainties are positive aids to effective—not perfect—competition. If such theory is to be accepted as theory, it seems that the first step which theorists can contribute is the setting up of an appropriate conceptual framework, including appropriate tool-concepts. Even within theoretical writings, many or most of the pieces of this framework exist as scattered fragments; but the writer is not aware that the framework has been assembled and articulated. The present paper is a modest attempt to bring together enough pieces to afford some suggestions of what the structure might look like.

II. *The Point of Departure: Existing Competitive Theory*

In the theory of a generation ago, competition played a twofold role: as an agency to eliminate excessive and exploitive profits and as a stimulus to technical progress—a keener stimulus than monopoly would

[1] The remarkable group of papers presented at last year's meetings of this Association and the proceedings of the 1951 conference of the International Economic Association (*Monopoly and Competition and Their Regulation*, E. H. Chamberlin, editor [London, 1954]) include an appeal for a dynamic theory and confirm the existence of an impressive body of factual material large enough to afford a basis for beginnings at fresh generalizations. They present a challenge to which theory should attempt to respond.

afford. As a stimulus to progress, competition included both the carrot and the stick (the carrot of profits to the successful innovator and the stick of losses for those who fall behind) and elimination for those who fall behind substantially and persistently. Temporary monopoly, under the patent system, was also recognized as a force for technical progress. Later, Schumpeter's theory of innovation made its mark, but without being integrated into general competitive theory.

Subsequent developments, leading to the body of competitive theory now prevalent, have concentrated on more precise definition of the conditions necessary to complete or "perfect" cost-price equilibrium—conditions that turn out to be nonexistent. The model then becomes an analytical device and, as such, a step toward explaining why things do not behave like the model, the next step being to take account of the inevitable departures and the conditions accounting for them. So far, this process has produced certain other models, still highly simplified, of monopolistic competition and oligopoly, with and without product differentiation. The preoccupation of these models with cost-price equilibrium is obvious. What may be less obvious is that static limitations are built into their underlying tools of analysis—demand curves and cost curves—which therefore need modification for dynamic purposes.

This prevalent body of theory is not a theory of production, as growth theory must necessarily be. Insofar as it deals with economy and efficiency of production, it does so in terms of optimum or less-than-optimum scale of production (size of plant or firm) on a fixed long-run cost curve; that is, on the obviously static assumption that the "state of the arts" remains constant.

A more troublesome consequence of the model consists of the normative conclusions that are, rightly or wrongly, drawn from it. As a standard of so-called "perfection," it is one-legged, focusing on the essentially static objective of cost-price equilibrium, to the neglect of the dynamic objectives of progress. This one-legged standard is frequently treated, without warrant, as an ideal. And because it is unattainable, all actual or possible conditions are, by comparison, judged inferior, monopoloidal, or actually monopolistic.[2] This interpretation Chamberlin himself has flatly repudiated, stating that perfect competition is not the ideal, and the ideal includes elements of monopoly—which might mean no more than such elements of uniqueness as most businesses inevitably possess.

The idea of the omnipresence of harmful departures from perfect competition seems to underlie the challenge of Professor Galbraith,

[2] The opposite case—of pure but imperfect competition (e.g., agriculture) which tends to be unduly severe—is commonly neglected.

who says in effect: according to theory, our economy ought to be suffering all the evils ascribed to monopoly; why isn't it? Galbraith suggests that it may be the theory that is wrong rather than the facts of the economy, and this contains much truth. But when he suggests that the trouble with the theory is its adherence to competition and recommends throwing competition out the window in favor of a substitute, he is surely overplaying his hand. I would contend that the trouble with the theory is not its adherence to competition, but its too formalized conception of what competition is. This includes, first, the one-legged character of its standard of perfection, and, second, the fact that its picture of reality is highlighted by models of oligopoly and monopolistic competition which do not tell the whole story about the cases with which they deal, and do not do justice to the competitive forces that exist, including forces of progress and forces working toward serviceable—not perfect—cost-price adjustments. The cases include monopolistic evils, but they also include—more typically, I suspect—cases in which the reality behaves better than the models, including the model of perfect competition, which sacrifices important factors of dynamic progress.

But recognition of this lies under a heavy semantic handicap, if it has to take the shape of stating that the ideal includes "elements of monopoly." This raises questions, not as to the facts, but as to the expediencies of the terminology. It seems unfortunate if economists who defend realistic forms of competition thereby expose themselves to the charge of defending monopoly. This danger is not imaginary. The outstanding case is that of product differentiation—a monopolistic element which has in itself no necessary antitrust significance. I have myself said, of product differentiation (*Economics of Overhead Costs*, page 418): "In a sense each competitor has a monopoly of the difference in quality . . . and this qualified monopoly is a feature of the typical 'competitive' market." I would not say he had a monopoly of his product, but only of the quality differential; and I would not suggest this as standard general-purpose terminology, to be used, among other things, in antitrust connections. With such uses in view, it would appear helpful if, in our general-purpose terminology, "monopoly" began with obstructions to imitation, and "incipient monopolization" began with actions of an obstructive character.

There is food for thought in the discrepancies or contradictions between pure theory and practical or policy judgments. For example, theory appears to regard product differentiation as always a shelter from the rigors of competition; but the Interstate Commerce Commission has apparently found that the element of differentiation between rail and truck transport tends to cause their competition to assume a

cutthroat character. And businessmen recognize that a rival's product innovation can be a very aggressive and formidable method of competition. A manufacturer who has no competitors at his immediate location is not for that reason a monopolist under the antitrust laws; but he must watch his step as to what he does with the element of monopoly resulting from his unique location, under the Staley and other basing point cases. On discrimination and freight absorption, neither theory nor antitrust policy is clear and unambiguous. Excess capacity tends to be treated by theory as a monopolistic symptom,[3] but in antitrust cases it may or may not be so treated, depending on circumstances. And businessmen tend to the view that competition does not begin until they have difficulty disposing of capacity output and becomes more severe as excess capacity gets larger.

The perfect competitive market of theory is one in which prices are identical and each producer knows the others' prices and profits, which implies knowing their costs. In practice, too perfect identity of prices is legally suspect, and so is interchange of information on prices, as in open price associations, or information on costs, or even the use of a uniform cost accounting system. All these cases serve to suggest things which current equilibrium models neglect and which dynamic theory may need to recognize.

III. *Dynamic Criteria of Appraisal*

In the light of the things we want competition to do for us, what are the features it needs to have which are implied in these objectives? I will put first the elements required for progress as being most important, since even a small continuing gain outweighs a substantial gain of the once-for-all variety.

First comes progress in economical methods of production. Under competition, this implies that some take the lead and others follow, while managements are changed or firms are eliminated if they fall too far behind. But an excessive rate of elimination of firms may be unhealthy for an industry. To avoid this without retarding progress, the bulk of the followers needs to be able to keep near enough to the leaders to stay in the race.

Secondly, we want competition to afford customers an amply differentiated range of qualities and types of any given product to choose from. This is, of course, wanted for its own sake; but in terms of progress it means that the producers are exploring—and influencing—the customers' preferences and potential preferences, and products evolve in the directions these preferences indicate. Whether they evolve in the best directions is, of course, a different question. Product differentiation

[3] See, especially, Chamberlin, *Theory of Monopolistic Competition* (5th ed.), pp. 109, 171.

costs something, mainly because of the selling effort that is bound up with it. (For that matter, maintaining homogeneity of product costs something, too. And homogeneity of each brand is necessary for effective differentiation between brands.) The combination of selling costs with research and testing introduces an element of overhead cost which makes for large size and limited numbers of producers. But in a market as large as the American, this seldom goes to lengths that spell natural monopoly.

Thirdly, we want new products developed; and this is a necessary correlative of more efficient methods of production, if an increase of 2 per cent a year in productivity is to be absorbed without technological unemployment. New products, even if patented, are generally exposed to imitation and competition after a delay which is, by historical standards, not long.

A second group of objectives is concerned with the diffusion of the benefits of progress, to customers in lower prices or to those who contribute factors of production—chiefly workers—in higher real rewards. This involves a progressive sharing of the rewards of successful innovation, and the sharing becomes complete when the improvement has become part of the generally available state of the arts, from which special profits can no longer be made. This diffusion means that what Veblen called "the wisdom of the ancients" is not "cornered" by current business enterprise; only the recent advances made in it, and these only partially. The diffusion is wanted for its own sake, but it also has a special role in the incentives to progress. What it means is that a renewal of differential profits can be had only by renewed innovation. Thus it is part of the system that can keep incentives to continued innovation alive in full strength. This role would be vitiated if the diffusion were instantaneous and complete, since then innovation would bring no rewards to the innovator. The dynamic system is not one of elimination of profits, but one of erosion and re-creation, both of which are jointly essential.

For the economy as a whole, this process implies the creation, reduction, and re-creation of differential rewards in different industries, as well as for different firms. Professor Harberger's paper at the 1953 meeting developed a case for the thesis that existing differentials are relatively unimportant defects, by conventional utilitarian standards. This argument may not prove that we should be unconcerned about them; but it does strongly suggest that their dynamic impact is more important than their static. The goal is to subject them to active wearing down and prevent them from becoming so enduringly entrenched that they need not be earned and re-earned by progress.

The diffusion process also affects growth via the distribution of in-

comes, which in turn affects the market for the increased flow of goods, new and old, which progress makes available. This has two main dimensions: inequality between persons and between different industries or sectors of the economy. If one sector of the economy is progressive, it will be handicapped in realizing its potentialities if most of the other sectors are too poor to buy much of the products of the dynamic sector and too stagnant to increase their purchases as the productive power of the dynamic sector increases.[4] Development needs to be not too badly unbalanced; and from this standpoint well-diffused competition is favorable. Something similar is true of personal distribution, a fair approach to equality being favorable to the kind of demand on which mass production depends. On the other hand, new products have often been introduced first as luxury items, appealing to the rich or well to do, and later coming within the reach of mass demand, as increased sales and the resulting productive economies reinforced one another cumulatively. Nowadays, with the American mass market thoroughly established, it is sometimes possible to promote a new product for mass volume from the start.

What is not obvious about all this is the part played by competition. It opposes the inequalities of intrenched privilege; but if the labor market were thoroughly competitive, inequalities of wages based on unequal performance might be, by and large, greater than they are. On either basis, however, this country would have a great mass market plus a more limited luxury market, not too far above the mass market and shading into it by easy gradations. Competition also implies mobility of productive resouries, which is needed for adjusting to the inevitable inequalities in rates of growth in different parts of the economy. And in a growing economy, mobility is possible with a minimum of actual shifting of persons or capital.

A phase of this diffusion process is the creating of a market in which a customer may have considerable confidence that the offerings of different sellers are of approximately equal value and that he will not be seriously victimized if he fails to shop around, looking for the best offerings. This (limited) assurance is of real value to the customer who has many demands on his or her time; but it works only if enough customers do shop around. A competitive market tends toward one price to just the extent that customers watch for differentials and take advantage of them. This is the customers' essential role in a competitive system.

A third group of objectives is concerned, not with products and prices, but with the conditions of competitive rivalry in itself. It regards free-

[4] For the suggestion here adopted, I am indebted to R. Nurkse's *Problems of Capital Formation in Underdeveloped Countries* (1953).

dom and opportunity as ends in themselves. It is concerned with the human impact of competitive pressures on those who are exposed to them. But especially it is concerned that efficiency and the diffusion of its benefits should not be dependent on the good will and arbitrary decision of private producers, nor on direct governmental order, but on a situation in which normal business motives impel business units (acting independently) to conduct that will tend to further the desired results. This goal, like the goal of diffusion of gains, is overrigidified in the theoretical concept of an economic law which dictates economic conduct and its results with a precision from where there is no escape.

This would reduce economic freedom to a paradox, since the meaning of freedom resides in a margin of discretion in choosing one's course of action. One of the tasks of the kind of theory here contemplated is to define the margins of discretion that producers have and the margins they should have or can safely be allowed to have. They may, for example, face uncertainties and be governed by longer or shorter perspectives and broader or narrower conceptions of their interests. These all belong in a dynamic theory. The essential limits on private discretionary power are presumably satisified if the discretion of business units does not extend to doing things that obstruct the progress of the "generally available state of the arts" or prevent the public from getting the benefit of it. This is admittedly a not-too-precise standard; and may need some care to avoid tautology, centering in the phrase "generally available." It has a logical kinship with Marshall's "representative firm," and I believe it is not wholly meaningless.

Given these goals, how should one use them to judge a system? A general judgment may be influenced by appraisal of the performance of the system in the large. But for purposes of specific policy in actual cases, rates of progress and fairness of profits are too uncertain, dependent on too many irrelevant circumstances; and their use to determine legality would hardly be consistent with a free private economy. Instead, judgment must hinge on whether conditions are of a sort inherently adapted, by and large, to promote these ends. Three key tests may be suggested: free and independent action, incentive to do the kinds of things that are called for, and capacity to do them.

As to the first, it is not easy to make people compete if they do not want to; but collusion can be checked and protection can be afforded to the more competitively-minded mavericks who will exert pressure on the others. Dynamic change also tends, as we shall see, to keep rivalry alive. As to incentive, mention has been made of the carrot and the stick, of the joint importance of differential gains and their erosion, but no precise optimum rate of erosion appears to be definable. Where progress requires heavy research outlays, atomistic industries are handi-

capped as to both incentive and capacity: as to capacity for obvious reasons and as to incentive because the output of the single enterprise is so small that individual gains fail to measure the importance of the resulting progress for the industry as a whole. Thus in agriculture this element of progress depends heavily (though not exclusively) on governmental research and promotion.

IV. *Elements of Competition as Dynamic Process*

A theory of competition as dynamic process must be not a model but a framework within which many models may find their places, including equilibrium models as limiting hypothetical cases. As I have said elsewhere (*Monopoly and Competition and Their Regulation,* pages 326-328), the process includes initiatory action by a firm, responses by those with whom it deals, and responses to these responses by rival firms, to which one should add the subsequent rejoinders of the initiators, plus any actions that may be taken on a basis of anticipation. (For example, a rival may react defensively, before his customers have time to switch to a competitor.) The moves and the responses may affect productive processes, products, selling efforts or prices, or various combinations. They may be aggressive, defensive, or counteroffensive. Fully dynamic theory needs to conceive these patterns as themselves subject to change, over longer periods of time. Marshall's life history of a firm may be matched by life histories of products or product variants, and of the marketing patterns connected with them. Stages of exploratory introduction, aggressive expansion, defense of established position, and decline—all have different features.

For dynamic theory, a key element is a time interval between moves and responses, or a time distribution of responses; and this time factor is typically essential to give firms an incentive to make competitive moves, by giving them a chance for a temporary gain before their moves are neutralized by the defensive or counteroffensive responses of rivals.[5] Or there may be other elements tending to prevent immediate and complete neutralization.

Perhaps the chief common feature of competitive action is an expectation of a gain that is sure to be eroded; and it is not convincing to make competition hinge on businessmen being so uniformly stupid that they experience this repeatedly without learning to anticipate it. At least the more intelligent must be thinking in terms of a result that will outlast the erosion process; and in this light the decisive motive must be a preference for eroded profit on a larger volume of business,

[5] After writing the above, I find these time elements recognized by A. Henderson: "The Theory of Duopoly," *Quart. Jour. Econ.*, November, 1954, pp. 565, 580. Also by the Civil Aeronautics Board, according to a report of its decision in Air Freight Tariff Agreement case, 14 C.A.B., 424, 428, 430 (1951).

over a similarly eroded profit on a smaller volume. I speak of an "eroded" profit rather than "zero profit," partly because before profit from one successful move is reduced to zero, other moves may have brought other profits, and partly because "zero profit" carries a misleading precision. It is interesting that Chamberlin's tangency theorem for differentiated products involves conduct of the opposite sort: pricing for short-term profit, which after erosion leaves the producer with a smaller volume than he might have had with a more farsighted policy. This can happen; but the diagrammatic proof that it must happen wherever product is differentiated, is unconvincing once one makes adjustments in the concepts of cost and cost curves and demand curves which are needed for dynamic theory.

Cost, conceived as including the minimum return necessary to attract capital and enterprise, includes, under dynamic conditions, large elements of unpredictable obsolescence, thus making zero profit an indefinite, but substantial, quantity. This makes it rational to prefer larger to smaller volume, at zero profit; and this seems clearly to accord with the characteristic American business emphasis on the importance of growth.

As for the envelope cost curve, so frequently used, each point on it must logically represent the most economical method of producing a given output, if a plant could be adapted to that output and never have to change. It takes no account of fluctuations or of growth by substantial-sized units or of provision of reserve capacity to be ready to handle contingent increases, including those which a vigorous sales-promotion campaign might bring, if it proved successful. No competitor likes to expose himself to losing customers to his rivals because he cannot fill their orders promptly enough. Thus on a more realistic curve of cost with scale or size of plant or enterprise, each point would represent, for a given size or expected average output, the average cost that might be expected during the period in which the plant or enterprise remains of this size, allowing for periods when it will be working at part capacity and relatively high average cost and other possible times when it may be working overtime or otherwise be pushed beyond its optimum rate. Such a point would not be on the short-run cost curve but above it, and a long-run cost curve connecting such points would intersect the short-run cost curve, not be tangent to it.

The real point is that the whole range of actual or probable movement on the short-run curve comes into play, not merely the point of supposed tangency. At that point, long-run and short-run marginal cost are equal as drawn, thus obscuring the fact, which is crucial for explaining actual competitive behavior, that at most times short-run marginal cost is quite substantially below long-run marginal cost (which in turn is

likely to differ little from average cost) while occasional peaks of demand may push short-run marginal cost well above both long-run marginal cost and average cost. A firm may price well above marginal cost—pricing for maximum profit as determined by a sloping individual demand curve—and may still fail to cover average cost.

A timeless two-dimensional demand curve of the conventional sort leaves out of account the fact that the effect of a given price, or price differential, on the volume of sales is a function, among other things, of the length of time during which it has been in effect. The effect may reach its limit in time, or may be indefinitely progressive, if the stimulus can be maintained. This time dimension has been recognized at times by economists—generally when they were not speaking as theorists. It means, among other things, that the effect of a given price on sales volume depends on the previous price or price situation; and that the curve is not fully reversible.

A given price may take full effect in minutes (as on a produce exchange) or may take decades where it requires changes in consumers' ways of life which have strong inertia. An example might be the effect of reduced electric rates in stimulating household appliance use of electricity. This case is typical in that the active variable is better described as a price policy than a price, and acts jointly with promotion of the sale of household equipment. Similar comments apply to alterations of the product and moves in the area of sales promotion. This complex of variables would overload any possible system of graphic presentation. A family of three-dimensional surfaces—the third dimension being time—with a different surface for each initial price or price situation, would still be a simplification.

Both for cost curves and demand curves, movements along the long-run curve involve a shifting of the short-run curve, setting up a new one of different dimensions from the former one. In the diagrammatic development of Chamberlin's tangency theorem, the treatment of cost and of demand in this respect is nonsymmetrical. Long-run movements of demand are treated exclusively as unanticipated shifts of the short-run demand curve, and producers are assumed to be governed exclusively by the short-run curve prevailing at the moment and not by longer run expectations. Thus his diagrams utilize a short-run demand curve, placed against a long-run cost curve. A different method could lead to significantly different results.

The responses of those with whom a competitor deals bring in an economic process which is naturally left offstage by equilibrium theory but is essential to dynamics; namely, market canvassing and bargaining. Galbraith has taken advantage of theory's neglect of this process to annex it as one sector of his rather heterogeneous category of counter-

vailing power, treating it as a substitute for competition which has lapsed into passivity. Actually, as theory should have been insisting all along, this activity of customers is a necessary complementary part of the process of competition, serving to bring to bear and make effective the competitive alternatives that may exist. Without it, competition would become passive, though, as earlier noted, many customers can neglect it without serious results if enough others attend to it.

Three grades of customer activity may be distingushed. First, buyers may simply canvass the available alternatives and choose the one they prefer. This increases the cross-elasticity of demand on which the effectiveness of competition depends and tends toward equalizing the attractiveness of different sellers' offerings. Second, some buyers may try to get a better bargain than is currently offered, using their power to shift their patronage as a leverage. It is chiefly the larger buyers who have a chance to do this, and it is likely to result in discriminatory favors, granted frequently (but not exclusively) by the smaller and less powerful suppliers. Letting contracts on a basis of sealed bids is one variant of this process in which one large buyer tries to bring to bear a specially active form of competition for his individual business out of which he hopes to get a differential price. Finally, there is the buyer with real monopolistic power, or bilateral monopoly, in which it seems that almost anything might happen.

V. *Some Applications*

The foregoing ideas suggest, among other things, some modifications in current theorems, including those of pure oligopoly, and the tangency theorem for competition with differentiated products and sloping individual demand curves, tending to reduce the departures of these models from older conceptions of cost-price equilibrium. In the case of pure oligopoly, the indicated modifications begin with the question: Under what conditions is it natural for a firm to expect an increase or decrease in its price to be followed or not followed by its rivals, and how may these expectations be modified by devices available to a firm, other than a simple increase or decrease in its list prices? The suggestion has been made that an increase is more likely to be followed if costs have risen generally. And this possibility can be sounded out by announcing in advance an intended increase, which can be withdrawn if rivals do not follow suit. If there is much unused capacity, a price advance is unlikely to be followed, and existing prices are likely to be subject to downward nibbling. But if prices get ruinously low, a move back toward a more normal level may be followed, though likely to be unstable.

The number of rivals plays a part in that, if unused capacity exists, the actions of the group tend to be controlled by that member whose ideas lead to the lowest price policy. Numbers also play a part in that

the initiator of a price reduction which is not instantly neutralized can stand to gain more than any one of his rivals stands to lose. Thus the outcome may be affected by differences between the slopes of what may be called the aggressive and the defensive demand curves, as well as by differences between the curves envisaged by different firms. More important, perhaps, are devices for making concessions which will not apply to a firm's whole business but mainly—at least for a time—to the added business it hopes to gain. When demand is strong enough to utilize capacity fully, prices would tend to rise under either oligopoly or old-fashioned competition; but the rise under oligopoly may be more moderate because influenced by a longer time perspective. Another mitigating factor is the existence of stand-by capacity, of only moderately inferior efficiency.

As to the case of differentiated products, if this is treated in terms of short-run cost curves, price may be above short-run marginal cost and still be below average cost, and below long-run marginal cost. A fairly typical hypothesis would be that the bulk of production is on a scale sufficient to afford the bulk of the economies of size, implying that the slope of the long-run cost curve is sufficiently flat and uncertain to be an unimportant factor, except for definitely small firms. I cannot conceive a firm deliberately following a price policy which, for the sake of a small price advantage, would condemn it to be one of these small firms when a more aggressively competitive policy might lead to progressive expansion. Such a size-limiting policy involves outright danger to survival, not merely larger or smaller profits.

While competition may emphasize model changes and sales promotion, these in themselves tend to create uncertainty whether a price reduction will be fully and instantly met. Thus they tend to prevent price differentials from becoming so settled in trade custom that a price reduction by one firm is sure to be instantly neutralized by the others. Thus, while competition spurs innovation, innovation in turn helps to keep price competition from settling into an inactive rut.

In both types of case, needless to say, a workably competitive result requires that there be no outright collusion. But it does not necessarily require the ignoring of rivals' probable reactions; merely that their expected reactions should not be so prompt and complete as to wipe out all competitive incentive, either hope of gain or fear of loss from letting others "get the jump." This latter may be the more important factor.

VI. *The Over-all Picture*

The foregoing is a mere fragment of the whole picture. It by no means implies that all industry is satisfactorily competitive. It leaves much room for inequalities in the impact of competitive pressures in different

parts of the economy. The most serious inequalities are those between the pressures of competition in industry and trade, on the one hand, and, on the other hand, the pressures it would produce, if unmitigated, on agriculture and labor. One explanation for this inequality seems to carry the implication that competition in these fields would impose no undue burdens if only industry and trade were not shot through with monopoly, raising the prices of the things workers and farmers buy. Without denying that there is a measure of truth in this, I venture to suggest a different hypothesis as more important; namely, that in industry and trade, the producers' side of the market adjustment is dominated by entrepreneurs' dollar expenses, which will (with qualifications) veto production if receipts do not cover them, while in the case of labor and one-family farming units, the producers' side is a genuine supply schedule which is highly inelastic and may be actually backward-sloping—meaning that a reduction of wages or farm prices tends to result, if anything, in more crops raised, or hours of work offered, rather than less.

This is, of course, not a new idea, but it seems to carry a two-sided consequence. One side is that labor and agriculture might need some mitigation of the pressures of competition, even if the rest of the economy were unqualifiedly competitive. In other words, if we got rid of all monopoly in industry and trade, that might still not make the economy safe for unmitigated competition in labor and agriculture. The other side is that an acknowledged need on the part of labor and agriculture for mitigations of competitive pressures is neither proof nor measure of the dominance of stultifying monopoly in the rest of the economy.

There can be no certainty that competition will remain vigorous in American business. The necessary conditions are a fascinating subject for speculation. Tentatively, they appear to include three mutually interacting areas: public policy expressed in the antitrust laws, the aggressive psychology and adaptability of the American businessman, and the impact of continuous change in techniques, products, and channels of trade, tending to keep things stirred up and uncertain and to prevent competition from lapsing into routine passivity. In the troubled years that lie ahead, this complex of factors is destined at best to an insecure existence. To keep competition healthy requires the traditional eternal vigilance. To recognize competition when we see it, in its present-day forms, requires not only factual study but some reorienting of the traditional framework of theoretical concepts in which the facts may find their interpretation.

REVISED DOCTRINES OF COMPETITION[1]

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I

Modern doctrines of market competition have now reached legal majority since their birth for public display in 1933. Like many an anxious parent bewitched by the enigma of whether their offspring are prepared for life, both Mrs. Robinson and Professor Chamberlin have recently been analyzing their intellectual progeny, betraying particular concern with the frailties of their creations. Mr. R. F. Harrod, too, with some priorities in this intellectual genealogy, has been lamenting some antics of his slightly later-born, lucid and renowned "Doctrines of Imperfect Competition";[2] an air of urgency covers the transformation that he desires to impart to his fondling.

Let us consider the recent parental diagnoses and precepts, some of which will arouse neighborly head-shaking. Later, brief attention will be paid to a few rebellious playmates of their prodigies.

II

I deal with Mr. Harrod's beliefs first and at greater length, for I surmise that from a doctrinal standpoint the issues raised are paramount. Necessarily, it is the total impact of the argument, omitting qualifications, that is sought.

Harrod proposes, first, to discard the doctrine of excess capacity, largely deduced from the tangency acrobatics, as incompatible with his new attitude on price formation. Thence he argues that it is increasing returns and not imperfect competition which is the causal culprit pushing output from the social optimum. All points devolve from the revised theory of price determination, with the first being an implication of it while the belaboring of increasing returns constitutes a revived scent in the old hunt for *the* social devil.

Driving up to his price doctrines, Harrod denounces as "altogether wrong" the "generally accepted" opinions on the alleged excess capacity defections of imperfect competition as being based on faulty and contradictory hypotheses of entry and short-period profit maximization. He assures us that "any experienced man of business would pronounce it most 'unsound' to make a temporary surplus profit by charging a

[1] I should like to thank Dr. Almarin Phillips, of the University of Pennsylvania, for several useful observations.

[2] *Quart. Jour. Econ.*, May, 1934. Reprinted as "The Theory of Imperfect Competition Revised," in his collection of *Economic Essays* (Harcourt, Brace, 1952). All quotations below are drawn from this source.

high price at which it is known that sales are unlikely to be . . . maintained in the long run. It is surely wrong for economists to insist . . . that this is none the less what entrepreneurs normally do" (page 147). If competition increases, he avers, all investment is "too large." So right from the start the price is designed to fend off potential competition.

Criticising the tradition further he asserts: "The radical mistake is in assuming that as soon as he has his plant he [the entrepreneur] forgets about long-period marginal revenue."[3] But "long-period considerations on the demand side remain just as relevant to day-to-day pricing policy, after the plant has been constructed as it was before" (page 150). Hence, with "free or relatively free entry, the entrepreneur . . . will plan to charge a price yielding only a normal profit" (page 151)—defined as a sum which the entrepreneur would "deem just sufficient" when contemplating a plant extension.

This provides ample clue to Harrod's conception of the pricing process. Unmitigated pecuniary myopia and malevolence are supplanted by secular prudence and beneficence. Fair long-run price exactions expel maximum short-run extortions in this quick overturn of dogma. Mr. Hyde is Dr. Jekyll again and we are expected to lie in comfortable repose with him. Once implored to defer to the view that firms always respired in short-run exhilaration,[4] we are now beseeched to accede that they are never wholly elated, but instead are pensively aglow in contemplating a long-run thrill to cap their short-run frustration.

All this stems from his doubts, pursuant to the well-known Oxford price queries, whether entrepreneurs "act under the pure motive of profit maximization" (page 139). Yet the entrepreneurs of his image behave in precisely this way, given only a longer price horizon. Plant layout, output, and price are of an order to entail the tangency of the long-period demand and cost curves. But the "equilibrium does not entail the presence of excess capacity. The old doctrine was partial and . . . incorrect, because it considered pricing policy in isolation from investment policy. The new doctrine rests on an integrated theory of investment and pricing policy" (page 162).

Readers who have followed the "full cost" controversy in the *Economic Journal* will be aware of the kinship of Harrod's ideas to those of Mr. Andrews.[5] Interestingly, Harrod contends that marginal and full-

[3] Rightly, and long overdue, he points out (p. 149) that the planning curve yields an erroneous solution if demand changes. Yet this is only part of the case against this technique. See my "Limitations of Planning Curves," *So. Econ. Jour.*, October, 1950.

[4] See Harrod's *Trade Cycle* (Oxford, 1936), pp. 75-76, where he declares that "entrepreneurs arrange matters in the short period, so far as they can, to make marginal revenue equal to marginal cost." Although a qualification is made for user cost, the over-all impression is that of short-run maximization.

[5] See the review article by E. A. G. Robinson, "The Pricing of Manufactured Products," *Econ. Jour.*, December, 1950, and discussion, M. J. Farrell, A. Silberston, and E. A. G. Robinson, *ibid.*, June, 1951.

cost analyses can be joined, with the latter providing the entrepreneurs with "a shorter and manageable way of reaching the same result as he would if he could assess these elusive [marginal] quantities." Compatibility, however, is "subject to the proviso that the entrepreneur dare not charge a price above full cost without rendering his market vulnerable . . ." (pages 161-162).

An integrated theory of investment and pricing, as well as the marriage of the contesting price theories, would, if consummated, merit applause. Yet the ideas promulgated sometimes appear deficient and incomplete. Under the space limitations operative, arguments will have to be brief.

First, there is one form of the theory of long-run maximization that is wholly barren and tautological. At least the theory that firms maximized short-run pecuniary profits was in principle operationally testable. But to declare that firms maximize long-run profits could preclude verification, for in a dynamic setting this fact could only be solidly established on a retrospective probe; even then, if the firm succeeded in blocking competitor entrants, it would be fuzzy without information on the profits that would evolve with entry and adherence to a short-run maximization policy. Only an omniscient mind could test this hypothesis. Of course, it may be alleged that businessmen seek to maximize their long-run profits, not that they succeed. This is a very different theory indeed. Inevitably, questions arise concerning the demand-cost appraisals and deliberative powers of the entrepreneur. Manifestly, the rationality, or even the intuitive judgment, required to validate the long-run hypothesis, considering the greater intricacy of the calculations, requires a higher order of acumen than necessary for shorter period maximization. Ironically, full cost doctrines were proffered in part as a simplification, on the grounds that marginal calculations were too complex for the businessman. Now he is required not only to know the present state of the market and costs, but also the future state of each, and the intentions of other firms as well. Mr. Farrell even has him performing matrix algebra on the argument that "our mathematics provides a fairly close analogue to the sort of reasoning that Andrews describes the businessman as performing." (See his "An Application of Activity Analysis to the Theory of the Firm," *Econometrica*, July, 1954, page 297.)

It does not help to ask the businessman whether his price policy is believed to pay best dividends over time. Presumably he thinks so; otherwise an alternative would be embraced. But this is hardly a theory of long-run maximization in any objective sense. It barely transcends the platitude that whatever he thinks is right, he does. It can, of course, be contended that only a subjective, nonmeasurable—even objectively erroneous—theory of price behavior can explain market conduct. But

it becomes a perverse argument when erected to dismantle the concept of excess capacity, for propositions on the latter phenomenon possess, or should have, an objective flavor.

Harrod's argument, in its pricing dimensions, is not so weak operationally—though the implicit diagrammatic appeals often convey a misleading objectivism—for his entrepreneurs calculate price in terms of unit costs at "normal" operations, with profits included. But the very reference to normalcy, together with the "sufficient" profit allotment, and the acknowledgement of the "jiggery-pokery in the fixing of prices" (page 163), disclose the subjective bias in the pricing technique. Equally normal individuals charged with identical responsibilities need hardly emerge with parallel price results. Curiously, after recognizing multiple-product firms, Harrod argues that the firm's own cost calculations describe what other firms will experience in undertaking the same output. This could be so only within some zone of tolerance and hence provides little reason for firms to price merely to cover costs.

Emphasis on the subjective aspects of the pricing process should in any event finally dispel mechanistic ideas based upon fully discernible objective demand and cost criteria. (Current welfare economics often seems to be based on the faulty premise that these are fully objective functions.) Prices ultimately come to be a resultant of the urges and restraints, the moods of caution and abandon, even the neuroticism of entrepreneurs. Efforts to polish it up and purvey it as a piece of objectivity or business rationality and moderation in other than perfect competition and stationary circumstances are illusive—maybe only occasionally true.

On his own terrain, open in all Harrod's discussion is the exact content of the long-run demand curve, or precisely what operational or clock span it serves to cover. (He declares, on page 151, that the long-run demand curve "has greater elasticity than the short-period curve" and consequently the long-period marginal revenue curve "will cut all the cost curves at points farther to the right" than the short-period curve. If Harrod means that the full-demand curve is more elastic at each point, there may be some error unless he visualizes perfectly elastic long-run curves. What he seems to have in mind is the slope rather than the elasticity.) Reference to Marshall does not immobilize criticism, for even in his work the concept is not particularly illuminating. What seems to be fundamental for Harrod is the time projection before competitors will react, not the time length before capital installations can be modified. If entry is suspected as in the immediate offing anyway, perhaps from an existing multiple-product firm and with a presumedly durable market demand division thereafter,

the theoretical pitch may be described as "long-run," but the effective theory of maximization either in operational or clock-moves would belong to the short-run. Cases where potential competitors among existing multiple-product firms express a willingness to produce a substitute product without actually receiving orders belong to the same family of illustrations; there is in these cases no immanent reason for prescribing a long-run expository format.

Failure to work in terms of overt clock intervals constitutes a major shortcoming of Harrod's analysis. Much of the burden of his argument relies on the hypothesis of complementarity between today's price and future demand, with lower prices today lending buoyancy to subsequent demand. Effective resolution of the pricing problem under this hypothesis requires a disclosure of just when "tomorrow" falls in view of discounting, as well as the exact magnitude of the demand complementarity. In neither respect is Harrod enlightening.[6] Some skeptics might even aver some inconsistency in that Harrod utilizes the Oxford studies to buttress the view that price-wise entrepreneurs adopt a "long view," whereas similar studies revealed that their investment purview was remarkably short, for equipment often under five years.[7] A long-run price theory might then be reasonably short clock-wise, suggesting mainly that price is not altered daily or even monthly. Concretely, there may be other good reasons to refute the assigning of any real temporal depth to the long run. For example, if an imminent depression is foreshadowed, with operations unprofitable anyway, it is obviously wiser to make immediate hay. It would be an act of temerity to counsel an entrepreneur to look far ahead. This is not without bearing if we are destined to live in an era of high employment compounded with prospects of depression. Furthermore, so much of Harrod's analysis seems to pertain to a new firm in the drafting-board stage making an initial pricing decision. Yet a child of innovation may well expect the dynamics to continue and not adopt a distant pricing vista.

Finally, Harrod's normalcy pricing criteria are predicated on the efficacy of entry for curbing price and profit distortions. Over a wide area this hypothesis is simply inconsonant with the facts. In the largest of our corporate entities, this may be among the lesser reasons governing price policy. Conversely, granted ease of entry, as in retail trade

[6] Farrell's treatment (*Econometrica*, 1954) is in this respect more helpful. J. R. Hicks, recognizing much the same point, moves to repair the dyke in "The Process of Imperfect Competition," *Oxford Econ. Papers*, February, 1954. It should be noted that Harrod is smitten by the temporal-complementarity case. Yet where quality is judged by price, lowering price toward "full cost" may reduce future demand. See T. Scitovsky, "Ignorance as a Source of Oligopoly Power," AEA *Papers and Proceedings*, May, 1950.

[7] See H. D. Henderson, "The Significance of the Rate of Interest," *Oxford Studies in the Price Mechanism* (Oxford, 1951), pp. 19-20. Also, George Terborgh, *Dynamic Equipment Policy* (McGraw-Hill, 1949), Chap. XII.

where nonpecuniary motives may dominate or in circumstances where large firms are bent on securing a foothold for alternate products, it is an act of valor for the firm to release the trapped profit-bird for the elusive profit-promise.

The theory obtains its best chance for survival in those intermediate cases where entrants are neither doomed nor undeterred. And here, as argued, its heuristic value is doubtful. We are entitled to more illumination on the temporal dimensions of the analysis, on the concept of "normal" operations and its revisions, on the compatibility of subjective and objective demand and cost phenomena,[8] and of the influence of product and technological innovation on price policy. While his fascination is with the "polypoly-entry" case, customary market categories omitted are pure and nonprice competition—though he declares his argument pertinent to the latter (page 159)—oligopoly, bilateral monoply, as well as monopsony and price discrimination structures.[9] Selling outlays, which do belong in a cost-oriented theory, are barely referred to, as if to uphold the laconic tradition of imperfect competition theories. The omissions serve further to narrow the area of materiality of the theory.

In his final point, on the causes of the divergence of actual prices from optimal norms, Harrod concludes that the mischief comes from increasing returns, which makes imperfect competition inevitable. Conversely, without increasing returns there is actual or potential entry and long-period price equilibrium. And: "I further hold that, with constant returns . . . output will normally attain the social optimum" (page 179).

Leaving aside questions concerning the optimum, the argument appears forced. Whether a firm operates in conditions of increasing returns is partly a result of the number of substitute products that rise to sate the diversity of tastes. For goods catering to a large audience, enforced standardization and rationalization might render decreasing costs less likely. But it is the very diversity of tastes which provides opportunities for new entrants, and it is this—the entry—which causes increasing returns rather than, as Harrod sees it, increasing returns

[8] This problem is generally ignored by those who emphasize the subjective nature of demand and cost constructs. I tried some time ago to analyze the problem but there seems to be a vast disinterest amongst price theorists in this question. Cf. my *Price Theory* (Pitman, 1949), Chap. 16.

[9] There is a tantalizing minor paradox here in that Harrod's emphasis on entry presumes an oligopoly aspect while his analysis of the pricing policy is premised on polypoly. Though logically impeccable, it seems more likely that where a firm fears new entrants, it already has close competitors, so that price making does partake an oligopoly character. Cf. M. E. Paul, "Notes on Excess Capacity," *Oxford Econ. Papers*, February, 1954, p. 39. Also Hicks, *op. cit.*, p. 49. Harrod's neglect of the oligopoly problem is noteworthy considering his flirtation with a Cournot solution as early as 1934. See his "Equilibrium of Duopoly," *Essays*.

being instrumental in preventing entry. This is a standard Chamberlin position and it lends credence to his protest that he has been misunderstood by his British friends. (See *The Theory of Monopolistic Competition* [6th ed.; Harvard, 1948], pages 108-109.) Once producers do become numerous, each closeted with a different clientele, then the refrain of too many firms, too many entrepreneurs, each producing too small an output—and thus excess capacity—becomes faintly audible, gaining in volume when the firms are detected operating at outputs below minimum average cost. It follows that the doctrines of excess capacity may hang on, not greatly shaken, much less shattered. In the illustrative form of nonprice competition in which Chamberlin presented it, surely Harrod has failed to dethrone it. (*Ibid.*, pages 193-194.)

Whether with constant returns output will be optimal seems to me doubtful. Apparently Chamberlin's arguments are, in this context, being rejected or ignored. As I accept them, I need only refer to his position. (*Ibid.*, pages 196-199.) Of course, much hinges on the interpretation of "normally" and "optimum." An equilibrium with sales outlays, fewness, nonprice competition, patent grants, and deceptive product variations seems remote from the welfare economist's ideal.

III

In a mood of self-deprecation and recantation, Mrs. Robinson prefaces her article by declaring that her significant volume, which endowed us with much of our market insight and analytical technique, "was a scholastic book" and not "a suitable basis for an analysis of the problems . . . which present themselves in reality" (page 579). Various aspects of the theory of market structures then pass before her pen as she expresses her current attitude.[10] The treatment can be brief, for her work is generally prescriptive and the discussion not exhaustive.

Some opening definitional remarks on industries and markets may be puristically unacceptable. The former is envisioned as consisting of "firms . . . alike in their methods of manufacture," for this "represents the area within which a firm finds it relatively easy to grow," while markets are comprised of "the demand for . . . commodities which are close substitutes" (pages 579-580)—essentially a "group" concept. Then Mrs. Robinson is moved to recommend that we recognize the prominence of the multiple-product firm and the importance of the theory of price discrimination, for while "dropping the fiction of one-commodity firms destroys the simplicity of the analysis of imperfect

[10] Quotations are from her "Imperfect Competition Revisited," *Econ. Jour.*, September, 1953.

competition," it simultaneously "enlarges its scope" (page 580). These are, I think, reforms long overdue to bring our theory closer to industrial facts. They need not render the presentation more abstruse in every case, for the analysis of single-product firms can be retained and applied to multiple-product firms whose outputs are independent on the cost side.

In some blithe paragraphs on "Who Maximises What," Mrs. Robinson sides with the retention of the entrepreneur concept, not as a concrete image but as a personification of a firm in its policy-making aspects. Opinions are likely to differ on descriptive details rather than upon the expository need for the construct.

It was Mrs. Robinson who originally made us face up to what she then called the "fundamental assumption," to be caricatured by some,[11] as pecuniary profit maximization. Appraising the aims of the firm anew, she argues that these are, first, to survive and, secondly, to grow. Both objectives invite a long-run maximization hypothesis. Critics are chided to produce a better premise. On the size of firms, she opines that there is no unique optimal size; some interest, however, attaches to her descriptive comments on growth. Confessing to having been baffled by the oligopoly problem originally, her present paper is disappointing in its failure to devote to it more than the few lines acknowledging her earlier bewilderment.

Reflections on the forms of market competition carry her into the Chamberlin camp, declaring that "the assumption that price is the main vehicle for competition is a great over-simplification of reality" (page 584). She lists the usual types of competitive maneuvers, including differentiation, sales promotion and advertising, price, as well as the legerdermain of raising price to impart a quality aura or snob appeal to a good. All this, detail aside, is already modern doctrine mainly due to the efforts of Professor Chamberlin.

Observing that profit-maximizing calculations are unlifelike, in the most significant section of all, on "Price Policy," Mrs. Robinson argues that in a continuing economy prices are only altered when the reasons are obvious. Her reappraisal of the "marginal revenue equals marginal cost" rule encompasses the following cases: (1) A new product or line for an existing firm: the conception is deemed overformal rather than misleading. (2) A multiple-product firm producing both competitive and noncompetitive products: she concludes that the assumption does not fare too badly. (3) A change in costs: under price leadership and oligopoly the maximization analysis is ineffectual. (4) New products: price policy, she argues, will be contingent upon costs to potential

[11] See T. W. Hutchison, *The Significance and Basic Postulates of Economic Theory* (London, 1938), Chap. IV.

rivals; she refers to Harrod's argument as pertinent. (5) Cyclical fluctuations in demand: competitive and oligopoly prices are distinguished where the latter leads to priorities in lieu of price rises on the upswing, while "full cost" and "spoiling the market" conceptions interfere with adjustments on the downside. Concluding, in a passage to which many of us will subscribe, she remarks that "it seems clearly impossible to replace the old text-book slogans with any simple generalizations. A debate which consists in defending or attacking 'principles,' such as the 'full-cost principle,' the 'marginal principle' or the 'normal cost principle' and trying to fit all types of situations into one system is obviously foredoomed to futility" (page 590).

On the concept of equilibrium, Mrs. Robinson arraigns much of contemporary work, in a manner reminiscent of Marshall, with the plaint that "in my opinion, the greatest weakness of *The Economics of Imperfect Competition* is one which it shares with the class of economic theory to which it belongs—the failure to deal with time" (page 590). Unfortunately she does not move the discussion very far, though she infers that a simple Marshallian hypothesis will not serve, for "there is no such thing as a position of long-run equilibrium which exists independently of the course which the economy is following at a particular date." Irreversibility is pronounced as the rule, for past decisions affect the content of the future equilibrium. This should not have required saying, except that Marshallian long runs appear sometimes to be divorced from the (recent) adaptative path. Mrs. Robinson declares pointedly that "the very notion of long-run supply curve, in its usual simple two-dimensional form, ceases to be admissible" (page 591).

In closing, Mrs. Robinson inquires into the causes of monopoly and declares that "the chief cause . . . is obviously competition," with the vigorous firms absorbing the weaker ones. "Vigorous" remains to be defined, for strength possesses several facets. As a twist on the Galbraith theme, she also renders the judgment that monopoly at one stage of nonintegrated output fosters competition at other stages. "The second main cause of monopoly"—enforcing discipline and loyalty to a price leader—is a "severe decline in demand." And "it is much easier to think of causes for monopoly and examples of monopolies being formed, than it is to think of causes or examples of monopolies breaking down" (page 593). Mrs. Robinson pursues the theme that there are only "narrow areas in the economy where conditions are such that competition can normally prevail" in "The Impossibility of Competition," in *Monopoly and Competition and Their Regulation* (Macmillan & Company, Ltd., 1954), E. H. Chamberlin, editor. Schumpeter's "creative destruction" by new monopolies replacing old ones

and Galbraith's countervailing power seem by accident or design to have eluded her. Her conjecture, however, does deserve more critical attention with empirical investigations providing the clues.

IV

Through Professor Chamberlin's reconsideration runs the strong faith that he has given his intellectual offspring a good constitution, though some mistakes in training are acknowledged. (See "Monopolistic Competition Revisited," *Economica*, November, 1951.)

The vital economic organ—"our economic atom"—is proclaimed to be the firm, with U-shaped cost vertebrae in both the short and the long run. The dissolution of pure competition is attributed to both economies of scale and diversities in tastes which "result in one producer for each product, with often a complicated set of oligopolistic relations between them. This is a far cry from pure competition . . ." (page 349).

Whereas by tradition entry is extolled as in the public interest, Chamberlin writes that this matter "urgently requires comment." Citing patents, trade-marks, colonial trading privileges, and public utilities, he deduces that "from these examples . . . it is not a self-evident principle of public policy to eliminate all such impediments to entry. Indeed they could not be completely eliminated, any more than all men could be made equal in ability or merit by legislation" (page 351). Whetting our appetite, Chamberlin omits a supporting elaborative sustenance.

On his own views, he undertakes to remove "two misinterpretations"; to wit, that " 'tangency' has been associated *only* with the 'group problem' " and the belief that the isolated monopolist "has been held strictly apart from the theory" (page 353). Both views, he alleges, are mistaken. The first error, surely, should never have been committed: a public utility may find profits barely possible only at one rate structure and output: certainly new entry need not be an indispensable prerequisite for the zero-profit result. Likewise the isolated monopolist, interpreted as a firm whose cross-elasticities of demand vis-à-vis other individual firms is small, obviously fits into his analytic scheme though the problems engendered are rather tame compared to the circular structures.

Chamberlin then develops a point on which, in my view, confusion is even more rife, judging from the textbook tradition and teaching practice. He assigns the sloping demand curve for the firm's product to the diversity of tastes. Thereupon, he finds nothing "ideal" about the minimum AC point and its associated output. In his words: "Such a programme [of enlarging output in each firm in order to minimize

average costs] . . . indicates nothing so much as a complete misunderstanding of the problem" (page 354). Manifestly, if the demand for the firm's product cuts AC to the left of its minimum, it would be pointless to compel the production of the minimum AC volume of output. Yet it is not uncommon to find this output level labeled "optimal," as if the concept has meaning isolated from a demand context.

In a more conscious time framework, more might be said on this point. For example, suppose that in the given time interval the maximum profit output was precisely one-half that of the minimum AC output—a hypothesis not too far-fetched to judge by some diagrammatics. If the AC disparity exceeded carrying costs on the stock, it would seem to be prudent for the entrepreneur to produce the larger quantity, doling out in sales (approximately) the monopoly-maximum output in each time period. In the interim—the "idle period"—the firm could devote its facilities to some alternate outputs. Real problems and solutions of this sort are concealed in a stationary framework but become obvious in a time-dress analysis.

Having originally placed the oligopoly problem before us, Chamberlin suggests that "the subject needs to be rewritten in terms of . . . cross-elasticity of demand, rather than in terms of the number of sellers in a market" (page 355).[12] This is likely to be merely definitional, elucidating criteria for the recognition of the problem rather than providing a guide for its resolution. There follows a salutory statement that insofar as a problem signifies "indeterminateness . . . it is certainly the job of the economist as a scientist to say so" (page 355). Some economists have persuaded themselves to the higher virtue of ignoring these situations for the very same reasons, using models of pure competition as their analytic guide. It is not by accident that their policy recommendations often lack relevance.

Discussing cases in which entry into an oligopoly sphere multiplies the number of firms and products and raises unit costs and prices, Chamberlin concludes that this upshot appears to represent "genuine waste" (page 356). More might be said here. Situations can be conjured in which this may occur and yet the product diversity cannot be so simply interpreted. Consider a new product which weans away so many customers from the old products as to drive up their prices. From the paucity of information and the enigmas of interpersonal

[12] In a more recent comment on R. L. Bishop's use of the measure, Chamberlin writes that "Bishop's major contribution . . . is a convincing demonstration of the complexity, even the treachery, of cross-elasticity as a measure of anything." (*Am. Econ. Rev.*, December, 1953, p. 911.) This puts a new complexion on the earlier remarks. Thus: "I . . . hold that the individual seller is isolated under . . . differentiated competition; also under pure competition; and that the essential distinctions are between heterogeneity and homogeneity, isolation and nonisolation" (p. 913).

comparisons, I do not think that we are entitled to conclude that well-being has suffered.

Infringing upon Schumpeter's trade-mark, Chamberlin states that between them "with respect to the role of monopoly in the process of economic development there is agreement." Monopoly stimulates innovation and then the dilemma: to prevent the monopolist from becoming "fat and lazy," there is required "both monopoly and competition . . ."—presumably of the monopolistic variety. This problem, too, I think can only be handled on a dating approach with the life of fixed equipment and the rapidity of technological change constituting the crucial elements.

In summarizing, Chamberlin explains that concepts involving numbers—almost the very language of product differentiation—are to be dropped from the technical vocabulary, to be replaced presumably by a proliferation of cross-elasticities. These austere gestures represent concessions—since withdrawn?—to precision and purity. As they strike a blow at concreteness, my guess is that in verbal accounts they will enjoy a hardy life. There is, too, the remark on entry, that "the problem . . . disappears along with the group, and becomes absorbed into the larger problem of the distribution of resources throughout the entire economic system" (page 362). As it stands, this is obscure and mystifying. Undoubtedly the relations are tangled, as are all general equilibrium matters. Yet there are some peculiar problems of new firms and resource organization under uncertainty which form part of the theory of entry but are not normally treated with the general theory of resource allocation. I see no easy way of excommunicating the theory of entry from particular equilibrium analysis. Perhaps Chamberlin will elaborate his ideas. In the same way it is an exaggeration, undoubtedly harmless, when in his final paragraph he declares that in the new conception "oligopolistic relations . . . [emerge] with greater force, as [do] . . . the full cost principle and other departures from profit maximization, and the relation of the theory to problems of development and growth" (page 362). I doubt that all this occurs merely through quantification of market interdependence via the cross-elasticity of demand. Also, as we have seen, the presumed departures from profit maximization normally constitute only poorly defined long runs substituted for short horizons.

V

Leaving the triopoly which has mixed the heady blends of "imperfect theory" of the past, I should like to turn to the surprisingly uniform doctrines on competition expressed at last year's meetings, written largely from an empirical base. The group already discussed shared,

initially at least, a common belief in real world departures from textbook purity of competition, and a parallel analytic attitude. A recoil from the fascination with monopoly diagrammatics and a new passion for competitive data, for some time inhibited, seems to have been released last year in a concerted move to turn the doctrinal cycle.

In mind, in essence but not detail, are the three papers by G. Warren Nutter, Richard B. Heflebower, and Arnold C. Harberger.[13] A penalty of some injustice to each of them is a price of brevity. However, I do not believe I misrepresent the spirit of their writings.

Last year we were told by Nutter that "it makes sense to say that competition is the normal condition in our economy. Theory should be brought back into touch with the world we live in by recasting monopolistic elements in their proper role: as a special, not the general, case." (AEA *Papers and Proceedings,* May, 1954, page 76.)

On the basis of his longer study he reported that segments of agriculture, mining, and manufacturing, accounting "for perhaps 10 per cent of national income," represented activities in which "competitive behavior . . . is continuous and prompt" (page 75). Industries classified as oligopolistic "account for as much as 35 per cent of national income," divided between those in which pressures of potential competition are strong (15 per cent) and the remainder (20 per cent) constituting "noncompetitive oligopolies." Thus "from these rough estimates, it would seem that about 45 per cent of national income is produced under continuously competitive conditions and an additional 25 per cent under irregularly competitive conditions" (page 76).

Heflebower remarked that "the general tenor of the argument, both as to the variables stressed and much of the empirical evidence referred to incidentally, is to suggest a strong tendency for economic results of industrial markets to be closer to the optimum than one would conclude from much of the literature about this sector of the economy" (*loc. cit.,* page 135). And, he adds: "It seems to me both evident and understandable, once one is versed in the whole of the structure and operations of modern industrial markets, that their operating results approach more closely the ideal than one would surmise to be true from textbook models of oligopoly and monopolistic competition or from the literature on concentration" (pages 138-139).

His opening sentence in the recent volume edited by Chamberlin

[13] I omit the slightly older lecture of G. J. Stigler, "Competition in the United States," in his *Five Lectures on Economic Problems* (London: Longmans, Green, 1949). His view that "competition declined moderately from the Civil War to the end of the nineteenth century, and thereafter increased moderately" (p. 54) was more tentative and less doctrinaire—in this place—despite the statistical scaffolding, than the authors quoted. Omitted also is Clair Wilcox' "On the Alleged Ubiquity of Oligopoly" (AEA *Papers and Proceedings,* May, 1950) in which he concluded that oligopoly was not "the ruling market form in the modern economy," contrary to views expressed by other writers.

reads: "Competition outweighs monopoly by a wide margin in the American economy today." ("Monopoly and Competition in the United States of America," in *Monopoly and Competition and Their Regulation,* 1954.) And though in resource adaptation, profit reduction, and income division, "the situation might be better," still "the economy as a whole, as operating now, reflects the presumed benefits of a competitive market system" (page 111).

Harberger, in an effort "to get some quantitative notion of the allocative and welfare effects of monopoly" (AEA *Papers and Proceedings,* 1954, page 77), confides, after identifying "monopoly power with high rates of profit" (page 84), "that monopoly misallocations entail a welfare loss of no more than a thirteenth of a per cent of the national income. Or, in present values, no more than about $1.40 per capita" (page 85). And: "Adjustment of our results to allow for a maximal distorting effect of advertising expenditures would accordingly make only a slight difference, perhaps raising our estimate of the welfare cost of monopoly in present values to $1.50 per capita, but not significantly higher" (page 86).

Appraising his calulations he feels they "tend to overstate" the "welfare loss due to monopoly misallocations of resources" (page 86) so that "monopoly does not seem to affect aggregate welfare very seriously through its effect on resource allocation" (page 87).

It is difficult to know what Nutter has in mind for the "general" case; surely it cannot be "pure" competition, much less perfect competition. For purity, at the firm's level, did entail adaptations which can hardly be true of industries such as agriculture and mining—often described as "sick" industries—and manufacturing sectors such as textiles. One of the virtues of modern analysis is its ability to cope with all market structures with a highly unified technique rather than exhibiting a predilection for one market arrangement as *the* descriptive form, as Nutter seems to be in danger of doing.

Heflebower goes even further in suggesting the proximity of our present world to the "ideal." Without information on the content of the latter, it is impossible to accept his judgment. What is the ideal output when user-cost calculations are recognized as reflecting a variety of opportunity costs? What is the ideal commodity-chain in an economy in which consumer demands for diversity prevent firms from exhausting decreasing cost opportunities? What is the ideal volume of sales outlays? What is the ideal rate of commodity innovation and product variation? The technological displacement of labor? To raise questions of this sort is to expose our ignorance. Ruefully, most of our welfare economics and optimal pricing propositions belong to a stationary model; the firms which Heflebower observes operate in a

more perturbed world. His assertion that "competition outweighs monopoly by a wide margin" is valid only where both terms remain vaguely defined.

I forbear comment on Harberger's calculations. Further study might lead to a revision of his estimates by another few pennies. Maybe even those who have argued that the economy approximated models of pure competition "in the long run" and "in equilibrium, excluding friction," were astonished at this remarkable statistical confirmation. I fear its statistical riches are too convincing, even embarrassing in providing too much.

In this whole discussion it is hard to resist quoting Chamberlin. Writing on attempts to classify "industries" as being monopolistic or competitive, he remarked that "the practice of classifying such 'industries' as *either* competitive *or* monopolistic . . . seems to me comparable to classifying all members of the human race as either geniuses or idiots." (*Monopoly and Competition and Their Regulation*, page 262.)

I do not propose to challenge the competitive-monopoly allocations of these writers at this place. Nevertheless, the determination that "only 20 per cent" of the income is being produced by noncompetitive oligopolies is not to be construed as signifying that resources are properly allocated in 80 per cent of the economy. There is a structural multiplier at work here; if industry A is classified as noncompetitive, not only are there resource maladjustments in A, but in all its flow-suppliers, and suppliers of suppliers, including the capital goods industries which provide equipment. In the same way, it is in error to argue that because income originating in advertising is only a minuscule part of national income it is unimportant. Without these earnings, many publications, radio stations, programs, etc., would cease; a series of interdependent incomes would topple, to say nothing of a reshuffle in demand. Demand for newsprint and paper, for printing presses, electronic devices, and certain transportation services would seriously dry up. The ultimate upshot would be a multiple of the small percentage of resources devoted to advertising as reflected in national income calculations.

VI

In summary, it seems to me that Harrod has oversimplified the pricing process. A cost-centered theory, long or short, can only be partly valid. Coerced to an arbitrary choice, a short-run maximization hypothesis is not without merit: it is reasonably definite and, considering that recent corporate profit and inventory valuation allowances approximate 25 per cent of current income originating in corporations, it does not seem too wide of the mark. If profits are not being maximized

there is real cause for alarm at the potential exactions from a psychological drift toward a short view.

Mrs. Robinson's recommendation that we work with a multiple-product firm ought to be welcomed. It is a pity that Professor Chamberlin failed to mention it, for it can be a realistic antidote even in the introductory phases of the theory of the firm. It follows that I prize highly the suggestion that the theory of price discrimination be brought to the fore, not as a special case, but one with wide applications of its own; substitute products turned out by multiple-product firms constitute an immediate area of relevancy. Further, in a temporal framework selling often partakes of the nature of discrimination over time, with the theory of inventory and speculation being essentially special cases in the broad genus. In this development the theory of bilateral monopoly also comes more securely into its own, sometimes as a form of discrimination inasmuch as sellers often deal with each buyer singly.

Mrs. Robinson's reference to time, in the clock sense, as well as her derogation of the long run, arouses interest. Yet she has not outlined her ideas adequately and there may be some clash in her espousal of long-run maximization simultaneously with condemnation of long-run analysis. This remains to be cleared up. Personally I should like to see a "dating approach," in the manner of the Swedish contributions, with the production-sales sequence occupying the center of the stage. I once tried to set out some plausible patterns which yielded interesting results, but I alone appear to have been intrigued by them. (See my *Price Theory*, Chapters 17-18.)

Though Professor Chamberlin's championing of the firm as the basic unit is generally accepted, and it ought be the multiple-product firm, fewer will acknowledge the U-shaped AC curve. A unanimous report here is unlikely, at least over the effective output-sales ranges. Notions of entry remain, as he articulately informs us, to be investigated anew, for entry being deleterious to economy on other than industry-fragmentation arguments encounters some natural resistance. Welfare economics may be especially delinquent for its shyness in diagnosing the problems created by entrepreneurial product variation.

Above all, the examination of monopoly in the dynamic milieu is sadly incomplete. Chamberlin's remarks are too brief and though we do have the ideas of Schumpeter, more remains to be said. Otherwise there is the danger of being swamped by the opinion of practical men that monopoly alone insures progress.[14] As a historical proposition, in an age of research staffs, something may be said for it, though it is

[14] See P. Hennipman, "Monopoly: Impediment or Stimulus to Economic Progress?" in *Monopoly and Competition and Their Regulation*, for some review of the literature.

not a distillation that springs from careful research, for breaches in the fence of advance can be assigned to the same cause.

All seem to agree—though perhaps not Harrod—that the tangency solution is not vital to the analysis. As anything other than an *ex post* addition of profit magnitudes, or a special case, this is a relic of a faith in an ultimate stationary repose. Here again we tread on the conflict of long- and short-run analysis. Proponents have probably already chosen up sides.

Surprisingly, there has been no mention in these "revisits" of the role of funds and money markets as a possible limitational factor on the firm's activities and production decisions, despite the participation of two such gifted macroeconomic expositors as Mrs. Robinson and Mr. Harrod in the debate. It must always seem puzzling that the entrepreneur is conceived as maximizing profits, which entails access to loan funds, and yet problems of borrowing, of loan credit, and capital rationing are rarely mentioned in price-production analyses. Despite disclaimers, apparently we really do visualize unlimited credit or barter modes of compensation to factor-owners.

All three, it seems to me, despite recognition and reference, still place inadequate emphasis upon the subjective aspects of the pricing and production process.[15] Usually—and this is true of most of the literature—either subjective or objective demand-cost schedules are recognized, but not both. Harrod's concepts turn out to be pieces of subjectivism. Objective facts should somewhere enter the scheme and the compatibility of the two ought be demonstrated.

[15] Compared, say, to the cogent emphasis of F. Machlup, *Economics of Sellers' Competition* (Johns Hopkins, 1952), Chap. 2.

DISCUSSION

FRITZ MACHLUP: We have been assured lately that we do not have to be afraid of monopoly in this country. Specifically, we have been told: that competition is the general case and the normal condition and its performance is not far from the ideal pictured in the old theories; that we have enjoyed a fine rate of technological progress thanks chiefly to our monopolies financing research and innovation; that thanks to big business or monopoly we enjoy a larger scope for freedom of choice and can satisfy a greater diversity of tastes; that bilateral monopoly is a blessing taking the curse off most of the unilateral monopoly that has sprung up; and that whatever monopoly we have costs us only $1.50 a person per year. Thus, we can rest assured and forget our persecution complex, and perhaps also our prosecution zeal against suspected antitrust law violators.

The two speakers in today's session do not share such views; they seem to take the social cost of monopoly, the danger of reduced competition, and the advantages of vigorous competition much more seriously. Professor Clark tentatively lists "public policy expressed in the antitrust laws" among necessary conditions of vigorous competition and he seems to advocate that collusion be checked and the competitively-minded individualist be protected. He does not accept the view that countervailing power is as good as competition, he objects that a major part of competitive behavior—bargaining with all your might—was "annexed" as a phase of countervailing power, and he states that under bilateral monopoly "almost anything might happen."

Professor Weintraub does not accept the view "that monopoly alone insures progress." And although Professor Clark mentions that "atomistic industries" would lack the funds as well as the incentive to undertake heavy research programs, he emphasizes the function of competition in the diffusion of the benefits of progress—in re-creating incentives for new ventures after eroding the profits from the previous ones. His remark that incentives to innovation could not exist "if the diffusion were instantaneous and complete" is probably intended to convey his opinion that the innovator's head start would be insufficient if imitators were not delayed by some institutional barriers such as the patent system. But how long a delay would be appropriate to achieve optimum rates of innovation and of diffusion of benefits? As I have stated in my *Economics of Sellers' Competition* (1952):

> To buy innovation by paying with unnecessarily long delays of imitation is a poor bargain for society to make. Imitation always and necessarily lags behind innovation. It will be the best deal from the point of view of society if innovators optimistically overestimate this lag. If they expect the lag to be longer than it actually is, innovation will be enhanced and imitation will not be delayed. That it may create this socially wholesome illusion on the part of innovators is the strongest justification for a well-designed patent system. (Page 556.)

The worst bargain for society would be patent laws or other monopolistic barriers which protect the innovator better than he thinks they do. For this would delay imitation without stimulating innovation accordingly. While Clark does not discuss this problem, his comments on the need for the erosion of

profits by competition and their re-creation through further innovation indicate to me that he might accept my way of thinking on these issues.

Weintraub is critical of the views of the all-is-well economists. He does not examine the cost-accounting methods behind the surprisingly low estimate of the welfare cost of monopoly, but he reminds us of the fact that Harberger identified monopoly power with high rates of profit. Many writers on the subject have insisted that monopoly is reflected less in increased profit rates than in increased or inflated costs. A monopolist may pay more than the competitive price for productive services; he may use them inefficiently; he may have transformed his monopoly rents into money expenses; he may have capitalized them, inflating the base on which his rate of return is calculated. (See my *Political Economy of Monopoly* [Johns Hopkins Press, 1952], pages 490-497.)

High production cost behind monopolistic barriers, such as tariffs, patents, building codes; high depletion cost in mineral monopolies; high management cost and high labor cost in enterprises sharing their profits with management and with their labor force—these are some of the economic consequences of monopoly excluded from Harberger's estimate of its welfare cost.

Not in such political or evaluative implications, however, but rather in their theoretical contributions lies the chief value of Clark's and Weintraub's papers. Weintraub's survey of what Harrod, Joan Robinson, and Chamberlin had to say when they revisited their earlier explorations is highly suggestive. Clark's "dynamic criteria of appraisal" of the goals and performance of competition is a piece of theorizing distinguished by a rare combination of analytical technique and mature wisdom.

The call for dynamic, more dynamic, and fully dynamic theory is repeated over and over again. I fear that the dynamics of the meaning of the word "dynamic" is too great for the term to be of continued usefulness if we want to communicate and not only to orate. Clark wants to communicate, I know, and therefore I take the liberty of commenting a bit on his choice of language.

He speaks of a gap between theories of equilibrium and theories of growth and development. As I see it, equilibrium is a tool of thinking needed to explain change, no matter whether it is change in market prices or in business investment or in employment or in the rate of growth. Equilibrium is a valuable concept both in models which disregard time lags—static theory—and in models which present sequences in time—dynamic theory. Since a theory of growth will ordinarily use the equilibrium concept, there can be no contrast between theories of equilibrium and of growth. What Clark meant, I suppose, was the contrast between "static theories of price and output" and "theories of growth and development."

Price theory itself may be static or dynamic. I once gave a graphic description of a "dynamic or time-sequence analysis of effects of a selling campaign," in which five different outcomes—different sales volumes at one price and different prices for one sales volume—were shown to result from a given selling effort, depending on the chosen sequence of actions and on the time intervals between the steps. The five points would all coincide if sequences and intervals of time did not matter and demand curves were assumed to be reversible

(*Economics of Sellers' Competition*, pages 185-189). Whether the complicated dynamic model has to be used or the simple static model will do, depends on the kind of problem at hand; that is, not on whether sales actually depend on the time sequences and intervals but on whether the differences really matter for the kind of answer required. Clark gave a very similar description but did not say whether he believes that the dynamic model is always superior. If he thinks so, I would disagree. Dynamic theory is better only where it is needed; where the analytic job can be done with the simpler, less realistic, static theory, the latter is preferable.

"Fully dynamic theory," Clark says, "must accept indeterminateness." I suppose he means to suggest this as a definition; namely, that a theory which is both dynamic and indeterminate should be called "fully dynamic." I do not know whether this would be helpful. Oligopoly theory, to be sure, will usually have to be dynamic, will have to make large allowance for uncertainty in the anticipations of sellers, and will have to leave a large scope for indeterminateness. (See my chapter on "Oligopolistic Indeterminacy," *op. cit.*, pages 414 ff.) I should prefer to use language which does not mix the dynamic elements, the uncertainty elements, and the indeterminateness, since all these are different things that ought to have separate names.

There is no disagreement of substance between Clark and myself on the dynamics of oligopolistic competition. I find his emphasis on the time interval between move and countermove as a key element in the theory of oligopolistic competition most appropriate and significant.

The tangency theorem comes in for a good deal of criticism, and in at least one respect Clark's objection coincides with Harrod's, reproduced by Weintraub: businessmen are not so shortsighted as to overlook potential competition; hence they may ward it off and secure larger sales volumes by charging lower prices. I believe that the proponents of the tangency theorem had only the case of a polypolistic seller in mind, who would regard himself as too small to arouse any responses on the part of competitors, old or new. The theorem was not meant to apply to the case of oligopolistic competition under the pressure of new entry, actual or potential.

Clark's remark on product differentiation as a competitive device confirms my own conclusions that quality competition can be quite vigorous and that product differentiation may weaken but will often invigorate competition. I also showed that product standardization may be an aid to price competition, but is often an aid to price maintenance and other forms of collusion (*ibid.*, pages 163-168). But may I take exception to the way in which Clark put his comment? He said that "theory appears to regard product differentiation as always a shelter from the rigors of competition." My theory, for one, does not. Clark obviously meant "some theorists." After all, no theorist is appointed or ordained to speak in the name of "theory."

Twice in his written paper Weintraub made bitter or disconsolate remarks about the fact that his fellow economists have sadly neglected certain important points he had made in his book on *Price Theory*. I know exactly how he feels. When I first read Clark's paper, at eight different points I cried out: "Oh

why has he not read my book! ! !" Well, I suppose, there just is not enough time to read all we want to read.

And perhaps I may turn the tables on Professor Weintraub and ask him whether he has read what Milton Friedman recently wrote about that funny ambition of some social scientists to verify the fundamental assumptions of a theory instead of the conclusions deduced from them? I am asking this apropos Weintraub's remarks about "operational testing" of the assumption of profit maximization. After complaining about the impossibility of the assumption of long-run profit maximization, he says: "Of course, it may be alleged that businessmen seek to maximize their long-run profits, not that they succeed. This is a very different theory indeed." It "is hardly a theory of long-run maximization in any objective sense. It barely transcends the platitude that whatever he thinks is right, he does." I cannot resist quoting the answer which I gave to this a few years ago:

> My emphasis on this methodological "subjectivism" has been widely criticized. One of my critics believes that "refuge in subjective interpretations of the cost and revenue functions . . . leaves theory saying that businessmen do what they do because they do it." (R. A. Gordon, . . . AER, 1948.) This is a misunderstanding. The businessman in the model acting upon marginal principles will react, say, to an increase in the price of fuel oil, not just by doing anything that may come to his mind, but by making some "typical" adjustments in his production, input, output, and prices. The point is that, in order to predict or explain the type of adjustment, we do not have to know the subjective whims and guesses that may have influenced his initial position. For, happily, the direction of the "equilibrating" (adjusting) action is to a large extent independent of the exact position of "equilibrium" from which we assume the businessman has been pushed by the "disequilibrating" change. (*Ibid.*, page 36.)

I propose a pact to Weintraub: that we shall read each other's books and discuss our disagreements.

KERMIT GORDON: If I sense correctly the tone of Mr. Weintraub's paper, he is disturbed by what he regards as the immoderate character of the current wave of revisionism in both the theory of markets and in the appraisal of market behavior in the American economy. I share his uneasiness.

The pendulum was bound to swing, and a swing in the present direction was, I think, salutary. Abuses were unquestionably committed in the name of the new theories of competition which were unveiled in 1933. Illustrative firm demand curves were sometimes drawn with a slope such as to suggest that in group equilibrium the scale of the firm would be comparable to that of an itinerant peanut stand. The assumption of short-run profit maximization was accepted much too uncritically. There was attributed to entrepreneurs a precision of knowledge with respect to their cost and demand functions which they obviously do not possess; witness Mrs. Robinson's recent acknowledgement, by way of expiation, that the firm's demand curve "is a mere smudge, to which it is vain to attribute elegant geometrical properties." The concept of the "group" or "industry" was taken much too literally, with the result that the analysis of substitutability relations was unduly circumscribed. And, of course, the whole analysis suffered from being conducted in a static framework.

But all of these matters are commonplace and have been for some time now.

What concerns me—and I think Mr. Weintraub—is the sense that the wave of revisionism has developed a momentum which threatens to carry it into some salients which cannot be held. There is even the possibility that some of the current themes may be mutually inconsistent. Two of these themes, for example, may be linked in the following curious proposition: the pervasiveness of monopoly power in our economic system has been favorable to a rapid rate of technological progress, and anyway, there is very little monopoly power in our system. I would not hold that the two parts of this proposition are necessarily irreconcilable, but I would maintain that those who assert both parts to be true have some explaining to do.

Mr. Weintraub examines at considerable length the views expounded by Mr. Harrod in his recent essay, "Theory of Imperfect Competition Revised." A reading of this essay leads me to ask, I suspect with Mr. Weintraub's approval, "Are these revisions necessary?"

Mr. Harrod would like to erect in the very center of price theory the concept of limit pricing to deter entry. In his view, entrepreneurs may be counted on to set prices at levels sufficiently below those indicated by the geometry of short-run profit maximization to render their markets unattractive to potential rivals. This principle leads Mr. Harrod to discard the doctrine of excess capacity and enables him to demonstrate the compatibility in certain circumstances of full-cost pricing with marginalism.

There is, of course, nothing very new or startling about the proposition that limit pricing is a fairly common phenomenon; it may be found in the textbooks. Serious doubts are raised, however, by two facets of Mr. Harrod's analysis: the apparent assumption that the full exploitation of short-run profit opportunities will, by attracting entry, generally entail the sacrifice of a greater sum of discounted future earnings, and the view, implicit rather than explicit, that limit pricing is typically employed for the purpose of deterring entry.

Mr. Weintraub has discussed the first of these points at some length, and I would associate myself with his misgivings. However misleading it may have been to imply that entrepreneurs typically act on the maxim, "Gather ye rosebuds while ye may," there are nevertheless important questions to be asked about Mr. Harrod's presumed substitute, "A stitch in time saves nine." One would want to know a good deal more about how many stitches are in fact saved by a stitch in time (it puts no strain on the imagination to think of cases in which nine stitches in time might conceivably save one); about the distribution over time of the stitches saved; and about the rate of time preference applicable to these stitches.

Mr. Harrod rests his case for limit pricing in part on the proposition that "the uncertainties of the future have a tendency to restrain the monopolist from raising his price much above a competitive level, even if all known factors indicate that he could do so with safety." But doubts arise if we look for the causes of this uncertainty. It would be popular current doctrine to hold that entrepreneurial uncertainty about the future arises in large part from fear of the "kind of competition which counts"—in Schumpeter's words—"the competition from the new commodity, the new technology, the new source of supply,

the new type of organization . . . competition which commands a decisive cost or quality advantage and which strikes not at the margins of the profits and the outputs of the existing firms but at their foundations and their very lives." But does this kind of anxiety foster limit pricing? Will the voluntary narrowing of profit margins fend off this kind of competition? Would moderation and restraint in the pricing of ice have stopped the electric refrigerator? I think not, and I would suggest the odd hypothesis that the perennial-gale doctrine is at least as easy to reconcile with a relatively short-run theory of profit maximization as with a long-run theory.

The second basic reservation I have about Mr. Harrod's analysis is the feeling that the motives for limit pricing are much more complex than he suggests. On the basis only of the most casual and unsystematic observation, I would hazard the guess that the conscious design to discourage entry is present only in a small proportion of those cases in which limit pricing occurs. One thinks immediately of the concentrated sector of American manufacturing industry in the two or three years following 1945. Limit pricing was widespread, but in many of these industries any serious fear of entry which may have existed could have had no firmer basis than neurotic anxiety. The automobile industry, by holding prices below the short-run equilibrium level, created an army of frustrated potential buyers. Yet if the automobile manufacturers had really been seriously concerned about the possibility of entry, which I doubt, they might have been better advised to raise prices to the equilibrium level for the purpose of eliminating the waiting lists; for these waiting lists, one might argue, were a clearer and more unmistakably inviting signal to potential entrants than very high profits would have been.

Mr. Chamberlin is, of course, not to be grouped with the ardent revisionists; it may be noted that the title of his essay uses the word revisited, not revised. I find little to quarrel with, either in Mr. Chamberlin's exposition of his present views or in Mr. Weintraub's summary of Mr. Chamberlin's position. I am a little sorry, however, that in the interest of theoretical purity and analytical elegance, Mr. Chamberlin has decided to part company with the concept of the group. Indeed, in another place, he deals even more harshly with the related but looser concepts of industry and community, castigating them as a snare and a delusion.

The argument here is a familiar one, and few would be disposed, given the usual premises, to question the pure logic of Mr. Chamberlin's position. It has been clear—at least since Mr. Triffin's book—that these group concepts are treacherous, tending to conceal variations in substitutability relations within the group and the existence of substitutability relations across group boundaries. The dangers, moreover, are not merely presumptive. Most of us have run across empirical work in which these group concepts have in fact "snared and deluded" the authors. And one can sympathize with Mr. Chamberlin's exasperation when someone draws inferences with respect to the size of the price-marginal cost gap from a calculation of the concentration ratio for the cast-iron lawn dog industry. In defense of the group concept, however, I am moved to say that anyone who could commit such a crime would, in the absence of the group concept, do something else just as foolish.

It would be a mistake, I think, to blink the fact that industries do exist; they were not just thought up by economists. They are institutionalized in thousands of national trade associations. Firms view themselves as members of industries. They do not in fact think of themselves as cast upon a sea of cross-elasticities which extends in every direction as far as the eye can reach.

It might be objected that these are institutional considerations which have no place in a discussion of pure theory. I would suggest, however, that the viability and growth of the trade association and the apparent strengthening of the firm's identification with the industry tend to lend plausibility to the hypothesis that these arbitrary industry boundaries do in many cases correspond roughly to significant breaks in the chain of substitutes. If this were not so, it would be hard to explain why trade associations, given their usual objectives, have fared so well.

More important, however, is the proposition that the identification of the firm with the industry causes the firm to behave differently than it otherwise would. Trade associations play an important role in the process of price and output determination; and the institutional framework to which he is attached conditions and modifies the businessman's perception of cross-elasticity relations. If the industry concept tended only to narrow the economist's view of the ramifications of competitive relations, there would be no loss in abandoning it; but if it tends also to narrow the businessman's view, as I suspect it may, then it would be a mistake to banish the group concepts from the halls of price theory.

Lest I be accused of a temperamental affinity for the *status quo*, I want to call attention briefly to at least one aspect of monopolistic competition theory which seems to me in need of revision. The argument is not a new one—the essential point having been made in 1918 by J. M. Clark and a few years later by Frank Knight—but it has been overlooked or rejected in the development of the new theories of competition. It relates to the treatment by Chamberlin and others of the selling cost problem. Most economists have followed Chamberlin in drawing a sharp distinction between selling costs and production costs. In Chamberlin's words, ". . . of all the costs incurred in the manufacture and sale of a given product, those which alter the demand curve for it are selling costs, and those which do not are costs of production . . . those made to adapt the product to the demand are costs of production; those made to adapt the demand to the product are costs of selling."

This distinction will not, I think, stand critical analysis. It would seem to be based on the questionable premise that the concept of product embraces only those material qualities which would commend themselves to an industrial purchasing agent or a home economist. Hence the notion that only production costs can alter the product. Yet on the other hand we are told that the test for homogeneity or heterogeneity of product is a psychological, not a material, test—the test of buyer indifference. This doctrine would suggest that products can be altered by producer activity which does not in any way affect the material qualities of the good; i.e., selling costs.

Economists have been reluctant to accept, or at least to incorporate into their economic thinking, what every good advertising agent knows to be

true: that in an opulent society such as ours, consumers are frequently more attracted by the sizzle than by the steak. Consumption decisions are often determined by the talismanic attributes of goods rather than by the objective qualities which fit more neatly into the rationalistic tradition of economics. Opulence surrounds the basic physical wants with a host of social wants which people try to satisfy through the consumption of goods. And the clear evidence of consumer behavior would suggest that the talismanic attributes of goods—the creation of which is one of the main objectives of modern advertising—are regarded by buyers as capable of satisfying these acutely-felt social wants.

If this view is correct, the commonly accepted sharp distinction between production and selling costs would appear to be untenable. What, after all, is the difference in principle between the type of cost entailed in lengthening the hood of a Cadillac, in order that greater prestige shall attach to the ownership of a Cadillac, and the printing of an advertisment showing a Cadillac standing in front of a country club, in order that greater prestige shall attach to the ownership of a Cadillac? And yet it is the custom of economics to regard the one as altering the product and the other as altering demand—the custom, even, of the less cautious among us to categorize the former as progress and the latter as waste.

Inconsistencies such as this suggest that we might well abandon the sharp cleavage between production and selling costs in favor of a view of the matter which is compatible with the subtle and evanescent character of consumer wants in an opulent society.

GARDNER ACKLEY: I must begin by saying that there is little in Professor Clark's paper with which I find myself able to disagree. As is always the case with whatever subject Professor Clark touches, the result is illumination and stimulation.

There were a few minor points out of the number he makes with which I might have picked an argument; but even these turned out, when I attempted to develop them, to become largely issues of semantics. I have decided, therefore, to use my few minutes for what might be considered a methodological matter. It relates to Professor Clark's fundamental thesis that our theories of competition, to be realistic and to be useful in policy, must be made dynamic.

I expect that there can be no disagreement—indeed it is a commonplace—that our economic theories need to be dynamic. The trouble is that the word dynamic does not always have a very clear or specific meaning. As someone has remarked, dynamic is merely an adjective which distinguishes one's own theories from everyone else's.

I wonder, therefore, if it would not be useful to specify rather clearly what we may mean by a dynamic theory of competition before we proceed to construct or to criticise one. I may say in advance that it is my feeling that Professor Clark has not given a very clear or consistent meaning to dynamic theory and that this may be responsible for my impression that his paper represents mainly a series of somewhat disconnected observations. I find many of these observations individually very penetrating and stimulating; but

nevertheless the thread which ties them together is sometimes hard to find.

Just what is the distinction that Professor Clark means to imply in his contrast of "static models and dynamic aspects"? In the very first sentence of his paper reference is made to "the gap between theories of cost-price equilibrium and theories of production, growth, and development." Only a few sentences later we have it that dynamic theories "include the tendencies toward equilibrium. But because these tendencies never reach their static limits, dynamic theory cannot use any features which are needed only to enable a model to attain this impossibly precise completeness. . . ." Still later we read that "a theory of competition as dynamic process must be not a model but a framework within which many models may find their places, including equilibrium models as limiting hypothetical cases." This is about all that I find which refers explicitly to this contrast of static versus dynamic theories. There is, however, also an implied definition of dynamic in the substantive matters and problems which Professor Clark chooses to discuss.

Leaving aside for the moment Clark's particular meaning, what meanings might we give in general to this contrast of static with dynamic?

I assume that we agree quite generally that static refers to an analysis of states or conditions of equilibrium. Given the values of certain outside or exogenous factors, there is (or may be) one pattern of the internal or endogenous variables from which all tendencies to further change would be absent. This pattern represents an equilibrium situation.

Now, because equilibrium is a situation from which change is, by definition, absent, equilibrium theory has frequently fallen into the trap of eliminating from consideration those things which are the inevitable concomitant of change—most of all, uncertainty. If equilibrium is a prediction—a state which will someday be reached—then this is legitimate. But if, as I assume it usually is, it is only an analytical device, then there is no excuse for eliminating from the description of behavior the effects of change and uncertainty. To put these back into static theory, then, is a first step toward making theory dynamic.

Some of Clark's dynamics consist precisely of this. It seems to me that much of what he has to say about the content of long-run and short-run cost curves and about Chamberlin's tangency theorem is merely an improvement of the static analysis by the recognition of the effects of change and of the uncertainty which change generates.

To introduce the effect of change and uncertainty into our demand and supply curves is an important step. But it is still not part of what I would consider a true dynamics: an analysis in which the concern is with processes of change. Nor does Professor Clark stop with this first step.

As we have come increasingly to realize, in order even to define the existence of a stable equilibrium, we need at least some rudimentary dynamics. These have always been implied in all of our equilibrium models, but they have often been inadequately explored. The question is: How do sellers (buyers, factor suppliers, et cetera) behave when equilibrium does not exist? What precisely is the nature and the time sequence of their behavior? How do they respond, and when? Only by investigating these questions can we be sure that an

equilibrium, or at least a stable equilibrium, exists. And the properties of the equilibrium depend on the nature of the disequilibrium behavior.

A great deal of the substance of what is now called "dynamic analysis" is merely an explicit exploration of the way in which equilibrium is approached. A comparative statics analysis concentrates on previous and the new equilibrium positions and ignores the process of change from one to the other. A dynamic analysis shows the time sequence and pattern of movement between the two equilibrium positions.

This type of dynamics may also recognize that full equilibrium is rarely or never achieved. Changes in the exogenous variables may be so rapid and so extensive that the system never reaches one equilibrium before that equilibrium is replaced by another. As a well-known text once put it, the dog continues to chase the rabbit (equilibrium), but the position of the rabbit continually changes. To understand the behavior of the dog we need to know where the rabbit (equilibrum) is at any time, even though we know that the dog will never catch him.

What would be the content of the analysis of competition which corresponds to this rather narrow concept of dynamics? It would deal, I think, with several of the questions that Professor Clark discusses. When he talks of the process of profit erosion, and its duration, he is, it seems to me, discussing the dynamics of the approach to equilibrium. Technological change or other disturbances to equilibrium occur. What is the sequence of events by which the market adjusts—through innovation, imitation, and competitive response —to this initial change?

This, too, is important and useful. And it requires explicit recognition, as Clark urges is necessary, of the time lags in the process of market adjustment.

But there is another and broader content to economic dynamics—the content which Clark suggests when he contrasts theories of equilibrium with theories of growth and development and which he deals with several times in the substance of his paper. The narrower dynamics retains an essential tie to statics; it merely investigates more explicitly the processes involved in "tendencies toward equilibrium." But the broader type breaks more fundamentally with the equilibrium concept. In the broader dynamic analysis, movements of variables represent not merely the working out of tendencies toward equilibrium. Rather, processes of change are seen at least in part to be irreversible, self-generative, and self-determining. Change is a product of previous change, as well as of other current variables. Partly, this is a matter of bringing into the analysis, as endogenous variables, factors which the equilibrium analysis took as exogenous. Thus we would attempt to explain, at least in part, such things as the state of technology, the number of sellers, the evolution of buyers' tastes, the nature of the market institutions (such as the form of price quotation and methods of sale), the attitudes of sellers toward their rivals' moves, their long-run versus short-run horizons, et cetera. These would be things to be explained rather than to be assumed.

But the broader dynamics to which I refer does more than merely to enlarge the list of variables. It also, as I have indicated, considers the factors responsible, not only for the levels, but for the rates of change of these

variables. When Professor Clark sets up his criteria for appraisal of market performance, he is clearly thinking very specifically in these terms. He is concerned with whether a particular market situation is productive of a more or less rapid rate of introduction of new processes and new products, with the nature and speed of product evolution, and with the speed of the creation as well as the erosion of profit opportunities. And he has some very interesting things to say about these.

All of these contributions toward a more dynamic theory are important and useful. But if we are trying to provide a general framework for a dynamic theory of competition, we probably need to recognize more explicity than I think Professor Clark has done that dynamic can have several meanings. And I believe that we must develop somewhat separately the appropriate theoretical tools as well as the substantive content of these several levels of analysis. As is the case with other branches of economic theory, the dynamic analysis of competition and markets is still in the stage of foundation laying.

Professor Clark has given us some sketches of a few elevations of the structure. But it is still a little early to tell what the building will look like when it is completed.

THE IMPACT OF ANTITRUST LAWS

VERTICAL DISINTEGRATION IN THE MOTION PICTURE INDUSTRY

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In July, 1938, the Department of Justice filed its antitrust action, *U.S.* v. *Paramount Pictures,* in the Federal District Court of New York, and in May, 1948, the Supreme Court, speaking through Justice Douglas, affirmed the government's hard-won victory. The five so-called "majors"—Loew's, Paramount, Twentieth Century-Fox, RKO Pictures, and Warner Brothers—were ordered to dispose of their theatrical holdings, and they and the three non-theater owning defendants—Columbia Pictures, Universal Pictures, and United Artists—were enjoined from certain monopolistic or discriminatory practices. One of these was renting films to theatrical circuits on a circuit-wide basis, to the disadvantage of competing independent theaters. Another was "block booking," or conditioning the offer of one film on the purchase of another, often (prior to 1940) by licensing the coming year's product in one transaction. The eight companies must now, therefore, rent their films "theater by theater," and, except where a noncompeting theater negotiates for several at once for convenience, "picture by picture." This is important, because the eight not only produce most of the domestic features but distribute—that is, negotiate the rental to exhibitors (theater owners)—some imports and many domestic features produced by independents.

The government convinced all eleven sitting judges of the District and Supreme Courts that the defendents had conspired to restrain trade and monopolize. Although the five majors had full or part ownership of only one-sixth of the 18,000 theaters, so many were first-run houses in big cities that they took in a much larger percentage of all box office receipts. Not only did all eight defendants' features usually appear first in these affiliated theaters but the majors had sometimes used their control over the film supply to force independent exhibitors to sell out to them or accept partnership arrangements. The remaining independents were in general relegated to "subsequent runs," behind a "clearance" of some stipulated number of days after the close of the next prior run.

These were some of the conditions that the government and the courts hoped to cure by divorcing theaters from the producing and distributing branches of the industry and by prohibiting such practices as block booking. Between 1948 and 1954 the integrated companies

were successively split by five consent decrees into two parts each, creating five disaffiliated theater circuits—which, in turn, were allowed a period of time to sell a quarter to a half of their theaters. Although many cynics suspect that these corporate pairs continue under common control, the evidence is that they deal with each other at arm's length.

The Paramount decrees represent the most important experiment in vertical disintegration under the Sherman Act—one of greater potential significance than the separation of anthracite mines and railroads ordered in 1920 or of Pullman car operation and manufacture ordered in 1944. I shall treat separately the effects on (1) exhibitors, (2) independent producers, (3) the defendants themselves, and (4) the public.

The aim of the whole case was to win for exhibitors protection against being forced to sell out if their location and policies proved profitable, against being forced automatically into subsequent runs behind long clearances, and against being forced to accept bad films as the price of getting good ones. The first two aims were also those of the "circuit" decisions, *U.S.* v. *Griffith* and *Schine Chain Theaters* v. *U.S.*, which Justice Douglas announced on the same day in 1948 and in which the Supreme Court found that the large, although nonaffiliated, circuits had been using their buying power to gain unfair advantages over smaller theaters.

These aims have been realized. If, for example, an exhibitor thinks he can prove that the location, size, or other characteristics of his theater deserve an earlier run, there are lawyers who will help him, on a contingent basis, to sue the eight companies under the treble damage provision of the Sherman Act. What Justice Douglas called their "marked proclivity for unlawful conduct" will be prima facie evidence on his behalf, and, under the precedent of the Jackson Park case (*Bigelow* v. *RKO*), decided by the Supreme Court in 1946, his damages may be estimated liberally. The producer-distributors, under this threat and the courts' 1948 injunction against discrimination, and because they have no reason now to favor their formerly affiliated circuits, are apparently granting "availabilities" to theaters equitably. The motion picture decrees, in the light of their aims, can be classed as highly successful.

The first qualification appears when we ask how many exhibitors have been helped in practice. Since an injunction to discontinue their theater expansion programs had been issued against the majors in 1940 as part of an earlier consent decree (which the government decided in 1943 to abandon), divorce may not have been necessary to save many independent exhibitors from being bought out. There are no figures

as to the number of theaters which have advanced their runs, but a questionnaire of my own, sent to 221 exhibitors and answered by 41, disclosed that only three of these had stepped up. Of my 41, 12 noted a shortening of the clearance after the first run in their towns, the average change being from fifty-six to thirty-three days. In the remaining towns, this clearance had averaged only twenty-three days and is unchanged. Apparently, and fortunately, it is the unreasonably long clearances which have been reduced.

The mere fact that only a minority of theaters had received a practical gain would not explain why this successful decree is so unpopular. Criticism of the outcome is a regular refrain in the opinion columns of the several trade magazines read by exhibitors. The Department of Justice has received more complaints against the decree than against any other in history. The Senate Small Business Committee held extensive and inconclusive hearings on it in 1953. The Theater Owners of America, the largest trade association of exhibitors, demands that the Department of Justice drop its veto on vertical integration. This change in attitude calls for explanation.

First, the decree is blamed for the "product shortage," or reduced flow of feature pictures. The big decline has come in 1954, in which the number of domestic features released by the "national distributors," or members of Eric Johnston's Motion Picture Association of America, has declined by more than 20 per cent. The case of RKO, which never got into full swing again after it had shut down its studios for a period in order to "root out Communists," is extreme; but the major studios generally have reduced their output, and smaller studios have turned predominantly to manufacturing grade B's for television. The theaters have had to fill their screen time with independent and foreign films and reissues and by giving some features a longer playing time.

The majors explain their part in the product shortage very simply: they have found that, when pictures are only moderately better than those on television, people stay home. Their deliberate policy, therefore, is one of better pictures, even if the higher cost per picture reduces the number made. But divorcement has played its role. Formerly a big company would release a feature a week, one aim being to keep the screen time of its affiliated theaters occupied. Now this motive is gone. Moreover, with block booking abolished, the producer can no longer count on renting a year's output, good and bad alike, ahead of time; he must make sure that each picture is salable on its own merits; and this suggests more attention to each one made and less scattering of efforts.

The exhibitors view the reduction in output as a conspiracy to raise

prices by reducing supply. The Allied States Association—second largest exhibitor organization—has considered the possibility of investing in the stocks of motion picture producers and then voting for directors who would increase production schedules. The Theater Owners of America endorses the proposal of National Theaters, the former Fox circuit, to go into picture making (which the Department of Justice has vetoed) and plans to raise 10 million dollars to finance independent producers. The very reason which stimulated William Fox to originate moving picture integration in 1910 in his battle against the Motion Picture Patents Company, the trust of that day, and which inspired the First National Circuit in 1917 to build studios in its fight against Paramount-Famous Players-Lasky Corporation, the trust of *that* day, is at work again: to assure a supply of the raw material. This is the same motive that causes oil refiners to go into crude production and steel mills to search for iron ore.

A really bitter complaint of exhibitors is against the rise in rental terms, which both distributors and exhibitors agree has occurred. A poll of theaters conducted for COMPO (the Council of Motion Picture Organizations, which represents producers, distributors, and exhibitors) disclosed that in 1946 they paid an average of 31 per cent of their box office receipts as film rental and in 1953, 36 per cent. This would account for 5 points of the estimated decline from 14 to 2 in average theater net profit as a percentage of box office receipts. Evidence also shows that the smallest theaters have been the hardest squeezed.

This upward shift in rentals, due in large part to the trend toward fewer, more expensive films, is linked with divorcement in another way, also. The big producers, no longer owning theaters and making their money on film rentals rather than at the box office, have become sharper bargainers. This is well illustrated by the 70 per cent (twice the average figure) which is now being demanded for some of the big features like *The Robe* and *The Caine Mutiny*. Moreover, competitive bidding has replaced bilateral negotiation as the means of settling terms in many hundred "situations." When the District Court originally ordered bidding, the Supreme Court eliminated the order; but it has developed anyhow. The Paramount decree having opened the possibility of an earlier run for independents, the question arose who was to get a feature which two or more theaters wanted on the same "day and date." Two solutions emerged: when the competing exhibitors were friendly, they would adopt a "product split"; that is, would alternate the sequence in which they showed successive features (thus violating the antitrust law, although with no risk of prosecution); otherwise the distributor could avoid accusations of favoritism only by asking for sealed bids.

One who makes a sealed bid is tempted to offer as much as he can afford, whereas in a straight negotiation he can bargain, hold his cards close to his chest, and threaten to walk out. The resulting rental rise in bidding situations is more important than the question whether some distributors handle bids unfairly, as unsuccessful bidders may claim. Unsuccessful bidders often include independent theaters which have demanded early runs but cannot outbid their larger rivals.

The Allied States Association, after vainly demanding that rentals be made subject to arbitration—a demand which won sympathy from neither distributors, the Department of Justice, nor the Senate Small Business Committee—has turned to a proposal that the industry be declared a public utility and its wholesale (not retail) prices regulated by the Federal Trade Commission. This threat, it hopes, can be traded for lower rentals.

A third reason why exhibitors are not happy about the decree is the abolition of block booking. The time and cost saving in buying all at once was considerable, the assurance well ahead of time that they could plan on their features for the big holidays was pleasant, and their hope of finding a "sleeper," or good film whose potential would emerge only upon its showing, matched their expectation of being given some "dogs" which they might not even want to run. On my questionnaire, twenty-nine exhibitors expressed the wish to have block booking restored as against eleven who opposed it and one who did not care. Some noncompeting theaters buy several pictures at a time today, but distributors are likely to frown on this because of legal doubts or the belief that renting one at a time makes it easier to get the maximum price for each.

Before leaving the exhibitors, I must at least mention the new and powerful forms of competition which hit them just as they were hoping to profit from the Paramount decree. With television we may lump all the new influences that keep people out of theaters, from night baseball games and tending suburban gardens to the cost of baby sitters and of parking space. A COMPO survey showing a continuous decline in theater net income from the 201 million dollar peak in 1946 to 24 millions in 1953 is consistent with the estimated 45 per cent decline in attendance. The same source reveals that in 1953, 29 per cent of theaters were losing money and another 38 per cent making it only because of concessions, of which popcorn earns the most profit.

The second new type of competition has been the drive-in theater. Between 1946 and 1954, COMPO gives their number as increasing by 3,750, while that of "four-wall" theaters declined by 4,400. The greater impact of television and drive-ins has taken the attention of some four-wall exhibitors off the antitrust situation but made others

all the more anxious to pull some rabbit out of that hat. On my questionnaire, seven called the Paramount decree good, eleven called it bad, three (correctly) said it was both, and twenty had not felt it at all.

To the independent producers, I can give only a few words. The government hoped that, once the majors owned no theaters, independents could dispose of their pictures in an open market. It is true that far more independents are active now than a few years ago, but apparently not because of greater ability to get their pictures into theaters. There were always more than 15,000 theaters that had no ties to majors; even the producer-owned theaters did not dare pass up box office hits like *The Best Years of Our Lives* or *Snow White and the Seven Dwarfs;* and most good independent films have always been released through the distributing offices of the eight defendants or even, as in the case of *Gone With the Wind,* financed in part by them. More important than the decree in accounting for the rise of independents is the increasing preference by the majors for profit participation plans as against the fixed charges of salaries of directors and stars, along with their declining production schedules which give them studio space to rent out.

The defendants, as has often been the case after antitrust defeats, seem not to have suffered very seriously. Their earnings are sharply lower than in their big years, 1946 and 1947, but television and other substitutes for the movie habit offer an adequate explanation. With the rush of successful pictures since 1953, perhaps aided by program boredom on the part of those who have owned TV sets several years, their profits have at last turned up strongly. Getting rid of theaters just when television was striking was by no means pure loss.

As to the newly divorced circuits, they have done better than one might have expected. The theaters sold under the decrees have often been their worst earners, and their remaining large theaters have had more financial backing than most independents to install technical improvements like Cinemascope and stereophonic sound and thus to share in the recent box office gains from the popular pictures using these techniques. The Stanley Warner circuit owns Cinerama, and United Paramount Theaters has merged with American Broadcasting Company. Had Paramount Pictures and Theaters not been divorced, the Federal Communications Commission would hardly have allowed this new form of integration.

One respect in which the big producers have been hit is as treble damage defendants. Although many such suits are settled by giving the plaintiffs their legal fees and the right to a prior run whose denial had been the ground of their suit, the defendants have lost several and paid substantial claims. Pending suits demand damages of more than half

a billion dollars, exceeding the net worth of all defendants combined. Eventually the statute of limitations, the crystallizing of new distribution patterns, greater caution by the courts in the face of such a Pandora's box, and perhaps an industry arbitration system should dry up the flow of new suits.

Last comes the public. How has it fared as regards quality, price, and convenience? The government's hope was that, with bad pictures no longer floated by block booking, with majors forced to rent their features in an open market, and with all theaters available to show independent films, quality would improve. I asked an officer of an exhibitor trade association about this, and he replied: "The exhibitor is constitutionally unable to judge quality except by box office; why don't you ask the film critics?" So I asked, and heard from thirty-two who said that quality has improved, one who denied it, and the *New York Times* critic (incidentally, the most influential in the country) who told me courteously but plainly that quality is too complex a topic to discuss in that fashion. Three critics felt that the improvement which they observed could be credited to divorcement, twelve said no, and seventeen saw no clear connection. If, however, the decree was one factor which caused the big producers to cut down the number of their pictures and increase the time and money spent on each, it would seem that quality should have improved, and the decree thus get part of the credit.

Several of these factors might have been expected to put box office prices up: (1) higher distribution costs with the abolition of block booking; (2) harder bargaining by producers, who sometimes admit, or insist, that their terms on a particular picture can be met only if theaters raise their prices that week, but who avoid contractual resale price fixing such as the Supreme Court condemned in the Paramount decision; and (3) the tendency of some theaters, because they have stepped up their runs, to raise prices. If this third tendency went to its logical limit, neighborhood theater prices would rise, and downtown prices be forced down by slumping attendance as people could see early runs at home—thus presumably reducing the variety of price-and-product packages available to consumers. Although one may guess that these effects will be small, price statistics are not clear. The Bureau of Labor Statistics index of admission prices, hidden in the "Reading and recreation" component of its consumer price index, has increased 20 per cent since 1948, but industry estimates of the increase in the net price after deducting admissions tax range from the *Motion Picture Herald's* 11 per cent for 1954 to the *Film Daily Year Book's* 53 per cent for 1953. The fact that attendance has switched from smaller to larger theaters and also from daytime and weekdays to eve-

nings and holidays, plus the fact that an undetermined number of theaters, but probably most, made little or no cut in price when the excise tax was lowered last April, muddy the waters further.

Convenience has been improved for some members of the public, worsened for others. The reduction of unreasonable clearances has been good for all; but the 16 per cent of adult moviegoers who attend more than once a week may have felt the "product shortage." The old problem of too many theaters showing the same feature the same week has been intensified by the stepping up of runs, although other causes have operated—like co-ordination of showings with advertising campaigns and economies of buying a picture for all theaters in a circuit at once (technically, to be sure, on a "theater by theater" basis). How a picture may come and go, before the whole potential audience realizes it, is illustrated by the 200,000 people in the Chicago area who (according to a survey by a theater circuit) were still waiting for *Quo Vadis* to come to their suburban theaters long after it had been shown. Members of the public who are not bound by old habits, and are more anxious to see the newest thing, are better pleased. As I wrote this, five of the eight theaters in my immediate neighborhood were showing *Seven Brides for Seven Brothers,* and my daughter had managed happily to see it four times.

The morals I draw will be brief. (1) Vertical disintegration can, as would be expected, effectively remove the danger that integrated firms will abuse their power. (2) It may, however, boomerang on the supposed beneficiaries. Although the more ambitious exhibitors have won their chance to fight on equal terms with the big circuits, others would prefer the old days with their well-established grooves. (3) Disintegration may have mixed effects on the public—as, in this case, very likely better quality but possibly higher prices and perhaps more patrons convenienced than inconvenienced. (4) Antitrust decrees are likely to have less influence on an industry than either competition of other commodities and services or shifts in population and consumer habits, of which television and the drive-in, respectively, are the prime examples here.

On net balance, this experiment in disintegration is not without its hopeful aspects, but its results to date are better characterized as disappointing than as clear or important.

THE DECLINE OF MONOPOLY IN THE METAL CONTAINER INDUSTRY

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I

The Clayton Antitrust Act is now over forty years old, and economists are still uncertain about its role in antitrust policy. Most would doubtless agree on the need for some instrument that is more flexible than the Sherman Act. That Act cannot be applied until monopoly control is well established, and one purpose of the Clayton Act has always been to prevent monopoly by arresting it in an incipient stage. Its success in this respect has been mixed; many persons have pointed out of late that a policy of prohibiting competitive practices that could eventually result in monopoly may be leading us toward per se rules of business conduct that are impossibly confining. But the Clayton Act has also been used against business practices in markets that already have a monopolistic structure. Sometimes this has been done merely to provide a bill of particulars in cases tried chiefly under the Sherman Act; but sometimes it has been done in an attempt to weaken monopoly through an attack on its manifestations. Doubts have also centered on the wisdom of this latter policy, on the ground that it does very little good to prohibit a market practice if the underlying monopoly power which makes it oppressive remains untouched.

The American Can case of 1950 illustrates in almost unexpected circumstances the force that the indirect and peripheral approach of the Clayton Act may have to make competition more effective. This was a case in which a specific market practice was the key to market power. The practice had nothing to do with the creation of market power, since it supervened upon a monopolistic structure that was already there; but it played a decisive part in the maintenance of market control after the foundations of monopoly had been undermined.

A cursory examination of the structure of the metal container industry just before the antitrust suit began in 1948 might well have made anyone skeptical of the success of a policy which did not involve fundamental structural reorganization. The industry was highly concentrated, and still is: the two leading firms, American Can Company and Continental Can Company, together accounted for nearly 80 per cent of total sales, and American alone had over 45 per cent.[1] There were in

[1] *United States of America* v. *American Can Company*, Civil Action No. 26345-H. In the United States District Court for the Northern District of California, Southern Division. Exhibits UU and 3124 (Stipulation on Statistics, March 31, 1949).

addition a number of small sellers—no one of them more than one-eighth as large as American—which occupied sheltered and specialized positions or else appeared to subsist on a margin of tolerance. The major companies manufacture a product-mix of containers for different purposes, but differentiation within each product line was and is insignificant. Cross-elasticities of demand between metal cans and substitute containers are very small. The prices of the two leaders, as well as their other market policies, have long been substantially identical. Prices for given products in given sales regions were set by one of the large firms, the other following; a policy of universal freight equalization eliminated the uncertainties of geographical differentiation; and prices exhibited the characteristic rigidity that we have come to associate with tacit oligopolistic collusion.

Tacit collusion in circumstances like these might be expected to result in the maximization of joint profits. However, the maximization of joint profits is always subject to limitations in any oligopoly structure, and in some instances the limitations are so severe that the principle loses its explanatory value. Economists have long preferred an eclectic approach to this problem. No universal solvent has yet been found that will exempt us from a detailed examination of the structure of particular oligopolistic markets and their history, as a prerequisite to explaining their behavior. The limitations upon monopoly power are often hidden from immediate view. A detailed analysis of the metal container industry reveals a number of them.

II

There is time for only a brief summary here. The first thing to be noted is that the limited-duopoly structure in the can industry is a relatively recent development. Twenty years ago any industrial economist would have classed the industry with those dominated by a single firm. American Can was organized originally as a trust controlling virtually the entire capacity of the industry. It suffered the fate of many of the trusts organized at the turn of the century: the umbrella it held over the industry encouraged the growth of competition, and within a dozen years its share of the market had fallen to 50 per cent. It was about this time, in 1916, that American was first charged with monopoly under the Sherman Act. Professor Hession in his able study of the food container industry (*Competition in the Metal Food Container Industry, 1916-1946* [Brooklyn, 1948]) calls American Can "the trust that wasn't busted." The court refrained from dissolving it principally because of the rapid decline in its market share, and the judge expressed the hope that the growth of rivals would soon restore competition in the industry.

Competition was not quickly restored. American possessed nothing approaching pure monopoly after 1916, but it remained the dominant firm. Its market share ceased to fall, and its capacity remained for a time enormously greater than its rivals'. American's prices determined the prices for the whole industry. It maintained its lead by rapid development and expansion from within. Nevertheless, its power to control the industry was considerably weaker in 1948 than it was in 1916.

This slow metamorphosis was due partly to developments within the industry, partly to the impact of the antitrust laws. The structure of the industry altered gradually as Continental Can began to challenge American's leadership. In 1916, Continental was a small cloud on American's horizon, hardly distinguishable from a number of other small rivals. Throughout the following decades, Continental grew rapidly with a bold program of acquisition and merger as well as by rapid expansion from within. By 1939, it was half as large as American; by 1950 it was three-fourths as large. Its growth eventually put it in a position that rivalled American's. Prior to the middle thirties, American was the only metal container manufacturer with an important research program and was practically solely responsible for the progress and development that had occurred up to that time. Moreover, it was the only manufacturer up to then that was fully able to realize certain other advantages of scale. These are not production economies, but are achieved through the interlocking of separate plants so that the extreme uncertainties of demand for food containers in any one area are diffused along a chain of production sites. The ability of American to offer certainty of supply, together with its facilities for research and customer service, made it the only acceptable source of supply to many buyers of cans, including most of the large and stable ones, until a second large seller appeared.

By the end of the thirties, American was at last confronted with a powerful, efficient, and progressive rival in practically every geographical region and every product, including many in which American had earlier enjoyed a monopoly. It took time for Continental to break into the geographical strongholds of American and to build up an effective and experienced research staff, but in the end the supremacy of American was undermined.

Conditions were more stringent for the smaller firms. No other was able to achieve anything like the growth of Continental; in fact, several of those which had begun to grow to substantial size were acquired by Continental. A few producers of food cans managed to build up integrated regional organizations, securing at least part of the advantages of size, and compensated for their inferior research and service facilities

by charging somewhat lower prices. Two firms even succeeded in entering the industry and establishing themselves in the food can market. All of these smaller producers were potential bases for expanded competition; that this potential was not fully realized was due to certain market practices of the larger firms which held the smaller ones in check.

A significant change was forced by the tightening of the law against price discrimination after 1936. The Robinson-Patman Act affected two markets: tin plate and tin cans. Tin plate accounts for 60 per cent of metal container costs. In the days of the Steel Trust and the Can Trust the tin plate market was almost a classic bilateral monopoly. The monopolies on both sides lost their near-exclusive control, but the "leading bargain" between U. S. Steel and American Can continued to determine the price of tin plate. In this market it appeared that monopoly power was offset by monopoly power. The benefits of this countervailing power, however, were mixed. Improvement and development in the tin plate industry, for instance, have been rapid and considerable, and there is no doubt that the large buyers, especially American, have pressed the steel producers closer to the maximum attainable rate of progress—both in product improvement and cost reduction—than would have been the case if buyers had been small and impotent. On the other hand, the price benefits of countervailing power filtered down to consumers very slowly; it requires a substantial degree of competition in subsequent markets before countervailing power in a prior market amounts to anything more than a division of spoils. One result of American's dominant position as a buyer of tin plate in the early years was that the tin plate producers favored it with secret rebates and systematic price discrimination. After a second buyer of substantial size had entered the market a new dimension of strategy emerged, and price reductions negotiated between individual buyers and sellers became more difficult to confine to the largest buyers. Nevertheless, systematic discrimination continued in various forms up to the passage of the Robinson-Patman Act, at which time American actually capitalized its claims to discriminatory treatment and liquidated them by accepting substantial lump-sum settlements from United States Steel and other suppliers. After 1937, countervailing power was exercised in behalf of all buyers of tin plate. This benefited the ultimate buyers of cans only insofar as there were limits to the exercise of monopoly power in the can market.

The buyers of cans were themselves not entirely helpless, though none had anything like the bargaining power that American had in the tin plate market. Even in the days when there was only one large

can manufacturer, the big canning companies had the option of manufacturing for themselves. Backward integration is not extremely difficult for a large user, and it would have taken only a slight widening of the profit margin on cans to make it economical. Potential entry from the buying side limited monopoly power on the selling side. At first the result was systematic, secret discrimination in favor of the buyers who could offer this threat. After a second seller grew to large size, the bargaining position of the large buyers was strengthened, since they could play one off against the other. The Robinson-Patman Act obliged the major can companies to abandon secret discrimination. It was replaced, not by uniform prices to all buyers, but by an open schedule of volume discounts, which eventually came to be based on total purchases of all kinds of containers from the supplier. The discounts were greater for large-volume purchasers than any concession that the smaller can companies could easily offer, and there is no doubt that these discounts worked to their disadvantage. On the other hand, the continuous alteration and reshaping of the discount structure did afford a means of limited price competition between the two leaders; and the large buyers had at least the potential power to enforce this competition between them.

III

Though the monopoly power once possessed by the dominant firm had been markedly eroded by 1950, the effect of the underlying structural changes on competition remained more potential than actual. The reason was that the leading firm followed several policies which concentrated its remaining market power in the most effective way, and its large rival did likewise. There were three important instruments of commercial dominance. The first was the volume discounts just mentioned. The second was the practice of selling under long-term requirements contracts. It is fair to say that neither of these could have had a powerful enough effect to justify action under the antitrust laws, though they did tend to reinforce each other. The contracts were written for specific containers for use at specific plants. Smaller can companies occasionally succeeded in becoming secondary suppliers of large buyers, but it was the usual practice for a buyer to concentrate all his purchases on a single source of supply. There were some buyers whose purchases considerably exceeded the volume necessary to get the highest discount that was offered, but most of these also had long-term requirements contracts with a single supplier or, less frequently, split their purchases between American and Continental. The amount of business that the smaller can companies were eligible to compete for at any given time

was thus restricted. Volume discounts were partly responsible, but the superiority of the two large sellers in research and customer service may have been equally important in determining the result.

The third instrument of control was the practice of tying the lease of can closing machinery to the sale of cans. (This was not done through an explicit tying clause, but by arranging the expiration dates of separate contracts so that no canner would be able to retain American's machinery to close competitors' cans. There were minor exceptions.) This requires a word of explanation. If the manufacture of closing machinery had been perfectly competitive, the larger can companies would have gained no net advantage by this practice. But American's can machinery has long been recognized as the best and most complete in the industry, and its progressiveness in this field is beyond dispute. Continental began late, but was overtaking American rapidly in the late thirties. Both large firms followed the same policy. Rentals on closing machinery, which also covered servicing by the manufacturer, were set below actual cost, to induce buyers to lease machines. Most can buyers were allowed to retain the machinery only as long as they purchased the cans to be closed on it from the same firm. The moderate advantage which the major sellers possessed in closing machinery was thus extended forward and magnified in the can market. The relation between supplier and customer became harder to break. (Closing machinery is generally unimportant for nonfood cans, and both major companies faced much more vigorous competition from independents in this field.) In markets for food cans, the smaller manufacturers were forced to imitate the leasing practices of the majors; but since they had to buy somewhat inferior closing machines on the open market and lease them at below-cost rentals, they reaped no comparable advantage. (One or two of the smaller companies also entered the manufacture of closing machinery, without much success until recently.) A side effect of the practice was that no canner would buy machinery in preference to renting it, and independent machinery manufacturers faced an uneconomically thin market.

IV

The government might have attempted a major Sherman Act case, with wholesale dissolution and structural reorganization of the industry as the objective. But this would have been risky, since the specific charge of monopolization had been tried in an earlier case and the judge had refused the suggested remedy of dissolution. Instead, the government attacked the practice of tie-in sales directly under Section 3 of the Clayton Act, retaining a Sherman Act charge to scoop in the requirements contracts if possible and to provide for equitable relief.

There is some evidence that the Antitrust Division was not entirely satisfied with the form that its case eventually took in consequence, but it won the case nevertheless. The court decided that American had violated both the Clayton Act and the Sherman Act.[2] In addition to a number of minor matters, which need not concern us here, the 1950 decree altered market practice in the metal container industry in three ways. (Continental accepted the same judgment in a consent decree.)

1. The major firms were prohibited from offering any annual cumulative volume discounts.

2. Requirements contracts were limited to one year. American Can had presented convincing evidence that requirements contracts are necessary to protect food canners, in view of the great uncertainty of crop yields and timing, and that canners preferred them to fixed-quantity contracts; hence the court did not prohibit them altogether. The court also ordered that separate contracts be written for individual plants when the customer had more than one cannery. The buyer could still place all his contracts with a single seller if he chose, but smaller can manufacturers would find it easier to bid at least for the business of a single plant of a multiplant buyer.

3. The tie between machine leasing and the sale of cans was broken. The major companies were permanently enjoined from conditioning the lease of machines on the sale of cans, by any subterfuge. In addition, they were ordered to sell their existing closing machines at bargain prices to anyone who wanted to buy them, giving priority to existing lessees. This order applied to all machines to be built in the future as well, for a period of ten years. The can companies were required to make all technological information and know-how available to buyers, to set up schools to train service men employed by the canneries, and to license closing-machinery patents without royalty. The court recognized that there were certain advantages to integration between machinery manufacture and can manufacture, since can manufacture and can closing involve much the same technology, and so it did not order divestiture. The can companies were required to lease to everyone any machine that they were then leasing to anyone. Moreover, rentals had to be fully compensatory, including a fair profit, after the end of 1953. (Continental has asked for a postponement. Fully compensatory rentals on its diminished stock of equipment would be prohibitive, it claims, since overhead has not been proportionally reduced.) No more could below-cost rentals be used as an inducement to lease.

[2] 87 F. Supp. 18 (1949). A simultaneous case against Continental was not brought to trial, Continental having agreed in advance to accept any judgment entered against American, short of divestiture.

V

This decree knocked out practically all the remaining props of market control. The decree has been in effect now for over three years. Some of its consequences have been striking. One instance is the sale of closing machines, which has greatly exceeded everyone's expectations. The can manufacturers had claimed that customers would be reluctant to buy them—that customers would prefer to lease instead and pay the supplier the appropriate fee for bearing the risk of obsolescence and the task of servicing and maintenance. If this reluctance ever existed it was quickly overcome. The bargain prices the court set on the machines existing in 1950 made it uneconomical not to buy them, and in addition American and Continental, in wholehearted compliance with the letter and the spirit of the decree, have made vigorous efforts to sell both old and newly manufactured machines. By the middle of 1954 both American and Continental had sold over 75 per cent of the closing machines they were leasing in 1950. While the lease market may revive in the future, can buyers are no longer heavily dependent on their can suppliers for closing machinery, and there is no way now for the machinery supplier to apply commercial leverage to the can market. After the transition period is over, the independent manufacturers of machinery will be able to market closing machines directly to the canning industry, and they have already responded to this broadening of their opportunities.

A second result has been an extensive breakdown of exclusive supplier-customer relations. While fruit and vegetable canneries have largely continued their requirements contracts, the large packers operating several canneries have begun to allocate them to different suppliers. Other canners have begun to split requirements within the plant, closing cans from several suppliers indiscriminately on the machines of several manufacturers. There has been an enormous growth in open-order purchasing at the expense of contract purchasing. Small firms are now better able to detach fragments of business which used to be held firmly by the large can suppliers. They can enter large-volume markets either by carrying off supply contracts for individual canneries in a chain or by becoming secondary sources of supply within the plant. The vertical partitions in the market which formerly made the ties between particular buyers and sellers very strong have been demolished, and competitive forces, wherever they originate, can sweep across it largely unimpeded. The market position of smaller manufacturers has been greatly strengthened.

It is interesting to note that these changes in the can market have been accompanied by some instability in the tin plate market. At least part of this instability has been transmitted backward from the

can market. Prices have become more flexible; open-order purchasing has grown in volume; the leadership of the leading bargain between U. S. Steel and American Can has weakened further. The tin plate suppliers still feel the weight of the large buyers' bargaining power, but this is more likely now to work to the benefit of ultimate consumers of cans.

It is possible of course to expect too much of the decree. The gross structure of the industry has not changed very much and probably will not change much in the immediate future. Doubtless the effects of a decree of dissolution, if American had been convicted of illegal acquisition and maintenance of monopoly, would have been more spectacular. Several firms could have been fashioned out of either of the leaders without an appreciable loss of efficiency, and the market would in time have enforced a high degree of competition among the fragments. However, such a monopoly charge could not have been sustained. We must decide whether we have any better ground now than the court did in 1916 for expecting workable competition in the future.

American and Continental together would still be able to dominate the industry in the short run if they maintained effective collusion in every dimension of the market. But instead the two large firms are in a state of intense rivalry. The challenger is staging a vigorous drive on the markets of the leader. A number of large accounts previously held by American have now been split. Technological rivalry is unrestrained. While open price warfare has been avoided, except in a few local instances, there is protracted maneuvering in an atmosphere of great uncertainty when the time for quoting new prices and renewing contracts comes round every year. American's price leadership is now merely barometric, and it cannot count on being followed by Continental or by any other seller.

The smaller firms, aided by a strong growth trend in the industry, have been establishing themselves on a more secure footing and have also been edging into the markets of both the leaders. In several instances recently the price structure of the majors has had to be adapted to the independent competitive tactics of a smaller firm aggressively reaching for a larger share. Although the oligopoly structure remains, enough has been said to show that the competitive pattern is in a state of flux.

No outsiders have entered can manufacture in the last three years, but the threat of entry from the buying side has been intensified. Large buyers can no longer be pacified with volume discounts. During the past year the very largest consumer of packers cans began to manufacture part of its own requirements, and another large buyer, located in one of American's few remaining monopoly territories, has announced

plans to manufacture all its own containers. The effect of these events is incalculable. Both of the large sellers are constrained as never before by the threat of backward integration, and the restraints exercised on profit margins in cans work to the benefit of all buyers.

American itself has no monopoly weapons left in its hands. Any firm which relies on size and the momentum of past achievements alone to protect it against competition is likely to find its position deteriorating with alarming speed. What American can do is to take advantage of the moderate superiorities it evidently has in efficiency and research. It can maintain its leading position by reducing costs and continuing its rapid rate of product development, passing along the benefits to consumers. Its large rival will push it, or perhaps lead it, in this respect. Its smaller rivals will offer a stronger competitive challenge as time goes on. And its large customers will never be more than a few years behind in potential efficiency if they should decide to produce their own supplies. A forecast of workable competition appears to be justified. Thus it seems that the Antitrust Division, in choosing to make a limited attack on market practices—however much it may later have doubted its own wisdom—made a good decision after all.

THE BASING POINT DECISIONS AND THE STEEL INDUSTRY

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Until a few years ago, the basing point system was used in a number of important American industries, including the iron and steel industry, the cement industry, the corn wet milling industry and others. Several Federal Trade Commission (FTC) orders, some adjudicated and some not, have put an end to the system; the key case was undoubtedly the one against the cement industry, on which the Supreme Court ruled in 1948. The purpose of this paper is to examine the effect of these decisions, with particular reference to whether they will significantly increase price competition in the former basing point industries.

The first step will be to discuss some of the rulings of the FTC and the courts and to assess their impact on the basing point system and their general economic significance where appropriate. The next step is to determine if possible whether the rulings will produce a greater degree of price competition in the former basing point industries. The discussion will be particularized for the iron and steel industry, which until 1948 used a multiple basing point system. The final few paragraphs will be a description and brief analysis of events in the steel industry since the end of the basing point system.

The term "basing point system" will frequently be used without qualification, even though the system has taken several forms. Sometimes the context will make clear what form is referred to. More often, the reference is intentionally generic; i.e., to a method of pricing which regardless of variations in detail permits only delivered pricing, and makes the price to a buyer the lowest combination of basing point price and outbound rail freight charges, regardless of the actual location of the seller and regardless of the form of transportation actually used.

I

Beginning in 1937, after years of inactivity in this field, the Federal Trade Commission issued a number of complaints against firms and industries using the basing point system. The Commission challenged single basing point systems used by the Corn Products Refining Company and the A. E. Staley Manufacturing Company and multiple basing point systems used in the cement, rigid steel conduit, iron and steel and corn wet milling industries. Three other complaints against the Morton Salt Company, Samuel H. Moss, Inc., and the Standard Oil Company (Indiana), though not concerned with basing point pric-

ing, produced applicable interpretations of the Robinson-Patman Act. All of these complaints except two have been adjudicated; the corn wet milling order was accepted by the industry and the steel order was negotiated between the parties.

It is not necessary for present purposes to discuss these cases one by one; a short and unfortunately rather dogmatic summary should suffice. It will be convenient to divide this summary into two parts, corresponding to the two laws upon which the Commission has relied: the Federal Trade Commission Act and the Clayton Act as amended by the Robinson-Patman Act.

The Federal Trade Commission Act declares unlawful "unfair methods of competition in commerce, and unfair or deceptive acts or practices in commerce. . . ." The Commission has used this act to attack basing point systems as combinations, conspiracies, or planned common courses of action. Recent adjudication has established the following:

1. *A basing point system used pursuant to a combination or conspiracy is an unfair method of competition.* This conclusion would seem so obvious as to require no justification. Notwithstanding, respondents in the Cement case challenged the jurisdiction of the Commission, on the grounds that the combination of which they were accused was an offense under the Sherman Act, against which the Commission could not proceed. The Supreme Court ruled that the Commission had jurisdiction under the Federal Trade Commission Act even if the offense also violated the Sherman Act. The Court added that individual or concerted conduct falling short of a Sherman Act violation could still violate the Federal Trade Commission Act.

2. *A combination or conspiracy need not be proved by direct evidence; it may be proved by circumstantial evidence, including evidence of market behavior.* Ruling in the Cement case, the Supreme Court stated that the Commission was authorized "to find understanding, express or implied," from evidence that the industry's institute worked to maintain the multiple basing point system, that the pricing system produced uniform (sic) prices, and that respondents followed the system. The Seventh Circuit Court of Appeals in the Rigid Steel Conduit case stated that an agreement could be shown by circumstantial evidence, including evidence of price conformity and of the existence of a plan which equalized delivered prices. These decisions give to economic tests a substantial place in determining desirability and legality. They also open the door to orders which can strike not only at written agreements but also at the economic acts which agreement may have been designed to perpetrate.

3. *Although not clearly established, it is possible that basing point*

systems are illegal per se. The FTC complaint in the Rigid Steel Conduit case was divided into two counts, each alleging a violation of the Federal Trade Commission Act. The first charged the existence of a conspiracy, effected by use of the basing point system. The second, however, did not charge combination, conspiracy or unlawful discrimination. Instead, it charged that the sellers individually violated the Federal Trade Commission Act "through their concurrent use of a formula method of making delivered price quotations with the knowledge that each did likewise. . . ." The cease and desist order included prohibitions based on both counts and was upheld by the Circuit Court. The Supreme Court, by a vote of 4-4, refused to overrule.

The impact of this case on basing point pricing is still not clear. On the one hand, the Commission brief before the Circuit Court stated that Count 2 was "frankly directed against the basing point practice as being, *per se,* an unfair method of competition. . . ." On the other hand, members and employees of the Commission later denied that basing point systems were under attack as such and argued that the Conduit case was basically a conspiracy case. The statement in the brief probably reflects the Commission's true intent; it is significant that the later statements followed anguished howls from the business community and the start of hearings by a Congressional committee.

The Clayton Act as amended states "that it shall be unlawful . . . to discriminate in price . . . where the effect of such discrimination may be to substantially lessen competition or tend to create a monopoly. . . ." Certain price differences, however, are exempted. The exception for lower prices "made in good faith to meet an equally low price of a competitor" is the only one relevant to a basing point situation.

In order for a basing point system to be found unlawful under the Clayton Act, it must be established that (1) it leads to discrimination in price, (2) the discrimination may lessen competition, and (3) the discrimination is not justified in one of the ways provided by law. Adjudicated cases seem to establish the following on these points:

1. *A discrimination is merely a difference in price.* The Supreme Court ruled in the Morton Salt case that the Federal Trade Commission, to establish a discrimination in price, had only to show that a seller had charged one purchaser a higher price for like goods than another purchaser. The legality or illegality of the discrimination would depend on whether the seller positively justified it in a way provided by law and on whether it had an adverse effect on competition.

2. *In a basing point situation, price is not the delivered price paid by purchasers, but the mill net realized by sellers.* In all of its basing point complaints, the FTC has considered price to be the seller's mill net, defined as delivered price less the cost of transportation from

seller's plant to place of delivery. Under this definition the basing point system necessarily produces discrimination, because sellers regularly absorb freight on some sales. Producers located at basing point have objected vigorously to this concept. They argue that delivered prices on freight absorption sales are the same as those charged by the mills whose prices are being met; hence, there can be no question of unlawful discrimination.

This argument is neither convincing nor necessary, the latter because freight absorption can be justified on other grounds. Any firm selling on a delivered basis will of necessity discriminate in price, unless its customers are located at a common point. However, its price differences are legal when they correspond to differences in the cost of delivery; they are also without fault economically as long as the cheapest forms of transport are used. If not, then *ceteris paribus* there is a misallocation of resources. If, however, a firm absorbs freight, its differences in delivered price no longer correspond to differences in the cost of delivery, and may be illegal no matter how price is defined. Its discriminations are suspect from an economic point of view because they lead to a possible misallocation of resources.

3. *It is not necessary to find that a price discrimination has injured competition, but only a reasonable possibility that it may.* Price differences constitute discrimination, but not all discriminations are unlawful; to be unlawful, a price discrimination must be injurious to competition. The Supreme Court, in both the Corn Products case and the Morton Salt case, held that it was not necessary to find actual injury to competition but only a "reasonable possibility" that it might occur. The Court went even further in the Morton Salt case, when it ruled that the Robinson-Patman Act justified a finding of injury to competition by showing "injury to the competitor victimized by the discrimination."

Some lawyers fear that an irresponsible Commission, freely interpreting the doctrine of reasonable possibility, will initiate complaints based only on a remote potentiality. But the Commission, in its 1948 statement of policy, denied any intention of doing so; the courts, in any event, would undoubtedly not permit it. On the positive side, the doctrine of reasonable possibility simplifies proof and permits the prohibition of practices which to an economist are clearly a danger to competition but which in the past have not met the rather legalistic tests of the courts.

The ruling which equates injury to competition with injury to a competitor is the really controversial one. Given pure competition as a standard, there is no place for price discrimination; since discrimination is a sign of monopoly, and monopoly and competition are mutually exclusive, the elimination of discrimination must perforce promote com-

petition. When the attainable and hence the standard is something other than pure competition, the answer is not so simple. It is still true, of course, that discrimination is the product of monopoly power. It is no longer true, however, that discrimination is necessarily anticompetitive. In a market where small numbers makes possible control over prices, as in the typical basing point industry, price discrimination has been a powerful competitive force when it actually undercuts rivals' prices.

It is in precisely these industries, however, that any competition in price, including price discrimination, will immediately and substantially affect a competitor. Hence, the two cannot be equated unless one is willing to give up competition altogether.

4. *The good faith defense of section 2b of the Robinson-Patman Act is an absolute defense to a charge of price discrimination, but probably not in a basing point situation.* The FTC ruled in the Standard Oil case that the good faith defense was procedural rather than substantive. As such, it was ruled not admissible if the Commission affirmatively showed injury to competition. The Supreme Court disagreed, stating that "we find no reason to destroy the defense indirectly, merely because it also appears that the beneficiaries of the seller's price reductions may derive a competitive advantage from them or may, in a natural course of events, reduce their own resale price to their customers."

It is doubtful, however, that the Court would have ruled the same way in a basing point case. In the Staley case, the company argued that it had adopted its single basing point system in good faith to meet the competition of the Corn Products Refining Company. The Court held that section 2b "does not concern itself with pricing systems or even with all the seller's discriminatory prices to buyers. It speaks only of the sellers' lower price. . . ." In its Cement decision, the Supreme Court cited the Corn Products decision and this portion of the Staley decision, and concluded that "the combined effect of the two cases was to forbid the adoption for sales purposes of any basing point system."

These statements have been disconcerting to sellers in the basing point industries. Basing point pricing involves the regular meeting of competitors' prices. The alternative is either to meet competition on an individual basis or not to meet it at all. If a seller meets it on an individual basis, he must still know how frequently and comprehensively he can do so without becoming systematic in the eyes of the law. As an executive of the United States Steel Corporation once put it, to attempt to handle 3,000 orders per day and to meet competition without following what appeared to be a systematic method was commercially impractical.

II

The FTC and the courts have clearly dealt a fatal blow to the basing point system. Adjudicated cease and desist orders have eliminated exclusive delivered pricing, the exclusive use of rail rates in the calculation of delivered prices, the industry-wide preparation and dissemination of freight rate books, and the plant which fails to quote a base price. The orders have probably also eliminated the systematic, routine meeting of competitors' prices, but probably not the individual non-collusive absorption of freight.

The important question is whether the decisions will lead to a greater degree of price competition in the industries which have used the basing point system. Views on this subject, though many and varied, tend to cluster around two poles. One general view is that the basing point system is an essential ingredient of combinations and conspiracies, which could only with difficulty be carried on without it, if at all. Hence, its elimination is per se a decisive step in promoting price competition. Some holders of this view go a step further by advocating the complete elimination of freight absorption, even if practiced individually. The other general view is that the basing point system developed quite logically and more or less inevitably from certain market characteristics, and that something like the basing point system is certain to be found in industries having these characteristics. Hence, the elimination of the basing point system insures neither a radically different method of pricing nor a markedly greater degree of price competition. These views may be called for convenience the collusive and the oligopolistic.

Since the industries which have used the basing point system have many characteristics in common, it is tempting to pursue this argument in general terms and to make what is said apply to all of them. There is the very practical objection, however, that no one person, or at least not this person, can speak with such broad authority. Moreover, significant differences among these industries do exist. Therefore the ensuing discussion will be restricted to the iron and steel industry. It may be suggested, however, that the discussion is at least broadly applicable to all the industries which have used the basing point system.

We start with the proposition that the nature and degree of price competition in an industry depends on the presence of various market characteristics or elements and the combination in which they occur. The conceptual schemes of price theory consider only a few of these elements, particularly the number of sellers and the nature of the product. The number affecting price behavior in the steel market and in other industrial markets is much greater. However, limitations of time and space will permit consideration of only a few here, which are fortunately rather decisive.

Steel sales are typically made directly by producer to consumer, without the intervention of a central market or other middleman. The steel market is characterized by the following: (1) A relatively small number of large sellers. On January 1, 1954, nineteen large integrated firms accounted for some 93 per cent of the industry's blast furnace capacity and some 90 per cent of its steel ingot capacity. The United States Steel Corporation alone accounted for 34 and 31 per cent, respectively. (2) A standardized product. Steel products are made to specification; hence, competition centers around price rather than around differences in quality, real or imagined. (3) Substantial fixed costs and a high ratio of fixed to marginal cost. (4) Freight costs high enough in proportion to sales price to be decisive in the selection of a seller. (5) A low price elasticity of composite demand but a high price elasticity of demand for the products of a single seller. (6) A significant difference between the location of capacity and the location of demand. These characteristics, with particular reference to the first two, are typical of pure oligopolies.

In the absence of collusion, what kind of price competition will result in a market with these characteristics? In the first instance, each seller would presumably name a plant price (we beg the question of what determines its level) and offer it to all comers. Buyers would purchase from a particular seller, however, only if his price was equal to or lower than the prices of cempeting sellers, taking into account the cost of outbound transportation. Hence, any given seller could do business only in his freight advantage territory; i.e., where the sum of his plant price and outbound freight costs was at least as low as that of his rivals.

Not all sellers—and possibly not any—would be satisfied to stop here. Lack of correspondence between capacity and demand, changes associated with the business cycle, changes in the pattern of demand, etc., would leave some sellers with unused capacity and a desire to increase their sales. They could do so, however, only by selling outside their freight advantage territories at delivered prices at least as low as those of their competitors; i.e., by absorbing freight and accepting lower mill nets. However, it is profitable for them to do so as long as they more than cover marginal costs, if the alternative is not to make the sale at all. There is no reason, however, for making these same concessions to buyers located within their freight advantage territories, except to meet the price cuts of a competitor. In short, there is an incentive to discriminate in price.

If sellers ignore the possible reactions of their rivals, they will act as if the elasticity of composite demand is the same as their individual elasticities of demand. That is, they will beat rather than meet competi-

tors' prices outside their freight advantage territories. To meet a price is to be less than certain that a sale will be made; to beat a price is to be absolutely certain. The difficulty, of course, is that all sellers, following the same process of reasoning, pursue the same course of action; those who suffer from price cuts retaliate. Since the price elasticity of composite demand is low, each seller experiences only a slight increase in demand and suffers an actual loss in net revenue. If each seller continues his price cutting in an attempt to recoup, the result will be cutthroat competition and prices will be driven to the level of marginal cost.

It may be argued that this description is a fanciful one, not related to anything that now happens in real life. And of course this is so, for oligopolistic sellers in real life are quite aware of their interdependence. However, the steel industry in the nineteenth century was frequently subject to cutthroat competition, and more recent episodes, particularly in the years 1911, 1921, and 1938, have brought cutthroat competition perilously close to reality. This history strongly suggests that unrestrained competition, if it occurred, would be cutthroat competition.

Bitter experience, then, if nothing else, teaches steel sellers that what one can do with profit cannot be done by all simultaneously. Hence it becomes in the common interest to avoid unrestrained competition or any competition which puts undue pressure on prices. Given this attitude and given the market characteristics of the steel industry, it is not unreasonable to suppose that a pricing system like the basing point system would develop. Sellers at a location with one or more rivals would charge their plant price plus outbound freight up to a point where a competitor's price similarly calculated was lower; then they would meet the lower price but not beat it. Isolated sellers might find no reason to quote a base price; if not, they would simply meet competitors' prices at every point of delivery. Thus would develop the familiar outlines of the basing point system with its dichotomy between base and nonbase mills.

What purpose does the basing point system serve in this context? The problem is to keep freight absorption from starting an unprofitable chain of price cuts and retaliations. This can be done if sellers follow a policy of meeting rather than beating competitors' prices outside their freight advantage territories. The basing point system makes it easy for such a policy to be carried out, for it tells each seller what every other seller will charge at every point of delivery. In effect, it eliminates the uncertainty which oligopolistic sellers normally have about their rivals' prices. One must distinguish, however, between pricing policy on the one hand and the mechanism for carrying it out on the other hand; the two are not the same. The policy, in this case, is to

meet rather than beat competitors' prices; the mechanism for carrying it out is the basing point system. The basing point system will do the things claimed for it here only if a desire to follow the policy exists in the first place. In other words, there is nothing in the basing point system which prevents the undercutting of rivals' prices if sellers choose to do it. Conversely, the policy of meeting rather than beating competitors' prices can be carried on without a basing point system, though with considerably greater difficulty. By the same process of reasoning, the basing point system does not cause discrimination; it merely keeps discrimination within certain bounds. It follows that the mere elimination of the basing point system does not remove the incentives which produced it and does not automatically lead to a radically different pricing policy.

The basing point system, in this view, has two indispensable elements: exclusive delivered pricing and a common method for calculating delivered prices. If f.o.b. sales are permitted or if delivered prices are calculated in different ways, sellers cannot know in advance the exact prices they have to meet, and the system fails to serve the purpose for which it was adopted. Freight absorption is an indispensable element in another sense, for if it is not permitted, the need for a basing point system disappears.

Those who adhere to the collusive view of the basing point system reject this line of reasoning, on one or more of three grounds. First, they argue that the foregoing description implies development of the basing point system through natural evolution, without the intervention of human will or volition. They then easily show that natural evolution could not possibly account for such a complicated and detailed method of pricing. Second, they assign quite a different role to small numbers. Fewness of sellers is considered important because it facilitates collusive agreements to use the basing point system, and not for the reasons accepted here. Third, they argue that a satisfactory degree of price competition can be secured in the former basing point industries through use of the uniform f.o.b. mill pricing system. Each of these points will be considered in turn.

The theory of natural evolution is indeed easy to demolish, for it is only a straw man. Those who adhere to the oligopolistic view do not claim that the basing point system came about automatically and impersonally. They claim only that it was not an artificial imposition, since the market characteristics of the steel industry and certain other industries make the emergence of a pricing system like the basing point system highly probable. Human intervention in the actual development of the system is not precluded.

Whether basing point systems are collusive devices is a question of

fact. The Supreme Court in the Cement case said: "We think that adequate findings of combination were made and that the findings have support in the evidence." The Seventh Circuit Court ruled similarly in the Ridgid Steel Conduit case. Users of the basing point system, however, have vigorously dissented. It is not necessary to decide here whether the courts were right or wrong. For, from the point of view of public policy, the real question is not whether there has or has not been collusion, but whether the mere elimination of collusion, if it exists, will produce a substantially different form of rivalry.

The foregoing analysis suggests that it will not, particularly if one's standard is near to the purely competitive. The incentive to discriminate and the further incentive to keep discrimination within certain bounds exist independently of any possible collusion and would continue to exist after it was ended. The basing point system, in short, only expresses and formalizes a type of price behavior which arises for reasons other than collusion. This is not to suggest that collusion is to be countenanced; it is only to suggest that its elimination, if it exists, would not be decisive. To change pricing policy in a way which makes sense economically one must change the market conditions which produced it.

The uniform f.o.b. pricing system requires that a seller receive the same mill nets on all sales; freight absorption is excluded. Therefore, each seller has a local monopoly in his freight advantage territory, which he can enlarge only by lowering his mill price. Proponents of this pricing method expect that it will lead to a substantially greater degree of price competition, because sellers are provided with a compelling incentive for reducing their plant prices; i.e., the expansion of sales territories in which they can have no rivals.

This reasoning ignores the realities of pricing under oligopoly; it erroneously assumes a one-to-one relationship between an incentive to cut prices and actual price cuts. The true situation would seem to be as follows: If business at the geographic margin is substantial, there is an incentive to cut prices to get it. However, the returns from this added business must be balanced against the lower returns on sales which could be made without a price cut. If the marginal sales are still worth making, then they are by the same token too lucrative to lose, and a price cut would probably be met. Other sellers might meet a price cut even if the marginal business was not lucrative, in order to prevent their customers from being placed at a disadvantage in competition with the customers of the price cutter. If for either reason a price cut is met, all sellers are worse off than before, including the original price cutter. The practical result is that price cuts are not made. As soon as a mutually satisfactory pattern of f.o.b. prices has been established, it tends to remain unchanged; a *de facto* market sharing agreement de-

velops. The likelihood of price cuts is therefore even less than under the basing point system. A base price cut by a single basing point seller makes freight absorption by his competitors more diffcult, but it does not bar them from entire portions of the market. Hence there is less pressure on competitors to meet it, and therefore a greater probability that it will be made.

The elimination of freight absorption is considered a salutary result by many, on the grounds that merely meeting a competitor's price is not really competition. We have seen, however, that competition in the steel industry is likely to take the form of freight absorption; that is, given the cost structure of the industry, both sales with and without freight absorption are competitive, each reflecting the different force and character of competition in different parts of the market. Since mill price reductions are not a real alternative to freight absorption, the prohibition of freight absorption involves a subtraction from competition and not an addition.

If the prohibition of conspiracy is beside the point, and the uniform f.o.b. mill pricing system undesirable, what is the appropriate alternative to the basing point system, if any? The answer lies in the reasons for which the basing point system was used. As we have seen, it produced certainty with respect to the structure of steel prices, and kept discrimination within certain bounds; it favored price meeting rather than price beating. The proper pricing alternative is surely one which puts back, so to speak, the uncertainties which the basing point removed and which lets competition, even including price discrimination, have the maximum possible effect on the prices paid by buyers.

The pricing method which best does these things combines features of both the basing point system and the uniform f.o.b. mill system. As with the uniform f.o.b. mill system, each seller is required to post a plant price and to sell f.o.b. at that price. Delivered sales are permitted, but they must include only the actual cost of the form of transportation selected by the buyer. As with the basing point system, sellers are permitted to absorb freight, but only if a competitor has quoted or can quote a lower price on the product type and size being offered. The last requirement forces sellers to make individual rather than collective decisions about freight absorption and prevents pricing from becoming completely routine. It also forces sellers to find out what their competitors are actually doing, and may lead to a more active, outgoing, and vigorous pricing policy.

Because sales under this method are made f.o.b. and because there is no longer a commonly accepted method for calculating delivered prices, exact prices at every point of delivery are no longer known in advance. It is more diffcult for a seller to know what prices he has to meet, and

more likely that he will beat rather than merely meet competing prices. It is also easier for buyers to play one seller off against the others.

Although this pricing method should produce a more effective type of price competition than the old basing point system, the difference will be in degree and not in kind. The plain fact is that rivalry in the steel industry and in similar industries is not likely to take the form of competition in price. The reason is not that conspiracy prevents it, or that steel producers are improperly motivated; it is rather that the conditions necessary for vigorous competition do not exist, as a matter of objective economic reality. Therefore, price competition of a substantial sort cannot be brought about through changes in pricing systems or through the artificial imposition of the symptoms of price competition. Unless and until the structural characteristics of the steel market are changed, if they can be, no tinkering with pricing systems will really pay off.

III

It would be very instructive if the preceding analysis could be tested against the performance of the steel industry since the elimination of basing point pricing in 1948 and the drawing up of the FTC steel order in 1951. Unfortunately, the period has been much too short; moreover, business was at such a high level for most of it that there was no particular pressure on prices. However, a brief look may be suggestive.

Very briefly, the FTC steel order prohibited producers from entering into any planned common course of action, understanding, or agreement, or from acting independently or otherwise so as knowingly to contribute to a planned common course of action, understanding, or agreement, to do the following: (1) fix the price of steel or any element thereof; (2) compile, circulate, and use lists of base prices and extra charges; (3) compile or circulate lists of freight rate factors; (4) adopt special pricing practices for particular classes of customers; (5) sell steel at prices calculated in accordance with a formula which produces identical delivered prices or which establishes a fixed relationship among prices; (6) fail to sell f.o.b.

In addition, the order provided that the FTC would not consider uniformity of price "alone and without more" as showing a violation of the law, and that it would not prohibit freight absorption as such "when innocently and independently pursued, regularly or otherwise, with the result of promoting competition." Thus the order, in its essentials, conforms quite closely to the pricing method suggested above as an alternative to the basing point system.

After being at or near capacity for a number of years, steel operating rates began to fall off in the fall of 1953. The period since then is

therefore the most significant from our point of view. The *Iron Age* reported, as early as September, 1953, that a few companies were again absorbing freight. In October, most companies formally announced that they would do so. According to the *Iron Age*, the policy was to consider each case individually, and to absorb freight only if there was evidence that a competitor was quoting lower prices. Producers were not offering to absorb freight to all buyers at a given locality, but only to those whose business was particularly attractive. The producers were planning to continue f.o.b. prices, and were not returning to the old basing point system. In December, the *Iron Age* reported that producers were going to a lot of trouble to make sure that competition actually existed before they absorbed freight, including a check to determine that their rivals actually produced the type and size in question. In July, 1954, the *Iron Age* reported that freight absorption might cost as much as 70 million dollars in 1954, or an average of $.75 to $1.00 per ton and that it had become a routine part of the business. It is clear, therefore, that freight absorption has been renewed on a wide front. There is no evidence, however, of the systematic, routine meeting of competitors' prices which characterized the old basing point system. There is also no evidence of price beating rather than price meeting or of a general increase in the forces of competition over the basing point days.

There have also been interesting developments in the transportation of steel. Under the basing point system, delivered prices were made up of base prices plus all rail freight, regardless of the method of transportation used. In practice, rail transportation, the most costly form, was used almost exclusively. A marked change began with the introduction of f.o.b. pricing in 1948; trucks, in particular, were used with increasing frequency. Figures for 1953, compiled by the *Iron Age*, show how far the trend has gone: on an over-all basis, trucks carried 53 per cent of total steel shipments, the railroads 44 per cent, and barges 3 per cent. Hence, the end of basing point pricing has clearly resulted in the use of varied forms of transportation. Since the cost of the form actually used now determines the price at the point of delivery and since the determination of freight costs is tricky even for a single form of transportation, uncertainty about competitors' prices has been substantially increased.

The steel industry's recent experience is inconclusive for the determining whether there will be a greater degree of price competition without the basing point system. No real answer will be possible until a number of years have passed and the industry has experienced varying demand conditions. It is significant, however, that the resumption of freight absorption almost immediately followed the drop in operating

rates. The pressure to expand sales when capacity is not fully utilized is clearly an important and ubiquitous force. It is also significant that there has been a major shift to forms of transportation ignored under the basing point system and that the freight absorption which has taken place has been on an individual and not a systematic basis. There have been some beneficial results, therefore, from the elimination of the basing point system. Perhaps the future will see more.

DISCUSSION

WALTER ADAMS: Mr. Whitney's study of the movie decree is comprehensive and competent. The facts presented are up to date and accurate. Nevertheless, I disagree with Mr. Whitney's general emphasis and his unduly pessimistic conclusion.

The purpose of an antitrust decree is to "effectively pry open to competition a market that has been closed by defendants' illegal restraints." Such a decree cannot, however, in and of itself, effectuate a permanent change in industry structure or practices. All it can do is to break up or render impotent, for the time being, the monopoly power which violates the law. All it can do is to remove temporarily some of the artificial restraints which in the past had limited the freedom of competitors and customers. The test of a decree's effectiveness is not that it minimizes complaints by vested interests nor that it protects any group from economic hardship or inconvenience. The test is whether substantial competition has been restored to the market place; whether consumers have a wider range of choice; and whether competitors have more freedom and potential competitors greater opportunity than before. With this standard in mind—and remembering that the last of the movie decrees took final effect only last fall—my appraisal is less pessimistic than Mr. Whitney's.

First, it seems clear that seldom (with the exception of World War II) have independent producers been as successful as since the divorcement of the Big Five. With the vertical control over choice exhibition outlets broken, independents have enjoyed an unparalleled opportunity to market a quality product. Moreover, here is one case where a comprehensive divorcement decree is not likely to impede technological progress. As Professor Hellmuth has pointed out, practically all major innovations in the industry—the star system, multireel features, sound, and now three-dimensional films—have been made by what were minor companies at the time. Here is one case where integrated size has not given us better things for better living through research.

Second, as Mr. Whitney concedes, the public has benefited from an improvement in film quality. Though admission prices have gone up, the consumer probably gets more picture quality per dollar today than he did in 1948. In addition, he has more first-run choices than ever before. Finally, not least among his blessings is the greater frequency and wider distribution of foreign films—the superb sophistication of Alec Guinness and the unequalled dramatic talent of, say, Gina Lollobrigida. After a moment's reflection, Mr. Whitney will, I am sure, concede this point.

Third, the exhibitors have no doubt suffered. But it seems to me that neighborhood theaters, relying primarily on a steady stream of mediocre films, would have been injured by TV in any event. It is true, of course, that the divorcement decree destroyed the umbrella which the old monopoly system spread over the small fry. It is true that the decree (as well as the advent of TV) accentuated the difference in reward between good pictures and bad.

It is true that these changes were a painful experience for many exhibitors. This does not mean, however, that the changes were bad or against the public interest.

Finally, and probably most important, is a point Mr. Whitney treats only casually and nonchalantly; viz., the extent to which the government itself has undermined the effectiveness of the antitrust decree. In 1953, the Federal Communications Commission approved the merger of Paramount Theaters and American Broadcasting Company, recognizing the combination's right to produce movies for TV exhibition. The Commission also acquiesced in the control by Paramount Pictures of DuMont TV—the fourth largest network in the country. If this development foreshadows a general trend in the industry, we may—as Professor Hellmuth points out—be witnessing the vertical reintegration of the Big Five—the very integration which the antitrust suit sought to destory. Obviously, horizontal mergers between movie exhibitors and TV chains would vitiate the stimulus of interindustry competition. Obviously, vertical mergers of motion picture producers and TV exhibitors would revive the very industry structure which entailed a maze of trade restraints. This, rather than the antitrust decree, is a cause for concern.

The recent action of the FCC has, I think, demonstrated once again that government is not an agent of countervailing power; that, instead, government often supports the economic forces which breed monopoly; that the government's exercise of administrative or regulatory powers often creates the very restraints which antitrust is subsequently required to dissolve. In short, here we may have a case where government approves what the antitrust laws are designed to prevent—where government helps remake an industry in the image of the cartel.

SAMUEL M. LOESCHER: Mr. Marengo is indeed pessimistic about the prospects of increasing effective (price) competition in the steel industry by means of antitrust policy directed at trade practices. Rigidity in pricing is attributable primarily to the industry's structure. Therefore, he draws the conventional, and safe, academic conclusion that public policy should be directed at altering (if possible!) the structural characteristics of the steel market. Now most economists would probably subscribe to, and believe to be economically feasible, a policy of limited restructuring of the steel industry. But that extensive dissolution is a practical policy alternative is most unlikely. As Dean Edward Levi has reminded us ("A Two Level Antimonopoly Law," *Northwestern Law Review*, December, 1952, pages 567-568), the courts have yet to adopt a realistic percentage share test of illegal market control. Furthermore, in the unlikely event that the courts moved toward a percentage test, market control can be calculated only after a meaningful designation of the market, in terms of both geography and products. In adversary proceedings, arguments as to the market would represent no little war of containment.

As a consequence, unless we are to allow the quest for more effective competition in the steel industry to go by default, it will be necessary for public policy to concern itself with behavior and trade practices. Moreover, there are

important reasons for believing that an attack on trade practices offers opportunity for substantial improvement in terms of price behavior.

I should like to agree with the author that nondiscriminatory f.o.b. mill pricing does not offer the best alternative to the basing point system. But I cannot accept his conclusion that compulsory f.o.b. pricing would make for less price flexibility than the old basing point system. If nondiscrimination were the only practical policy alternative, I would readily cast my vote for f.o.b pricing for two reasons. First, the spatial distribution of sellers in the steel industry is such that the gain of important markets at the geographical margins by a firm which lowers its f.o.b. price is likely to be greater than the individual market losses suffered by the rival sellers. As a result, oligopolistic reckoning notwithstanding, some significant uncertainty exists as to whether all excluded rival sellers will retaliate. Such uncertainty differs markedly from the built-in automatic multiple-price leadership of the basing point system. Under the familiar formula, base price increases and decreases are bound to be met in the initiator's "natural" market, save for the effect of encouraging or discouraging access into this market to the rivals most distantly located. Secondly, geographical shifting demand, which we might call the Stiglerian effect, although less striking than in the cement industry, is fairly significant in steel since construction has recently been estimated to absorb 20 per cent of steel's output. Long-term as well as short-period peripatetic shifts in steel consumption should prevent a simple freezing of market shares based on stable intermill price differentials. Reconciling of conflicting interests without flagrant consultative collusion stretches too far the elastic possibilities of imperfect competition theory. However, there is something fairly definite and orderly about compulsory f.o.b. pricing systems which suggests that geographical pricing practices incorporating even greater uncertainties should be sought.

I agree with Mr. Marengo when he emphasizes that freight absorbing delivered prices were an inevitable quasi-competitive marketing technique in industries with market structures such as steel. I also agree with his emphasizing the distinction between the price policy restraining competition which an industry with a few sellers might wish to adopt and the mechanism—a system or formula—which could guarantee success in the price policy so long as each seller had merely the will to adhere to it. Mr. Marengo, however, believes that all rail delivered prices and the circulation of freight books were the essential mechanical elements of the basing point system. These elements permitted rival sellers routinely to meet and avoid the fears of beating competition when absorbing freight.

It seems to me that one's analysis of formula pricing must be carried further, and will be improved if one emphasizes imperfect collusion in place of the more conventional dichotomy of collusion and oligopolistic imperfect competition. In every basing point system which I have studied, there has always been the refusal on the part of sellers to offer to buyers located in each mill's "natural" market (in which freight is not absorbed) an option of a bona fide f.o.b. mill price. Bona fide f.o.b. mill prices are considered to be the posted base mill prices without fictitious premiums. Given bona fide f.o.b.

mill prices, several local buyers might wish to purchase from the mill and transport the product in the buyer's private, leased, or contract trucks. With uncontrolled trucking, there are no published transportation charges. More distant sellers find it not only difficult but virtually impossible to determine the destination cost of steel to the buyer. At least within the range where trucking is economically feasible, formula destination prices could not be ascertained. Therefore, a freight absorbing distant mill which seeks to penetrate many of his rivals' natural markets cannot meet competition, but on the contrary will probably win such patronage only through an inadvertent beating of competition. A distant seller either partakes in unsystematic pricing or he declines to penetrate.

The steel industry in 1935, after the demise of the NRA, simultaneously and arbitrarily imposed on truck shipments a surcharge equal to 35 per cent of the applicable rail freight. This action followed the passage of a resolution by the American Iron and Steel Institute. That such an action represented a collusive act seems unmistakable. Of course, given the fact that any basing point system involves profit reducing cross hauling suggests that the resulting collusion was imperfect. But by discouraging buyer's trucking, the formula of known destination prices was preserved, and achieved, if you will, effective collusion. Once trucking is eliminated or controlled by the seller (so that published transportation tariffs are made available), further overt collusion is unnecessary. The ability merely to meet competition is available to those who have the will to maintain the system.

Have the basing point decisions effectively altered trade practices in the steel industry? I believe the answer is "no"—and my reply applied especially to the period between September, 1953, and November, 1954, of operations substantially below capacity. I agree with Mr. Marengo that freight absorption during this latter period has been performed on an "individual" basis rather than routinely in accordance with a simple formula. But except for precluding the old practice of identical formula sealed bidding to state and local governments, the so-called "individual" meeting of competition still represents all the essential characteristics of a system for identical pricing. The only difference may be that in the absence of an organized distribution of freight books, and with the added presence of common carriers and sellers' controlled trucking, the verification of a much greater number of freight rates becomes a more complicated task. Errors of freight rate calculation may more frequently lead to some beating of competition.

There has been a transportation revolution in the steel industry in the postwar period as trucks now haul the majority of total steel shipments. The basing point decisions have been partly responsible for the change and a more rational and economical use of the nation's transportation facilities has resulted. However, the major advent of trucking of steel products does not appear to have brought about, to the extent which might have been expected, the erosion of a freight formula for purposes of systematically meeting competition. Recently I made a survey of steel companies, to which seven major (million-plus ingot ton capacity) and eight minor firms responded. It indicated that the overwhelming proportion of trucked steel products move

to buyers in common carriers, supplemented by the seller's contract, leased, and private carriers. In each of these cases of controlled trucking, tariffs are either filed with public commissions or the steel producers are able to publish truck rates to sundry destinations. With the exception of two minor steel concerns, the responding steel companies referred to transportation in buyers' contract, leased, or private trucks as being "small, relatively small, or negligible." I have been unable to determine as of this date if some practice may have been adopted so as to discourage transportation in trucks controlled by the buyers. Familiarity with controlled trucking in the cement industry, however, leads me to suspect that bona fide f.o.b. prices are not being offered.

An evaluation must necessarily follow concerning the FTC's cease and desist order which prohibits failure to sell on an f.o.b. basis. I believe that the Commission's order against the iron and steel industry is highly defective.

Although the FTC's findings of facts do not find conspiracy (implicit or otherwise), the initial complaint and the subsequent consent order are clearly phrased in the language of conspiracy. And even more important, the FTC appears to have blundered in failing to prohibit each respondent individually from quoting fictitious f.o.b. prices and refusing delivery to buyers who wish to use their own form of transportation.

That the Commission failed to draft an effective trade practice order for the steel industry is strikingly illustrated by an unsuccessful attempt of one major steel company to sponsor an industry-wide delivered zone pricing system for steel. In December, 1953, Republic Steel initiated for hot-rolled carbon bars a delivered price, based on an average freight factor from its mills, for each of 1,481 counties it normally served. That bona fide f.o.b. prices were not to be offered was apparent in Republic's statement that the "consumers in switching areas adjacent to mills have the option of free delivery or can come into the mill and pick up the steel themselves" at an f.o.b. price including phantom freight. Republic, at the time, also announced that it still intended to meet local competition on an "individual" basis. That Republic was taking the leadership and hoping that other steel companies would accept the invitation to establish an all-delivered zone pricing system can be inferred from Republic's October, 1954, announcement. This stated that Republic was abandoning its zone system because other firms failed to follow. Meeting of competition under a county zone system of delivered prices would indeed have been vastly more definite than even the old basing point system. Other firms in the industry may have feared a general adoption of the zone system because of the vulnerable elements of phantom freight which would have been inherent in single deliverd prices for entire counties. Moreover, systematic geographical price discrimination on an individual basis had been fairly satisfactory.

Some increase in effective competition in the steel industry, even in the absence of a major restructuring, would appear to be feasible. However, the current model of meeting competition on an individual basis, albeit an improvement over the older simplified base-price-plus-rail-freight formula, represents less of an innovation than can reasonably be expected of a progressive antitrust policy.

JAMES H. STAUSS: In his lucid analysis of the metal container industry, Professor McKie emphasized the economic consequences of the American Can case of 1950. Among other judgments, he said that "the gross structure of the industry has not changed very much and probably will not change much in the future." He also concluded, in view of the impact of the decree, that "a forecast of workable competition appears to be justified." I shall comment on these two judgments, and I shall do this in terms of market structure, performance standards, and mutually completing aspects of structure and performance.

In terms of structure, my impression is that the following situation prevails in the metal container industry:

1. The industry is, and probably will be, characterized by "fewness of sellers," although a significant number of concerns have little spots of business after the large bulk of the industry is accounted for with respect to sales.

2. The industry has, and probably will continue to have, two "large" sellers sharing most of the industry's output and several "small" sellers sharing most of the remainder. A similar generalization fits the realities of separate markets, regionally or locally.

3. The industry is free of cartelization, and it is prudent to expect a continued absence of collusion.

4. The impediments to entry are not, and probably will not be, seriously formidable insofar as patents and capital requirements are concerned. On the other hand, ease of entry is not characteristic of the industry; that is, the established leaders have, or are thought to have, critical advantages in the management and unified servicing of multiplant enterprise, in the quantity purchasing of raw materials, in the financing and carrying out of research and product improvement, in the possession of marketing know-how and historic patronage, and in the waging of non-price rivalry. It is not clear that ease of entry will be characteristic of the industry, even though the 1950 decree knocked out a number of formidable exclusionary devices, especially cumulative volume discounts, long-term requirements contracts, ties between the leasing of closing machines and the sale of cans, and supports for the dependence of can buyers on the closing machines of American and Continental. These devices, representing customers tied to the established can suppliers, once protected the major market shares. Yet, now that the exclusionary devices are temporarily or permanently outlawed, only certain buyers of cans offer a significant threat of entry through the manufacture of their own containers. It is not clear that this threat will be a decisive disciplinary force acting on the established leaders.

5. The structural conditions, which are oligopolistic, compound themselves into alternative sources of supply available to buyers. From one point of view, these alternative sources of supply are "enabling conditions" making it possible for buyers to select their sellers. In consequence of the American Can case of 1950, these enabling conditions were strengthened, because tying arrangements were broken, dependence on closing-machine leases was greatly reduced, and requirements contracts were shortened. The canning industry, much of which is effectively competitive, is more independent than before,

and it has an increased opportunity for choice among container manufacturers. From another point of view, however, the alternative sources of supply are "limiting" conditions that restrict in some measure the range of decisions and actions practically available to each container manufacturer.

As I see this structural situation, the two pertinent questions are these: (1) Are the buyers' enabling conditions equivalent to "genuine" or "real" alternatives of supply—in terms of downward pressures on price, upward pressures on product improvement, and opportunity to avoid price lethargy and strictly parallel action by sellers? (2) Are the sellers' limiting conditions equivalent to "genuine" or "real" disciplinary restrictions—in terms of a rivalry that compels the sellers to participate in downward pressures on costs and prices and in upward pressures on product improvement? The structural situation, present and prospective, in regard to fewness of sellers, dominant market shares, and impediments to entry, does not appear to represent "workable competition." However, this competition could emerge in a mixture of effective intraindustry rivalry, effective intercommodity competition, and successful meeting of performance standards applicable to the metal container industry.

In terms of performance, my impression is that the uncertainties are too great to justify a forecast of workable competition. I accept Professor McKie's judgment that small sellers reaching for enlarged market shares, as well as certain buyers threatening backward integration, will enhance price rivalry to some extent. I also agree that technological and service rivalry will act as a disciplinary process. On the other hand, the established leaders will probably safeguard their dominant shares through their superiorities in waging this technological and service rivalry. The downward pressures on price may not pervade the bulk of most markets, and parallel—and perhaps rigid—pricing may be common practice. It is too early, moreover, to estimate the future strength of intercommodity competition from fiber, plastic, aluminum, and glass containers, and it is not clear that the metal container industry would try to protect itself by reduced-price measures. Finally, a forecast of workable competition in the metal container industry must depend on how well the industry is meeting, and how well it is expected to meet, standards of performance in terms such as reducing costs, making price reductions pursuant to cost reductions, and keeping down its level of profits.

Although I believe that a forecast of workable competition is not now justified, I agree that the American Can case of 1950 has improved the economic health of the metal container industry. The case is notable, too, from the standpoint of the unprecedented control over business practices and dealings that the court decreed, particularly with respect to compulsory licensing of patents and compulsory extension of technological information. I suggest, however, that the District Court's interpretation of antitrust law in this case may be deceptive; that is, economists may conclude that economic power was cut with a sharp knife. As I read the opinion and the ruling, the emphasis is on "abuses" and on evidence of intent. In other words, antitrust law may prevent and suppress certain arrangements and behavior that "real" competitors do not engage in; but antitrust law, in its present state of legisla-

tion and adjudication, does not compel the anatomy and functions of workable competition. It does not prevent and suppress the anatomy and functions of large size, monopoly, oligopoly, or concentration, if any one of these phenomena simply exhibits economic power and only violates workable competition. Thus, in the American Can case of 1950, the district judge said, "I am not willing to burn a cathedral down because someone committed a sin therein."

RESEARCH ON THE BUSINESS FIRM

LIMITS TO THE GROWTH AND SIZE OF FIRMS*

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I. *Size Versus Growth*

There are two approaches to the question of the size of firms. The traditional approach attempts to explain size in terms of the balance of advantages and disadvantages of being a particular size. Another approach emphasizes the process of growth and treats size as a more or less incidental result of a continuous on-going or "unfolding" process. The first—which I may call the size approach—has found its exponents among those who stress the economies and diseconomies of large-scale production and among those who stress the monopolistic advantages and economic power of bigness. The second—the growth approach—has so far been expounded in any systematic form only by the "biological economists"—by those who view firms as organisms and conclude that they grow like organisms. That variant of the growth approach leaves no room for human motivation and conscious human decision and I think should be rejected on that ground. (See "Biological Analogies in the Theory of the Firm," *American Economic Review*, December, 1952, pages 804-819.)

I want here to suggest an alternative growth approach which, in common with the biological variant, insists that a predisposition to grow is inherent in the very nature of firms, but which, in contrast, makes growth depend on human motivation—in the usual case on the businessman's search for profits.

The analysis presented in this paper is most applicable to business enterprise in the form of corporations. It will be concerned only with industrial firms and only with internal growth. Growth by merger will not be considered. While it is true that merger in the broadest sense has been of great importance in the growth of firms—and of greater real significance, perhaps, than statistics of the percentage growth by merger for different firms would indicate—merger by no means accounts (at least in recent times) for even the major part of the growth of most large firms, as the studies of Weston, Schroeder, and others have shown.[1] Furthermore, I think it likely that growth can proceed just as

* I am particularly indebted to Carl Christ, Evsey Domar, and Fritz Machlup for helpful criticism of this paper.

[1] Fred J. Weston, *The Role of Mergers in the Growth of Large Firms* (University of California Press, 1953); Gertrude G. Schroeder, *The Growth of Major Steel Companies, 1900-1950* (Johns Hopkins Press, 1953).

far with or without merger, though not at the same rate. To show this, I want to consider growth without merger.

II. *Causes of and Limits to the Growth of Firms*

First, I shall be concerned with the causes of and limits to the growth of firms. It is convenient to divide the relevant considerations into two categories: those that are external to the firm and those that arise from the nature of the firm itself. For example, difficulties in obtaining capital or the existence of unfavorable demand conditions have been used to explain why some firms fail to grow, or disappear entirely. Conversely, a growing economy or changes in technology increasing the so-called "optimum size" of firms have often been given as explanations of continuous expansion. Such things do indeed have important effects on the rate of growth of firms. They are external factors which influence the speed and direction of growth but the importance of which cannot be fully understood without an examination of the nature of the firm itself. The more interesting question, to me, is whether there is anything in the nature of the economic institution we call a firm that induces growth, makes it possible, yet limits the rate of growth. This is the problem I want to discuss here: the internal incentives to and limits on growth—a theory of the growth of firms that does not relate to fortuitous external events.

The Problem of Planning. To begin with, let me stress an obvious fact, but a fact of central importance for the growth of firms: Successful expansion must, in the usual case, be preceded by planning on the part of the firm. Firms do not just grow automatically, but in response to human decisions. And if firms act on the basis of plans, it follows that they have some degree of confidence in these plans. The question therefore arises how a firm obtains the required degree of confidence. Unless it is assumed that knowledge is perfect and that uncertainty is absent—assumptions that are useless and inappropriate in this context—it is clear that a body of knowledge sufficient to sustain rational plans for action must be developed within the firm. What may broadly be characterized as managerial research will be necessary for the purpose. Consequently, some part of the managerial and entrepreneurial services of the firm must be available to work on the requisite plans whenever expansion is considered.

The fact that expansion must be preceded by research and planning, on the one hand limits the amount of expansion that can be undertaken at any given time and on the other hand permits continuous expansion through time. I propose to show that expansion itself tends to create opportunities for further expansion—opportunities that did not exist before the expansion was undertaken—and this for two reasons. First,

the execution of any plan for expansion will tend to cause a firm to acquire resources which cannot be fully used at the level of production contemplated by the plan, and such unused services will remain available to the firm after the expansion is completed. Second, planning itself and the execution of a plan will tend to absorb managerial services which will not be required in the operation of the enlarged concern and which therefore will be released upon completion of the expansion program, while at the same time the services that the firm's management is capable of rendering will tend to increase between the time when the plan is made and the time when its execution is completed.

The Continuing Availability of Unused Productive Services. Let us begin with the problem of using fully all services acquired. This raises two separate questions: Why does a firm acquire services it does not need? And why, if it acquires them, does it not use (or sell) them?

The answer to the first question involves a familiar analysis with an extended application. As long ago as 1832, Babbage set forth the principle that the most efficient output for any establishment will be that which uses a "direct multiple" of the "number of processes into which it is advantageous to divide" the production of an article. This is essentially what Austin Robinson a hundred years later called the "balance of processes"[2] and what Sargent Florence calls the "principle of multiples."[3] If a collection of indivisible productive resources is to be fully used, the minimum level of output at which the firm must produce must correspond to the least common multiple of the various maximum outputs obtainable from the smallest unit in which each type of resource can be acquired. The principle has usually, if not always, been applied only to machines, and even in this case Robinson has suggested that it may be necessary to plan production on a very large scale in order to use all machines at their most efficient level of operation. When, however, the full range of resources used in any firm is considered, including management, engineering, and even research personnel, as well as the minimum resources needed to sell the product, it is clear that this least common multiple may involve an enormously large output. This output will tend to be greater the larger the variety of resources and the more diverse the units in which they come. A firm would have to produce on a vast scale if it were to use fully the services of all the resources required for much smaller levels of output.

Obviously the output that will utilize most efficiently all of the firm's resources can only be reached if there is no limit short of this output on the supply of any one of them or on the market for the product. If

[2] E. A. G. Robinson, *The Structure of Competitive Industry* (Harcourt, Brace, 1932), pp. 31-33.

[3] P. Sargent Florence, *The Logic of Industrial Organisation* (London: Kegan Paul, 1933), pp. 18-20.

there is any fixed factor in the productive operations of a firm, it will only be by chance that the firm will be able so to organize its resources that all of them will be fully used. The firm will, to be sure, use the services for which its resources were acquired to the fullest extent consistent with the lowest-cost combination of these resources for the amount and kinds of product it has decided to produce, but there is no reason to expect that it would be possible to make them come out even, so to speak. If the limiting factor were removed, additional services from some of the resources already acquired by the firm in the course of its operations would be available at no extra cost to the firm.

In addition, of course, most productive resources, including labor and managerial personnel, are capable of being used in many different ways and for many different purposes. Hence a firm in acquiring resources for particular purposes—to render particular services—also acquires a range of potential productive services, most of which will remain unused. This multiple serviceability of resources often gives firms a flexibility in a changing and uncertain environment which may become of great importance in determining the direction of growth.

Limits on the Supply of Managerial Services. There is one type of productive service which, by its very nature, is available to a firm in only limited amounts. This is the service of personnel, in particular, of management, with experience within the firm. Even if all other resources, that is, factors of production as available in the market, including the best managerial personnel, were in perfectly elastic supply, executives with experience within any given firm can only be found within that firm. The question, therefore, arises whether what I might call internally experienced managerial services are a necessary input in the process of expansion. It would seem so. Both general reasoning and discussions with businessmen confirm the hypothesis that a firm would never have a high degree of confidence in any extensive plan for expansion drawn up and executed exclusively by men with no experience within the firm itself.

There is little doubt that experience in a given environment does increase the ability of individuals to deal effectively with that environment, to anticipate and provide for circumstances they might otherwise have overlooked, and, in particular, to use other men to better advantage. This does not mean that indefinitely large plans for expansion cannot be made by any group, no matter how small or inexperienced, but only that such plans are apt to be less workable or reliable according as the scope of the plans increases in relation to the number of people with the requisite experience. To the degree that internally experienced personnel are not available, the firm (that is, the existing executive group) is likely to have less confidence in its ability successfully to

execute an expansion program. Management consultants, industrial engineering consultants, and similar advisory groups undoubtedly can render very important services. They reduce the extent to which a firm's own managerial resources must be left free for expansion purposes and widen the range of problems the firm can successfully handle. Nevertheless, not only the execution of plans for expansion but also the examination and approval of recommendations (after all, it takes time even to read reports let alone to pass on them) require a great deal of time and effort of the firm's own management.

Thus the making of expansion plans in which a firm has the requisite degree of confidence requires services which can only be produced within the firm. The production of these services requires time, and this limits the scope of a firm's expansion plans at any given time, but permits continuous extension of these plans through time. There are, of course, a number of restrictions on what can be achieved within any given period of time. Operations must often proceed in a certain sequence and many services required in expansion must be internally produced. These restrictions must be taken into consideration when the firm makes its expansion plans including its estimates of the time it will take before they are fully executed. But even to discover the size and composition of an optimum output under given circumstances requires an input of managerial and entrepreneurial services, some of which can be supplied only by individuals who have had experience within the firm. It is this restriction on the horizon of the firm, so to speak, that is significant from the point of view of its growth.

Let us imagine a case, for example, in which a firm has a fixed amount of experienced entrepreneurial services available for planning expansion; that the use of these in conjunction with other services will enable the firm to create a plan in which it has absolute confidence as to the outcome; and that for any more extensive expansion it will be necessary to fall back on the use of less experienced services, with the consequence that the firm will have less confidence in the outcome of actions undertaken in accordance with the plans. This lack of confidence can be looked upon as causing increasing costs—costs that are neither short run nor long run in the traditional sense. They are not short run, because plant and equipment are considered variable. They are not long run, because the considerations causing increasing costs are associated only with a temporary, though unavoidable, limit on the availability of services required for planning purposes. In both this and the traditional approach, managerial and entrepreneurial services are a fixed factor but costs are increasing, not because of some indefinable complexity due to size beyond the power of human beings to resolve even when organizational techniques are fully adjusted, but because of the limited

supply of experienced personnel at the time the firm plans its expansion. So long as executive resources can be added to a firm's staff, the supply of internally experienced management can be increased in the course of time, but the number of new executives that can be absorbed by a given firm in a given period and placed in situations from which they can gain the requisite experience is also limited by existing managerial resources and by the existing scale of operations of the firm. The rate at which the latter can be increased is precisely the problem that we are exploring.

In view of this limit on a firm's ability to extend indefinitely its plans for future operations, it is almost inconceivable that these plans can always, or even usually, be of such a scope that all of the firm's resources will be fully utilized. In general, there will always be services capable of being used in the same or in different lines of production but which are not so used because the firm could not plan extensively enough to use them. Such services are wasted for the time being. The type of resources for which this waste of service is most significant will depend on the nature of each particular firm. For some firms it may be machinery; for other firms the waste of administrative ability, executive talent, or the specialized connections of the sales force will be more important; for still others the significant waste may be of know-how, research-acquired knowledge, and engineering ability. That these last two are not an unimportant type of available, though unused, service is readily seen in the history of the expansion of individual firms in the modern world. The particular services that remain unused need not be of the same type as those that are actually used in current production; unused services are sometimes those of products and materials, skills and ideas, which are by-products of the primary productive activities of the firm.

Unused services are in themselves a challenge and an incentive to the ambitious entrepreneur. They are the excess services of resources which have been acquired and are needed in current operations; they are available for further production at no extra cost to the firm. An increase in production of the existing products of the firm which uses these services would, other things being equal, enable the firm to produce at a lower average cost. Of course, if a firm overestimates this free element in costs and underestimates the costs of other services that will be required, particularly increases in overhead costs due to additional production, a mistake will be made. The danger of "creep"—imprudent expansion on the basis of existing unused services—has frequently been expounded by accountants and others. But this does not affect the validity of the argument presented here. (See, for example, Robert L. Dixon, "Creep," *Journal of Accountancy,* July, 1953, pages 48-55.)

If the services were used in the production of products new to the firm, a competitive advantage would obtain over firms not having this free or joint element in costs. In many cases, of course, the ability of a firm to use such services depends on increases in knowledge and improvements of technique. As a matter of fact, many inventions and technological advances can be directly traced to the desire of a firm to take advantage of already possessed services not used in current production. Sometimes the profitability of devoting available resources to the primary lines of production has been so great that exploitation of minor lines has for long periods been neglected. That all profitable opportunities for productive activity known and open to a firm are not exploited together need not be explained by limited supplies of capital or other resources. As the investigations of Heller have shown, limited managerial services are often stated by management itself to be the important restriction on expansion. (See Walter W. Heller, "The Anatomy of Business Decisions," *Harvard Business Review*, March, 1951, pages 95-103.)

The Release and Growth of Managerial Services. If the preceding argument be accepted and it be agreed that the supply of the services of experienced personnel will, other things being equal, limit the amount of expansion a firm can plan at any time, then it is evident that further expansion becomes possible if, after the expansion plans are executed, such services become available to an extent greater than is necessary to operate the expanded concern.

Any substantial expansion involves not only acquisition of new personnel but promotion and redistribution of the old. Not infrequently a new subdivision of managerial organization is effected and a further decentralization of managerial functions takes place. Such reorganization may be done all at once or gradually as the progressive execution of the expansion plans calls for it. But the point is that both the new men brought in and the existing personnel of the firm gain further experience. Insofar as the increase in the experience of the men concerned is necessary to the execution of the firm's plans and was taken into consideration when the plans were made, the newly available services will be absorbed in the process of creating and operating the expanded concern. But it is impossible that all such to-be-created services be anticipated and their utilization provided for, not only because they include the as yet unknown experience of the planners themselves, but also because the variety of the potential services is unpredictable and their creation extends into the indefinite future.

Whether in carrying out an expansion program all of the firm's personnel are fully used depends partly on the policy of the firm. Firms whose general policy is to devote some part of their executive personnel

to the planning of expansion will have this personnel available for new planning as soon as any given plan is completed. At the other extreme, a firm's position may be such that those who make the plans also execute them and are therefore unable to devote time to additional planning until current plans are fully in operation. This policy is likely to be adopted of necessity by small and new firms whose executive staff is not very large. Nevertheless, once expansion has been completed and the operation of the firm fully geared to the new level of activity, additional productive services will be both created and released to provide the basis for further expansion.

They will be created because all personnel of the firm will gain additional experience as time passes. They will be released because as the range of services the managerial staff can perform increases, the new services will not be fully absorbed in the existing managerial tasks. Not only is there likely to be a generalized improvement in skill and efficiency but also the development of new and specialized services. When men have become used to working in a particular firm or with a particular group of other men in a firm, they become individually and as a group more valuable to the firm because the range of services they can render is enhanced by their knowledge of their fellow workers, of the methods of the firm, of the best way of doing things in the particular set of circumstances in which they are working. Individuals taking over executive functions new to them—either because the firm has added new functions or because the men are in new jobs—will run into difficulties and make mistakes merely because of the relative unfamiliarity of their work. When these individuals become more familiar with their work and succeed in integrating themselves into the organization under their control, not only will the effort required of them be reduced, but the range of services they are capable of rendering will be increased through experience. Both processes leave a residue of certain types of unused services in the wake of an expansion program.

The unused services thus created do not ordinarily exist in the visible form of idle man-hours but in the concealed form of unused abilities. The more complete use of the services of any given individual is supposed to be made possible by the process of promotion. Just as machinery after a point becomes less valuable and is "down-graded," so managerial resources may become more valuable and be "promoted." That promotion does not take care of all the increase in services that becomes available is the common experience of many firms in periods when growth is slow. Pressure from younger executives for advancement sometimes even creates for the firm a problem of maintaining the morale of personnel and cannot be entirely explained by the hypothesis that individuals tend to overestimate their own ability.

So far, managerial services have been treated as though they formed a pool of services differentiated only by the experience of the people rendering them. We have begged the question of the type of managerial services required to enable a firm to plan effectively and operate efficiently on an ever increasing scale. If the problems of adapting to rapidly changing external conditions are ignored, the simplest function of management is that of co-ordinating everyday operating activity within a given administrative framework, coping with minor problems, and improving existing ways of doing the same things. Growth requires initiative and imagination and thus a type of service different from that of the routine manager. The simplest type of initiating service is called for when expansion takes the form of increasing production of the products already being produced. Here management need only use on a wider scale the specialized knowledge it has already acquired. The managerial services available may be highly specific to the firm's existing products, but this makes it all the easier to expand as unused services develop, provided that additional goods can be sold at profitable prices. The analysis of this type of expansion falls within the framework of the conventional theory of the firm. The firm will expand the output of its products up to the point at which the increment in total costs equals the increment in total revenue, which, in the absence of diseconomies of scale, will be set by the well-known limitations on demand. This is familiar analysis and need not be elaborated.

The Utilization of Productive Services and Limits on Demand. The emphasis on demand as a restriction on the output of a firm arises because in the traditional theory of the firm, the firm is usually defined with reference to a given product. The theory of the firm serves primarily as a foundation for a general theory of price and output and hence is concerned with products rather than with the analysis of the firm as a many-sided economic institution. Once it is recognized that, in principle, the market of a firm in the wider sense is restricted only if the kinds of product it can produce are for some reasons limited, then it becomes clear that the flexibility and versatility of its own resources are the important factors governing the possibilities of its expansion. So long as there are profitable production opportunities open anywhere in the economy, a firm can take advantage of them if its resources are versatile—in particular, if its management is imaginative, flexible, and ambitious.

There are many examples of firms with vigorous and creative management who have substantially altered their range of products, sometimes completely abandoning their original lines and expanding their total output in spite of unfavorable demand conditions for their original products. There are, of course, many examples of other firms which

were not able to make the required adjustments. In such cases, failure to grow is often incorrectly attributed to demand conditions rather than to the limited nature of entrepreneurial resources.

There is not time here to examine this problem in more detail. Insofar as a firm is organized to produce only a given range of products, any limitations on the market for those products will prevent the firm from expanding the production of them. This is likely to mean that the firm will be unable to use fully its productive resources. A versatile type of executive service is needed if expansion requires major efforts on the part of the firm to develop new markets or entails branching out into new lines of production. Here the imaginative effort—the sense of timing, the instinctive recognition of what will catch on or how to make it catch on—becomes of overwhelming importance. These services are not likely to be equally available to all firms. For those that have them, however, a wider range of investment opportunities lies open than to firms with a less versatile type of management.

The most effective restriction on the versatility of executive services is that which stems from a lack of interest in experimenting or a lack of confidence in dealing with new and alien lines of activity. I say that this is the most effective restriction because mere specialization of managerial knowledge and ability is not itself a serious bar to a firm's branching out into new lines of activity if existing executives are sufficiently interested and confident to bring into the firm people possessing other relevant knowledge and ability. But it often happens that the horizon of management is extremely limited, particularly in smaller firms. Content with doing a good job in his own field, the small entrepreneur may never even consider the wider possibilities that would lie within his reach if only he raised his head to see them. If occasionally he gets a glimpse of them, he may lack the daring or the ambition to reach for them, although he may be an ambitious, efficient, and successful producer in his chosen field. Specificity of managerial resources means that some of the managerial services most essential for expansion are not available to the firm even though other managerial services essential for efficient operation in a particular field are fully available.

It is not helpful to dismiss this lack of vision, of confidence, or of experimenting ambition as an example of a failure to attempt to maximize profits, for they are intimately connected with the nature of managerial ability itself and must be reckoned as a limitation on the supply of specific types of productive services. After all, even a preliminary investigation into the possible profitability of any particular type of expansion presupposes a prior decision as to the allocation of managerial services between normal operations and the extension of these operations—a decision which, by its very nature, cannot in the

first instance be the result of a careful analysis of anticipated costs and revenues, since before such an analysis is undertaken the decision that it should be undertaken must be made. Whether such a decision is made at all is often a matter of temperament.

III. *Are There Limits to the Size of Firms?*

The managerial limits to growth discussed above are rather different from the type of limit on the supply of managerial services that is implicit in the traditional theory of the firm. There it is assumed both that the factors of production are in perfectly elastic supply and that the long-run average costs will at some point start rising—a situation which fundamentally can only be due to the quality of managerial services. The chief difference lies in the fact that imagination and ideas are not confined by time and space while the administrative activities of a single individual must be so confined. There is no known limit to the former. But there is to the latter, and from this it has been deduced that there must be a size of firm beyond which the services required for its efficient operation simply cannot be supplied by mortal men. This deduction is frequently stated as if it were an empirically established fact. The complexity of structure and the scope of activity are believed to become such that even the minimum decisions required of the chief executives in order to ensure the requisite degree of co-ordination are so difficult or so numerous that the firm must suffer in efficiency. Costs of production would therefore be greater in very large firms than in smaller firms—the so-called "diseconomies of scale" would overshadow all economies.

The planning of appropriate changes in the administrative organization of a firm is part of the planning of an expansion program. To some extent the kind of services necessary to effect this type of reorganican be hired. Just as techniques of production appropriate to large-scale operations are known and can be adopted from the outside, as it were, so known techniques of administration can be learned from the outside. Changing organization as a firm grows is very largely a progressive decentralization of authority. By its very nature, decentralization of this type cannot precede to any significant extent the expansion of the firm but must develop more or less simultaneously with it. Some firms may be incapable of effecting the appropriate administrative reorganization—for example, the existing executives may be unwilling to relinquish any of their personal control. These firms will find themselves unable to operate efficiently on a larger scale and will run into trouble as they attempt to expand. While some executives may be insufficiently used, others will be overworked, presenting a truly fixed factor, and diminishing returns will proceed with a vengeance.

We do not know how effective the decentralization of authority can be as a means of keeping costs per unit of output from rising as a firm expands. Reliable empirical evidence does not exist and all studies of the matter are inconclusive, but there is no evidence that a large decentralized concern requires supermen to run it. It does require more men who are competent to make high-level business decisions and who have had experience with the particular kinds of problem with which the firm has to deal. Neither is there significant evidence that the ability to fill the higher administrative positions is excessively rare or that the demands on the men occupying these positions exceed their ability to cope with them effectively.

As to the problem of co-ordination, a strong case can be made for the proposition that as a firm increases in size and its various parts obtain a greater and greater degree of autonomy, the real issue is not whether the whole thing becomes unwieldy but whether it should properly be called a single firm in any economic sense. For many analytical purposes, we must treat some of the parts of the firm as having an independent existence, while for other purposes we must recognize the nature of their interdependence in the administrative structure. As administrative co-ordination becomes more tenuous with increasing size, efficiency becomes less a matter of expert co-ordination and more a matter of the efficient operation of virtually independent parts. It is common knowledge that one department of a single firm may be efficient while other departments are inefficient. Some department heads are good and some are bad. I suppose that if an inadequate manager is placed in charge of a department, this is, by definition, poor co-ordination on the part of some central authority. But it need not be looked on in this way. There are plenty of poor managers in the business world—in responsible positions, too—and the large firms undoubtedly get some of them.

Co-ordination is relevant to efficiency only insofar as co-ordination is required and is attempted. That central control can be dispensed with over wide areas has surely been amply demonstrated by many large firms in the present-day economy, and that the end is not yet in sight is generally admitted. Neither the conception of a fixed factor nor the analysis of the diseconomies of scale is relevant in these circumstances. In the process of growth the "organism" radically changes its form; the firm becomes less "organic" and, indeed, less of a "firm" in the pristine economic sense.

This suggests an interesting paradox. The growth of firms may be consistent with the most efficient use of society's resources; the result of a past growth—the size attained at any time—may have no corresponding advantages. Each successive increment of growth may be

profitable to the firm and, if it uses otherwise underutilized resources, advantageous to society. *Ex ante,* the economist interested in resource allocation should approve. But once any increment of expansion is completed, the original justification for the expansion may fade into insignificance as new opportunities for growth develop and are acted upon. In this case, it would not follow that the large firm as a whole was any more efficient than its several parts would be if they were operating (and growing) quite independently. *Ex post,* the economist might disapprove.

MANAGERIAL MOTIVATION AND THE THEORY OF THE FIRM

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This paper is essentially a report on a research project undertaken in collaboration with Dr. Manford Kuhn, social psychologist at the State University of Iowa. His part in the formulation of the conceptual apparatus abstracted herein and in the development of appropriate research techniques has been a major one. This project is now also being carried forth independently, as the testing and application get under way.

The paper will include: (1) a statement of the importance of the motivational assumption, especially to the theory of the firm; (2) a consideration of certain methological issues involved in examining such assumptions; (3) a short résumé of recent modifications of the assumed motivation; (4) a capsule description of "self" or "reference group" theory, as a promising general orientation from which to examine motivation; (5) a reformulation of motivation in terms of this theory; (6) a partial structuring in such terms of existing knowledge of the manager and his role; (7) suggested lines of research; and (8) possible research techniques.

Motivation has long been important, if often implicit, in economic analysis. In our particular economic system, featuring the business firm, managerial motivation is of special importance. Yet, few attempts have been made to systematically study managerial motivation, to isolate the factors that may influence and condition such motivation, and to bring economic motivation into the orbit of motivation generally.

The question of managerial motivation has thus far been handled primarily by establishing simple assumptions as crude first approximations. Thus most economic theory (especially the theory of the firm), the bulk of applied economic analysis, and much public policy assume profit maximization as the paramount goal of business and businessmen.

Establishing managerial motivation by assumption rather than by empirical means has become somewhat difficult to justify. Evidence of a variety of managerial goals continues to accumulate; our changing social and political institutions and attitudes have carried obvious motivational implications; and the economic theory and public policy predicated upon a single simple motivation have recently been subject to critical re-examination. Meanwhile, the concepts and researches

of social psychology, sociology, cultural anthropology, clinical pyschology, and psychiatry have made available new findings, approaches, and techniques relative to motivation.

Why has systematic study of managerial motivation been largely lacking, at least until very recently? One partial explanation has been reluctance on methodological grounds. Profit maximization is an assumption which is useful in constructing a model. To study this or any other assumed motivation, it is suggested, may involve a misunderstanding of the methodology of model building.

The realism or accuracy of the initial assumptions of the model is not, it is argued, what matters. It is the ultimate usefulness of the model that counts. If the model is not useful, then it is time to go back and tinker with the assumptions if need be, or to build another model. If the model is useful, however, then the assumptions matter little.

Assumptions, in short, should mostly not get in the way. They should be simple and clean cut in order to enable us to build models that are internally consistent and that give determinate results. A simple, unrealistic assumption might prove to be better and more useful than a realistic but complex or variable or ambiguous assumption. Thus many economists deny that recent modifications of the assumed motivation of profit maximization are necessarily a step forward.

One of the more explicit statements of the methodological case for such simple assumptions as profit maximization is found in a recent book, *A Textbook of Economic Theory,* by Alfred W. Stonier and Douglas C. Hague. They defend the assumption in these terms (pages 87-88):

> "Rationality" in the theory of the firm implies that the individual producer aims at earning the greatest possible money profits. Now, no economist believes that all business men do, in fact, always maximise profits, nor does he necessarily believe that they should do so. But every economist wants his analysis of the individual firm to enable him to point to a single position where, in given conditions, the firm will be in equilibrium. It is therefore essential for him to make some assumption of this kind. If no such assumption were made, there would be no single equilibrium for the firm in any given circumstances. It would be very difficult to produce any definite explanation of the way firms fix output and prices at all.

Later, Stonier and Hague also assert (page 499) that "the assumption of 'rationality' amongst business men seems a most reasonable one to make until empirical research provides a better hypothesis; and so far it has failed to provide one."

If such positions reduce to this assertion that profit maximization is the most plausible assumption yet available, one may register reservations or tentative disbelief, but the burden of proof is admittedly on the critic. If this position means that if two assumptions are equally plausible or if the relative plausibility is not known, the simpler assumption is to be preferred, again no serious quarrel can be sustained,

provided, of course, that the greater simplicity of the one does not discourage anyone from investigating relative plausibility.

Perhaps the most disturbing undertone in the Stonier-Hague argument is the suggestion that simplicity is a prime value. The difficulty here goes beyond the fact that determinativeness alone may not always serve predictive ends. There also seems to be a good deal of prejudging as to the degree of simplicity which may be found at the end of a trail of research.

Despite such methodological concerns, there has been a tremendous amount of effort devoted since World War II to the modification of the previously assumed motivation of the manager. Economic theorists, students of business organization and management, businessmen and their spokesmen, and a host of others have either tinkered with profit maximization or have proposed additional or alternative motives. Out of this welter of opinions certain tentative conclusions emerge: (1) There is still considerable support, among both scholars and businessmen, for the primacy of the profit motive. (2) There seems to be an increasing tendency for this motive to be translated into something longer run, broader, and less precisely defined than the classic concept. (3) Other motivations of an economic character, such as preservation of the firm or maintenance of the size and position of the firm, are held to be of major importance. (4) The so-called "noneconomic" motivations are now taken much more seriously, despite the evident difficulties involved in measurement. (5) The complexity and variability of motivation have been recognized and some hypotheses have been suggested.

Thus many have groped toward a reappraisal of managerial motivation and in the process have moved a long way from the traditional concept of profit maximization. Most of these analyses continue, however, to treat the manager as an autonomous, isolated individual rather than as a member of groups and a participant in a culture. There has also been little attempt to apply what is known about motivation per se or motivation in other sectors of human behavior to the economic realm. There has also been little or no interest in the use of data relevant to managerial motivation in the construction of a general theory of motivation.

There now appears to be available an approach which might allow the fruitful re-examination of managerial motivation. From what was long termed the symbolic interactional approach has come what is now increasingly referred to as "reference group" or "self" theory. A capsule description of this approach, with a few of the attendant definitions which are of special interest to us, might now be in order. We may conveniently divide this brief résumé of self-theory into two general parts: a consideration of the nature of the world of man's

experience and a consideration of the nature of the person himself.

For the self-theorist, human beings do not live in a universe the events and objects of which have intrinsic meaning for and bearing upon human experience and behavior. They live rather in a universe mediated to us through social definitions couched in language. The stimulus-response-oriented researcher, in one of those rare occasions in which he attempts to use human subjects, characteristically puts his "subject" in an experimental room which he describes as a "neutral situation" and then subjects him to a predetermined "stimulus." The self-theory-oriented researcher, adhering to the idea implicit in "four walls do not a prison make," takes the view that he does not know whether plain gray walls make a neutral situation unless he has the relevant attitudes of the participants. Nor, for that matter, does he know the objects which he is going to vary without getting the relevant definitions from his participants.

No objects or events have "intrinsic stimulus value" for human beings. Those concatenations of events which we think of as objects have become objects as a result of the structuring of language. We experience them and behave toward them not in terms of any essential nature they may have but in terms of their meanings which they have as a result of language.

To understand the nature of the person, understanding of status and role and how they are acquired is necessary. When a child is born into some specific family, he is also given a specific status and assigned a specific role. Statuses are the building blocks—the smallest units of which any social group is made. They are the various positions, quite apart from the individuals who occupy them, which constitute the structure of a group. Every status, thus considered as a position in a group, has inseparably bound up with it a function to be performed. We refer to this function as a role. A role, like its correlative status, is defined quite apart from the person who is performing it. A role is a prescription, a recipe, a socially expected plan of action. It is an accepted and sanctioned proper-thing-to-do, together with the proper-way-to-do-it.

Each status and role derives its significance from the total configuration of statuses and roles of which it is a constituent part. No status, no role means anything by itself. An individual learns to occupy a status and to play a role by learning not only the rights and duties of his status and the requisite behaviors of his role but also those of all the other persons in the group or social system of which this status and role are a part. He must not only behave in such a way that the expectations of others will be met, but he must also be able, reciprocally, to anticipate correctly their behaviors.

We must distinguish, at this point, between those groups of which we are officially and formally members (called "membership groups") and those groups with which we feel self-identified (termed "reference groups"). It is the latter—the reference groups—which contain our "significant others" whose members' judgments we take into account and by which we identify ourselves as social objects. We may be, for example, a Baptist, a father, and a civil engineer. Each of these as a reference group provides us with relevant norms which we have taken as our own to be applied in certain kinds of situations.

The crucial significance of the self as a social object is that it is the only common object in all of the widely varied situations in which we participate. As such, it comes to serve as the anchoring point from which to make judgments and subsequent plans of action toward the many other objects in each specific situation. There is nothing mystical about this self. It consists of the attitudes (plans of action) toward the self as an object. We may think of it as consisting of all the relevant answers the individual might make to the question, as he would apply it to himself, "Who am I?" There is no way to answer such a question except by referring to oneself as a member of the groups that he feels identify him; or by referring to social categories which his reference groups hold to be significant in identifying him; or, finally, by referring to evaluations of himself which hinge on norms held by his reference groups. In any of these events, his answers are attitudinal statements which are the best indices of what he will do in any situation.

In terms of such a general orientation, how does one approach motivation? Self-theory would first lead one to reject the proposition that motivation is either individual or biological. The idea that motivation is individual has led us to focus attention on the solitary person, at the expense of a consideration of group norms and expectations. The idea that motivation is biological has resulted in attention to neural and glandular matters to the veritable exclusion of symbols and communication. The two ideas combined have led us to attend to ontogenetic sequences with individuals as units rather than to uniformities within groups and differences among groups. Our inability to discover much has been obscured by our failure to realize the circularity which characterizes explanations which have been made by constructs such as "drives," "survival," and "individual differences."

What, then, do we mean by motivation? Because it commonly points to the goal toward which behavior is directed, what ever else it may include, we may define it as the purposeful organization and direction of behavior. Inasmuch as goals are not implicit in the organism but are acquired through social participation, the word "purposeful" is

intended to put our use of the term motivation entirely on the social level of analysis.

Roles are the motives of men. Considered from the standpoint of groups, roles are the recipes for behavior. They are the symbolic statements of what ought to be paid attention to, what kind of action ought to be taken, with whom, toward what objectives. Considered from the standpoint of the individuals who play them, roles—once adopted—are the very definitions of the persons themselves. John Smith, President of X Corporation, does not define himself as "one who plays the role of president of X Corporation." He *is* President of X Corporation. This may not be all that he is, but the other things which he is are of the same order. The role is antecedent to the individual who adopts it, who by adopting it both defines himself as an object in terms of it and takes over the socially approved plans of action which it comprises as his own. They are then his motives.

In order to define a role, we must specify what attitudes constitute it. Attitudes are the plans of action toward the objects which are significant to the role being played. Their counterparts in the structure of the group are the norms which serve as the social definitions of situations and of roles. The role, then, is the over-all plan of action of which the attitudes are the constituent parts. The role as the role-player himself conceives it is the large design of motive, but one must not conclude that it is simply a sum of the subsumed attitudes, for any one of them makes sense only in terms of the role as a whole.

The term "attitude" is commonly used by social psychologists to mean a persistent tendency to act in some specific way. We accept this definition, provided it be understood that the term refers to events at the symbolic (social) level rather than to events at the physiological level. By attitudes, we mean verbal statements which constitute blueprints for behavior. These statements indicate the ends toward which action is directed, the justifications for holding these ends or values, the proper methods of achieving the ends, and the proper feeling and evaluation regarding degrees of success or failure in achieving the ends.

Motivations differ, of course, not only between groups but within groups. In any given group we do not find a standard motivation for two reasons. Each member belongs to a number of other groups which may have different or even conflicting values; and the group includes a number of different kinds of roles. Yet the group's very existence hinges on the common acceptance of a minimal common core of values for every member; so to establish the fact that an individual identifies himself with a particular group is sufficient to enable prediction of his motivation for a considerable range of behaviors.

The aim of science is to establish the most economical ways of describing the regularities of behavior. It may appear that we have here discarded earlier theories of dependable regularity of motivation (e.g., profit maximization, biological drives) without offering in their place any economical method for establishing regularities in the tremendous variety of motives described by social roles. It is true that the methods needed are only in process of being formulated and tested. Yet enough has been done already to establish the possibility of determining with what reference groups individuals feel identified and of finding relatively high correlations between these identifications and significant attitudes toward a wide variety of situations.

If one accepts this general frame of reference, then the intensive study of the role of the manager is the clearly desirable avenue toward the delineation of his motives. We shall now merely suggest some of the types of information presently available which bear on the role of the manager and on the manager's conception of that role. Any sketch of the social role of the corporate executive must start well beyond the corporation itself. Our first concern would be with the larger socioeconomic-political environment within which the contemporary firm and its leadership must operate. This familiar environment features not only such visible elements as big government, big labor, and other competing power blocs, but also an extensive and influential body of attitudes. These forces and these attitudes furnish many of the limits, constraints, and inducements which condition business motivation and behavior.

The manager must then be examined within the setting of the firm itself—often a corporation in which the stockholders have a rather nominal control and in which an apparently autonomous management appears to largely run the show. The preoccupation of the executive with his corporation as a sort of independent entity, whose welfare is to be maintained and protected, is powerfully reinforced by the fact that he is a member of a group interested in the same things. Modern management—especially in the large corporation—operates in a group setting, with group decisions—and group attitudes, constraints, and aspirations—although individual roles may be sharply differentiated. Members of management play a distinct role in the corporate scheme of things, and within the confines of that general role there are dozens or hundreds of specific roles. Each man attempts to fulfill his role, and if he is blocked in his attempt to play this role, he is frustrated and he often resists, although he may ultimately accommodate.

In his role playing, he is also concerned with his groups "outside" the corporation—in the community, neighborhood, club, and home.

There is, of course, some carry-over of his managerial role. William Henry notes (in *American Journal of Sociology,* January, 1949):

> In both its economic and its social aspects the role of the business executive is sociologically a highly visible one. It has clearly definable limits and characteristics known to the general public.

This role comes to have compulsive power as far as the manager is concerned. Chester I. Barnard has observed (as reported by R. Richard Wohl in *Change and the Entrepreneur,* pages 9-10):

> Often enough, an individual will be confronted with an offer to take on a particular job. He may think this job distasteful or hazardous. He may not wish to take it on. He is not overtly compelled to do so; he is invited to accept or reject the tender. But he accepts it. For should he refuse to accept it, he would lose caste with his associates, a consideration which rules out the apparent discretion he supposedly enjoys. He takes the task, and works at it, because part of his "job"—his role—is to deal with it. . . .

The role played and the personality of the man playing it are not unconnected. Henry further comments:

> It is not a game that the person is playing; it is the way of behaving and thinking that he knows best, that he finds rewarding, and in which he believes. Thus, the role as socially defined has its counterpart in personality structure. To some extent, too, the personality structure is reshaped to be in harmony with the social role.

He adds, however, that the necessity for such reshaping is minimized by the initial selection process. Henry has undertaken an intensive analysis of the personalities of a group of over one hundred successful business executives in various types of concerns. He has found eleven personality characteristics shared by this group. He has also concluded:

> The successful executive represents a crystallization of many of the attitudes and values generally accepted by middle-class American society. The value of accumulation and achievement, of self-directedness and independent thought and their rewards in prestige and status and property, are found in this group. But they also pay the price of holding these values and of profiting from them.

This analysis suggests, of course, that research has scarcely begun. It may also give some clue, however, as to lines of research that hold promise and which might now be explored if the orientation proposed herein is to be tested or applied.

It will be necessary to identify the values and norms of the larger American society in which the manager lives and functions, presumably by such empirical means as content analysis and attitude measurement of these values. It would then be possible to compare and contrast these over-all values with those values which have been held by managers alone, probably as the result of their special reference-group identification. An inquiry could then be conducted into the differential types of associations holding these special "motivational vocabularies" which mark the manager off from the other members of his society.

It would be important also to determine how managers define the over-all society and how they see themselves in relation to it. How are

the groups and categories of our society defined as social objects by managers? What channels of communication, mass or otherwise, are used by managers and what definitions and values do these channels purvey? How differently do those who play other recognized roles in society define that society?

Another crucial research tack would have to do with motive blockage. What happens when the group-held values, such as that of profit maximization, are perceived by managers to be blocked by action of government or labor? Under what conditions is there conflict, under what circumstances is communication cut off, and when is there accommodate change in values and role?

A number of studies along historical lines are also needed. Just how has the role of the manager changed during the past several decades? Specifically, what changes have occurred in the manifest and latent functions of the corporation to the larger society, in the system of roles constituting the structure of the corporation, in managerial perceptions of and attitudes toward these changes, in the vocabulary of motives used by corporation managers, and so on?

Finally, it would be important to examine the whole range of groups with which managers feel identified. If, for example, they do feel identified with their communities, social cliques, and families, it would be useful to explore to what extent the norms from these groups converge to a single value-system including that derived from the managerial role itself and to what extent they conflict with each other. If they do conflict, how are they handled? Do they become compartmentalized, in accordance with norms supplied with the managerial role itself, or do they complicate his motives as manager in some regular and describable ways?

A final question remains. Are there effective research techniques available with which this approach can be tested and applied? A theory without research techniques is a mere set of circularities—an attempt to explain by naming. The theory which we have set forth in this paper is well over half a century old, but only within the past decade have research techniques appropriate to it been employed. Fortunately, however, the major difficulty has been in coming to some agreement about the class of phenomena which is to be investigated. Now that there is a growing agreement that the self is a set of attitudes, we are able to bring to bear the techniques of attitude measurement which have been developed over the past quarter to the point where Newcomb can speak of measurement of attitudes as the greatest technical achievement of social psychology. The techniques we are able to employ in the identification and measurement of self-attitudes are thus ones which are already considerably refined and rather rigorous.

Empirical treatment of self-attitudes, nonetheless, is in its infancy. Although the application of attitude-testing techniques is now possible, there remain some complicated problems besetting the researcher into self-attitudes. First, it is very difficult to know what kinds of items to construct for inclusion. Second, there is the very great danger of suggesting responses to the respondent. Third, there is the possibility that the self-attitudes which are elicited will be those which are relevant only to highly limited situations.

A self-attitude test has been constructed which meets all of these difficulties. It consists of asking the respondent to give, as if to himself, twenty different statements in answer to the question, "Who am I?" In asking this pertinent but very inclusive question in this way and by requesting multiple responses from each respondent, the formulation of relevant items is left up to the respondent. Thus the first problem is solved. We may assume that when the respondent formulates the items himself they must be relevant, at least to him. By asking such an inclusive question as "Who am I?" and by asking the respondent to direct it at himself and to answer as if to himself, we minimize the possibilities of suggestion to the vanishing point. Finally, by phrasing the question in this extremely general form we minimize the risk of cueing in the specific situation under which the test is being administered.

This test—termed the "Twenty-Statements Test" by its authors, Manford Kuhn and Thomas McPartland (see *American Sociological Review*, February, 1954)—is what is known as an "open-ended" test, which requires for any kind of systematic treatment of results some kind of content analysis of the replies. The most useful content analysis which has been made of replies to this test is the sorting of statements into those which are "consensual" and those which are "subconsensual." A consenual statement is one which requires no further explanation in order to be understood by the analyst or anyone else. A subconsensual statement is one which refers to norms which vary and which must be inquired into if the statement is to be at all denotative. Significantly, it turns out that the consensual statements, in overwhelming proportion, refer to the respondents' statuses in social categories and groups. It further emerges that respondents, if they have any consensual statements at all to give, give them first.

We earlier made the proposal that it is necessary to know how managers define themselves in order to find out the regularities in their motivations. We have now suggested a probable line of attack. We must ask what we want to know. If this sounds simple-minded, we must remind ourselves that the habit of asking some other, perhaps quite irrelevant, question (e.g., "What do you see in this ink blot?")

is fairly endemic in our society today. We have come as a society quite prematurely to the conclusion that man is irrational, and that there is no use asking people who they are and what they are doing or what is important to them. We have here suggested some direct questions which have demonstrated initial promise and for which there are some fairly rigorous techniques for manipulation already in existence.

We have advanced in this paper the suggestion that self- or reference-group theory may provide a useful approach to managerial motivation, and that it has effective associated research tools. If this claim proves by inductive test to be valid, as we hope that it will, then it will have demonstrated that the regularities of human behavior are such as to support the proposition, not that man is irrational, but that he is rational in a broader, more inclusive sense than previous models have been able to describe.

DISCUSSION

ALBERT LAUTERBACH: By agreement with the chairman, I should like to use most of the time available to report briefly on research which I carried out last summer, with the aid of the Social Science Research Council, on managerial attitudes in Great Britain, Norway, Sweden, and Western Germany. All of these countries are characterized by the coexistence of private, public, and co-operative enterprise, in varying forms and proportions. It was the specific aim of my study to explore the perception by managers of the managerial function as such, managerial incentives and success, division of responsibilities and risk-taking, the qualities desired in executives, the leadership role of managers, and the impact on management of national and cultural differences.

My project was meant to be a pilot study, and no quantifiable results were aimed at. Those interviewed, however, included a cross-section of managers in large private corporations and medium-sized family enterprises in a variety of industries, and managers of publicly and co-operatively owned enterprises; also union officers and shop stewards and research workers and public servants in related fields. The managerial sample included many persons who had had executive experience both in private and in public or co-operative enterprises.

The appointments were based on suitable contacts and introductions, and the interviews were semistructured—the prepared questionnaire was used in an informal and flexible way. The total number of interviews was 134, including 75 managers, 19 union officers and shop stewards, and 40 public servants and researchers. The results obtained should be interpreted as raw material in formulating pertinent questions, in throwing some light on the multitude of variables involved, and in posing some promising hypotheses for future testing. With this qualification, the results can be summarized as follows:

1. The ownership situation carries far less weight in determining the basic attitudes of managers in most kinds of enterprises, especially the large ones, than has traditionally been assumed by supporters of capitalism and socialism alike. Ownership, of course, remains of considerable importance in its legal and financial aspects, but for managerial attitudes other variables—such as size and organization—appear to be of equal or greater impact. This was emphasized by many of the managers themselves as well as members of other groups, including labor. I was especially impressed by the remarkable success of certain enterprises in Germany which have long been ownerless; they have been managed effectively by able men without anyone really knowing for years to whom these enterprises were going to belong eventually—to private owners, to the employees, to the state, or to foreign interests. The personnel director of one of these enterprises—a very large one—commented as follows: "We have assumed a certain basic capital for the balance-sheet, in a fictitious way; but there really are no stocks. Legally, it is an absurd situation. But we want to perform as if the plant belonged to us, that is, to both management and the employees."

2. The personal qualities of a manager are widely considered the most important element both in his attitudes and in his performance. Technical competence is taken for granted as a requirement in specific jobs, but for top management it is often rated less important than organizational gifts, vision, and a strong personality in general. For the rest, the personal qualities required in managers are frequently summarized as "character," meaning as a rule the ability to get on with people, to participate in teamwork, and to supply leadership without authoritarian traits. The significance of personality variations in managers was stressed especially by labor representatives in Sweden and Germany. Persons with the managerial qualities mentioned are widely judged as capable of performing well in any ownership form of comparable enterprises—private, public, or co-operative. Actually I found interchange of managers between these ownership forms to be fairly frequent, especially in Britain and Sweden.

3. Nonbureaucratic and competitive forms of management organization are almost universally regarded as essential if the best personal qualities of managers are to work out in practice, regardless of how the enterprise is owned. In particular, a sharp distinction is often made between those state enterprises which are subject to political controls and those which are organized as independent corporate units. Comparable yardsticks are applied to the organization of large private corporations. In Norway, where both bureaucratic and nonbureaucratic forms of state enterprise are represented, the sharp distinction mentioned is usually drawn by private managers as well as other people. For that matter, the head of an autonomous state plant in Norway pointed out that it was just as essential for him as for his private competitors "to have *time* to use commonsense"; that is, to enjoy both independence and relative freedom from routine duties.

4. Private management attitudes toward the government, not unexpectedly, are generally characterized by a desire to keep intervention down—a desire which is often shared by managers of co-operative enterprises and sometimes by officers of labor unions, too, even where their influence upon the government is strong. (The *functional* differences involved were stressed frequently by spokesmen for the Swedish co-operatives, labor, and government, which all favor the same ideological creed.)

The strong emotional, if not fanatic, undertone, however, which is so frequent in American business attitudes on the question of public intervention, is usually missing in the European countries studied. Certain areas of government intervention, even state ownership, are widely regarded as legitimate by private managers. The whole issue of government and business is discussed in Britain and Scandinavia on pragmatic grounds in each case, not as a matter of dogma or a quasi-moral issue of absolute character. The chairman of a large British concern told me that as a Conservative he would have put the nationalization of coal into his program, though it might have been suspect coming from a Conservative. He also approved of the nationalization of electricity and gas, as a device against a "parochial outlook," and he would add water supply. On the other hand, he was emphatically opposed to nationalization or public supervision in his own industry and

most others: "If we were open to investigation of all the silly things we do, we'd never get anywhere. We need authority to make mistakes."

Conversely, a socialist leader in Norway emphasized that his party no longer advocates nationalization or even intervention for its own sake, that they judge each case on its merits, that intervention should be confined to fields where private business fails or is inapplicable (because of war effects or large capital needs, for example), and that even there the trend leads away from centralized planning ("try to regulate the climate; don't try to count the raindrops"). Such attitudes are now fairly frequent in both ideological camps.

5. Delegation and decentralization of responsibilities is widely considered conducive to initiative and risk-taking on the part of individual executives; but top managers tend to emphasize that final decisions are unmistakably reserved for their own uncontestable authority. This again applies to all ownership types of enterprise, with some variations in emphasis or hopefulness. I heard a leading German banker give the following reason in explaining why he had quit the presidency of an important public enterprise: "I like to be with an institution where I can refuse a credit application without having to give a reason." Many public and co-operative managers in each country were no less emphatic in stressing the importance of individual responsibility, decentralization, and delegation of managerial authority. For example, managers of the public electricity and gas boards in Britain were practically unanimous in praising the decentralization in their industries compared with the older, centralized pattern in the nationalized coal industry, while executives of the latter were somewhat defensive on this point.

6. The managerial function as such is, of course, in part associated with the achievement of a profit or surplus for the enterprise; but it is widely perceived as exceeding this goal in many respects, even when the goal is qualified by long-range considerations. Additional elements of the managerial function are seen in striving for a firm financial basis and public standing, assurance of stable employment, up-to-date equipment and possible expansion in the interest of coming generations of managers and owners, and the performance of a public service of one kind or another. Rationalizations undoubtedly occur but the frequency of a broad conception of the managerial function, again with rather limited variation according to ownership, is certainly interesting. A somewhat extreme case was presented by the head of a medium-sized family enterprise in Norway, who is a supporter of Moral Rearmament and sees the real function of management in "helping to make people happy," by creating mutual confidence among all the groups in industry. Thus far, it is true, he has done little about it on the practical level.

The chairman of a large and profitable company in Britain commented as follows: "You have to give people a field in which they feel they can perform; satisfaction comes from *performance*, from fulfilling oneself (assuming the income is adequate). Remuneration *is* weighed in industry—as something to get out of your way as a problem, in order to do the things you are really interested in."

7. Managerial job satisfaction, accordingly, is related only in part to financial achievement in the sense of either profit or individual income. Or,

rather, profit is widely considered the indispensable starting point of managerial job satisfaction, not its true essence. The latter is mostly seen in doing the job itself; that is, in being active, getting things done, fulfilling an assignment, justifying the trust of associates and employees, building an up-to-date and durable structure, being considered successful by others, being respected, doing something useful, meeting a challenge.

The divisional manager in a British nationalized industry introduced himself with the words, "I am a capitalist," and wound up the lengthy interview with the words, "I am a violent antisocialist." But in between, he explained elaborately why he had quit his prosperous business in the same industry, why he had accepted the government job (after first heading a committee against nationalization), and why he opposed current schemes of denationalization as impairing efficiency (interestingly, he slipped once in referring to his government board as "my company"). His reason for accepting and carrying out the assignment, despite hostility to nationalization in general, was that he welcomed the opportunity to prove that he could handle a very big job. He was now proud of his financial and organizational achievement and enjoyed thoroughly the importance of his job. Moreover, he loved this particular industry which, he said, was "in his blood," and was going to remain loyal to the job still to be done in it, regardless of ownership.

Another element of managerial satisfaction was exemplified by the head of a medium-sized family enterprise in the British Midlands, who emphasized that aside from the balance sheet, the reputation of the company in the town mattered very much: "We all belong here. We were brought up that way."

8. Human relations of a desirable type rank high in the scale of managerial satisfactions. Distinct traces of paternalist attitudes can still be found, especially in older managers and in family-type enterprises. In fact, the family background or history of certain enterprises—including some very large ones—is often considered an asset. At the same time, the importance of good communication of the give-and-take type is now widely recognized. Various types of joint consultative committees are being tested in all of the countries mentioned. On the other hand, direct participation of employee representatives in managerial decision making has thus far been attempted only in the German steel and coal industries and is quite controversial there at this point.

The director of a German employers' association felt that "every employer has the shop council he deserves" and that there is something to be said for "the workers sharing the employer's worries." However, he also felt that the codetermination scheme went much too far in encroaching on property rights, in bringing alien elements into each enterprise, and in confusing the accustomed dividing lines between unions and management. This last point was virtually confirmed by some labor representatives, too. However, the *Arbeitsdirektor*—one of those strange new hybrids who are approved by the unions but are part of top management—in a large private enterprise pointed to the loosening up of industrial relations under this

act: "People remain in permanent conversation now; those old terms exploiter and agitator are no longer used; the employees now feel that they are no longer at the mercy of an inscrutable management." In the three other countries, however, I found very little interest on the part of either labor or management in any comparable scheme for codetermination, though joint consultation was endorsed, in varying forms and degrees of enthusiasm, by the majority of those interviewed.

9. Varying national and cultural traits appear to be of considerable significance in explaining managerial attitudes in each country, especially the attitudes toward employees and their organizations. Where the population has come to believe generally in persuasion and co-operation—often after a stormy industrial history—management interprets its social role and executive leadership function with restraint, and labor reacts accordingly. Despite outward similarity of the technical forms of industrial management, therefore, the actual attitudes and behavior patterns of both management and labor vary considerably according to the general value system of the population concerned.

A few of the executives interviewed, it is true, saw little difference between managerial problems of the various countries, but most of those who had had first-hand experience abroad took a different view. The head of a Swedish bank felt that most Swedish businessmen would be considered radical socialists in the United States. A Norwegian industrialist who is familiar with Germany contrasted the German readiness to be disciplined with the Norwegian desire to be told "why" and to be held in esteem. The chairman of a large British concern with international links thought the national differences in the way in which one gets co-operation to be most significant even when the forms of industrial organization are similar. He claimed that in the experience of his company in France, for instance, "no rules were possible," but that significant differences also existed within Britain; for example, between the courteous ways of both managers and employees in the Midlands and the intensely emotional reactions of their colleagues in Wales.

Systematic testing of the tentative results and hypotheses that were just presented and illustrated, with possible quantification of the additional data to be secured, would seem to be very promising. Such a study would require the prolonged co-operation of research institutes in every country to be included. More areas with diversified types of enterprise ownership should be added to the sample. In such co-operation, American participation could provide methodological leadership as well as a co-ordinating influence. The international exploration and comparison of the various determinants of managerial attitudes could put greater realism into the continuing debate on the merits and trends of economic institutions.

W. W. Cooper: The requirement of reviewing two papers within the limited time allotted to a discussant makes it desirable to confine attention to a few selected points from the many covered in these papers. So far as

possible, both papers will be discussed simultaneously and my own comments introduced as seems appropriate. I shall then conclude with some general observations and suggestions.

At the outset it may be well to qualify my remarks by noting that both papers apparently represent only parts of larger pieces of work to which I have not had access. It may be helpful, also, if I note that a fair assessment of these papers requires a rather broad view of economics as the science which is concerned with describing, predicting, assessing, and prescribing methods for changing economic institutions; i.e., institutions which are viewed as being primarily devoted to organizing and allocating scarce resources. Furthermore, these institutions need to be considered in both their formal and informal aspects.

Both papers are concerned with new directions for research in economics and both draw from "alien" disciplines. Drawing on the literature of business and administration Mrs. Penrose seeks to incorporate into economic theory the processes of entrepreneurial (or quasi-entrepreneurial) recruitment that firms have available. By this means she seeks to explain the growth of firms (over time) as well as limits to their size at any point of time. Mr. Hickman proposes to study managerial motivation by drawing on the constructs of sociology, psychology, applied anthropology, and related fields. In particular, he seeks to provide an empirical basis for establishing the range and relative importance of various managerial motivations, including profit maximization.

This general statement of their objectives already provides an example of important differences in the approaches of the two authors. Mr. Hickman's approach is empirical; Mrs. Penrose's is theoretical. This remark requires elaboration in order to avoid possible misinterpretations. Mrs. Penrose, in the traditional manner of economists, poses her problem in the context of certain large "facts" which appear to demand explanation: The fact in question is that firms at any particular point in time are clearly limited in their size and yet these firms by various devices which are evolved by themselves or others—e.g., training of a larger managerial class and development of new devices for recruitment, co-ordination, etc.—are enabled to grow beyond these limits at subsequent times. Her formulation can, at least conceptually, lead to empirical validation and reformulation. The important point is that her approach, even though resting in part on alien disciplines, seeks to provide a basis for systematic synthesis and change of the existing body of economic doctrine or theory.

Mr. Hickman does not take his facts for granted—other than some obviously inadequate and perhaps unguarded methodological statements culled from certain economic texts. He proposes to begin by collecting the necessary facts. Moreover, his approach rests squarely on the methods and knowledge of the disciplines he proposes to use. His paper does not make clear how either these methods or knowledge, or indeed his own findings, will be systematically used to synchronize with or to alter the existing discipline of economics. It is possible, of course, that this question is premature (or that it has been answered elsewhere), or that Mr. Hickman will be content with the cumulation of a body of factual information which he feels is

important in its own right. The task of assessment would have been facilitated if he had clarified the status of this matter.

Both papers promise an access to new problem material which is welcome in an area of economics which has not provided an adequate bridge for this purpose. An adequate bridge would, I believe, provide for two-way traffic. It would not only enable economists to move across it in search of the facts that their theory (or motivation) seems to require, but would also enable the world of economic institutions to bring to economics the kinds of problems which ought to be occupying our attention in the quest for new knowledge (and new light on old knowledge). The kinds of interrelations and communications that have now been achieved between governments and economics at the macro level might prove equally fruitful in the area that deals with the business firm at the micro level.

Of the two papers presented here, Mrs. Penrose's seems to offer the most promise in this direction. This may seem strange in view of the fact that her paper appears to be much more closely aligned with traditional economic formulations. Indeed her propositions can be regarded as rather simple extensions of the observations of Kaldor, Chamberlin, and others on the status of the entrepreneur as a fixed factor guaranteeing the ultimate appearance of diminishing returns to limit the size of firms. There is a difference, however, which may emerge more strongly in subsequent work. To explain the existence of these limits and, simultaneously, to explain growth beyond these limits, Mrs. Penrose has adopted the expedient of splitting the entrepreneur into a variety of planning agents. By this means she is able to fix the amount of entrepreneurship that the firm has available at any point in time and allow it to grow over time. This represents a step away from the traditional viewpoint wherein the business firm is treated as an elementary economic unit composed of an entrepreneur and selected factors of production. This simple entrepreneur-factor model of the firm as an organization is adopted for economic analysis, not, it may be argued, because economists are unaware that it is oversimplified for individual firm analysis, but because it is adequate for purposes of industry and general social analysis. I believe that this argument is subject to serious qualification in many respects. It leaves many questions unasked and unanswered. It also seriously hampers our ability to test many fundamental economic presuppositions.

In any event, Mrs. Penrose has taken, I believe, a step toward repairing the deficiency. Efforts to push further in this direction will, I think, lead inevitably toward analysis of the structure of business firms as a means of explaining their behavior. The difference in approach that I am attempting to portray may, perhaps, most easily be understood by reference to regarding the atom and the cell as the elementary units for analysis of matter and living organisms in contrast to regarding them as units with a structure and organization which require representation both in their own right and as part of the general study of physics and biology. By further movement in this direction we may secure a more adequate model of business firms which will assist in building the kind of bridge previously referred to. This may add to the layer of "aggregation problems" that economics now confronts but

it need not divert it from the task of social analysis—any more than modern physics has been diverted from the task of explaining physical behavior or any more than biology has been diverted from the task of explaining the behavior of organisms. In many cases our units, the firms, are so gigantic (or occupy such positions) that an adequate explanation of their behavior will suffice for an explanation of industry behavior as well. In other cases the devices of "representativeness" can be employed to advantage. Techniques of stratification and clustering can also be used if required.

It seems to me that Mr. Hickman is moving in a different direction. Unless he is willing to make rather strong assumptions which would equate managerial motivation and business firm behavior, his proposed agenda of discovery, even if successful, will not necessarily provide us with adequate descriptions, predictions, or assessments of these economic institutions. Failing these assumptions (and their validation), his analysis can provide us only with a means for gaining insight into the opinions, utterances, and behavior of a managerial "class." This is not to say that his analysis will be irrelevant to business firm behavior even in this extreme. Such class behavior might by a circuitous route—e.g., through its effects on government, public opinion, or the behavior and opinion of subordinates, union officials, etc.—affect the behavior of business firms. The point I am seeking to make is not that Mr. Hickman's findings will be valueless but that underlying his seemingly radical proposals is a model of the business firm which is of the same kind as the traditional economic version—an organization in which entrepreneurs, managers, or some dominant group of managers manifest their wishes and the firm behaves accordingly. For example, the entrepreneur "selects" an optimum transformation or cost function because he wishes to maximize profits. A study of the literature of standard costing or cost accounting practice will reveal that this is not the case. (See, e.g., James L. Dohr and Howell A. Ingraham, *Cost Accounting Principles and Practices* [New York: Ronald Press, 1946], Chapter 28.) Optimum costs, even when known, are regarded as undesirable bases for guiding and controlling behavior. Organizational (and psychological) considerations arising from the systems of delegation and control employed by modern business enterprise enter into these optimality rules in a way which is not comprehended by the traditional economic model of the firm. I might add that these considerations also enter into the methods of cost record keeping and estimation in a way which may make the resulting costs unsuitable from the standpoint of traditional economic analysis, even though essential to survival and maintenance of the firm as an organization.

Both papers invite far more attention than can be devoted to them here. Both are incomplete, imprecise, unclear, and unbalanced. But this criticism needs to be interpreted in the perspective of what the papers being reviewed are attempting to achieve. It is unfair, and perhaps dangerous, to demand such perfection and clarity from papers which constitute pioneering efforts in new directions as might be demanded from papers which restrict themselves to areas which have already been explored and partially settled.

Although she utilizes much of the traditional apparatus of economics in the form of indivisibilities, idle capacity (overhead) costs, etc., Mrs. Penrose's

model can also be represented by the differential equations of biological growth. (*Vide* V. A. Kostitzin, *Mathematical Biology* [London: Harrap and Co., Ltd.].) By splitting the enterpreneur into groups of planning agents, however, she has begun to introduce something which is to be found neither in traditional biological growth models nor in traditional economics. A further step consists of introducing hierarchical arrays of such agents as the common method for effecting recruitment and securing integration and control. She can avoid some of the difficult and tortured translation of traditional definitions and categories of cost to be found in her paper, I believe, by recognizing explicitly that she has departed from the model which gave them their original meaning. This is implicit, for example, in her description of planning "agents" as unused "factors"—as unused or wasted resources from the standpoint of operations, even though perhaps fully utilized from the standpoint of planning for future activities. J. M. Clark's concept of idle capacity and overhead cost does not sit any more gracefully and easily in this context than does E. A. G. Robinson's concept of "unbalanced additions." Nor is the problem resolved by reference to long- and short-run costs and profit maximization.

The distinction between planning and operations drawn by Mrs. Penrose is a real and significant one. I believe that she can more fruitfully proceed by maintaining this distinction than by assuming that planning agents become operating agents when their plans are put into effect. Preserving these distinctions will require introducing what Professor H. A. Simon (*Administrative Behavior* [Macmillan, 1948], page 9) calls "vertical" specialization into the model of the firm as well as the "horizontal" specialization which is envisioned in the usual process of labeling variables in the production function. A really good planning agent is likely to remain in the planning hierarchy (in a so-called "staff" capacity) rather than become an operating agent (in the so-called "line" organization) even when his plans are adopted. For concreteness, one might think of an operating agent in terms of the usual production foreman and a planning agent in terms of the usual industrial engineer (standard setter) or budget officer.

By introducing a hierarchical array of planning agents Mrs. Penrose will have introduced a new array of problems (e.g., the phenomenon of agent promotion within the hierarchy) into economics. As she correctly notes, this will also help throw new light on old problems as well; e.g., the problem of handling uncertainty at various levels of entrepreneurship or quasi-entrepreneurship within the firm over time. (It is for this reason that I have persistently referred to these persons as agents to designate a category lying between the traditional entrepreneur and factors of production.) The addition of another hierarchy of operating agents will add still further elements of richness. In particular, it will expose the problems of co-ordination and control that are most easily thought of in terms of still a third hierarchy of agents—a group that may be thought of as control agents (e.g., accountants). The systems of internal control maintained by such personnel are designed and maintained for this purpose within the context of the delegations made within and between the other two hierarchies.

A moment's reflection on these arrays will reveal, I believe, why the ac-

counting system must be maintained in terms of the documented historical cost record of the accountant rather than the undocumented opportunity costs of traditional economics. Control may be visualized as the process of securing conformance between plans and actions—a process, it should be noted, which may require adjusting plans to actions as well as the reverse. (See E. L. Kohler, *A Dictionary for Accountants* [Prentice-Hall, 1952], page 108.) Clearly it is as undesirable to allow industrial engineers (planning agents) to set arbitrary standards of performance as it is to allow production superintendents (operating agents) to behave arbitrarily with respect to these standards. This is the nub of the problem of internal control and system design as visualized in accounting, especially if one views it in the context of a system of authority delegations which makes it desirable to minimize upper level intervention in operations and plans. Efficiency can then be viewed in terms of the levels at which the controls operate (e.g., costs and volumes) as against possible alternatives. Opportunity cost thus may become an ancillary device for the accountant although a primary device for operating and planning agents.

Models constructed along these lines will, I believe, provide more adequate bases for understanding, predicting, and assessing the behavior of individual business firms engaged in allocating scarce resources. They will, of themselves, suggest new problems and methods of attack. Perhaps even more important, such models and studies should contribute toward building a more adequate bridge between economics and business firms.

I am afraid that I have few suggestions to make in connection with Mr. Hickman's paper. A good deal depends on whether one agrees with his optimistic assessment of the state of the knowledge and the methods of the sciences on which he proposes to draw. I do not feel wholly qualified to render a judgment on this although, in common with many other economists, I have myself been disappointed in past attempts to draw on these disciplines. I remain to be persuaded, however, that we will be able to use these techniques without a good deal of selection and adaptation, although I should hasten to add that I feel the same way about techniques drawn from other fields as well. (That this is true even in such purely methodological fields as statistics has been made clear by recent work of the Cowles Commission and others in relation to problems of structural estimation and identification.) For example, what I am told are Professor Linton's applied anthropological constructs of status and role may be suitable approximations for analyzing an informally organized society. It is by no means clear that they will prove equally useful in discussing entities, such as business firms, which have important elements for formal organization as well. Lazarsfeld, Newcomb, and others may have reported some success in applying these constructs to studies of the American soldier in a military organization which does have important formal aspects. But it should be realized that the relative permanence of status designations in such a military organization may be misleading in studying (competing) business organizations. Perhaps a more apt analogy is in terms of a play being put together and operated by a director who retains the right to edit the script and change both status and roles in the light of his judgment of

the actor potentials at his disposal and possible competing plays which are (or will be) produced at the same time.

I have already indicated my uncertainty as to how Mr. Hickman proposes to translate his findings (and methods) into the institutional entities with which managers, qua managers, are concerned. Perhaps by beginning with already available data, such as the Commerce Department-SEC series on plant expenditures, he could effect such a translation. The types of methods he employs in terms of self-theory (self-directed depth interviews, content analysis, etc.) are likely to prove quite expensive even if he restricts his attention to such ongoing series. If he proposes, instead, to employ himself in studying motivations, aspirations, etc., of a managerial class, the task is likely to take on truly forbidding proportions. Even modern sampling methods would require huge numbers to achieve any degree of representativeness from some five million firms of varying sizes scattered over a multiplicity of industries and regions—and status, self, and role are likely to vary to some extent with each of these properties. Of course, he may restrict his studies to a handful of firms and managers. He would then face, it seems to me, a rather difficult problem of generalizing his findings concerning the relative importance of different kinds of motivation in a form which would command the confidence and respect of others. Such empirically oriented studies as he proposes would require this kind of confidence and respect if they are to have the effects he indicates.

I am afraid that I can do little more than point out these possible pitfalls to Mr. Hickman. One way to circumvent some of them would involve the construction of adequate models utilizing, at least tentatively, the kinds of findings that Mr. Hickman might conceivably envision as an end product of his studies. Such a course would involve some degree of change in emphasis and timing in Mr. Hickman's proposed agenda. In any event I have already devoted sufficient attention to suitable models of the business firm in my comments on the paper by Mrs. Penrose.

PRICE AND WAGE FLEXIBILITY

ECONOMIC CONCENTRATION AND DEPRESSION PRICE RIGIDITY

By JOHN M. BLAIR
Federal Trade Commission

On January 17, 1935, the Seventy-fourth Congress of the United States issued Senate Document No. 13, *Industrial Prices and Their Relative Inflexibility*, thereby touching off what became one of the most vigorous controversies in recent economic literature. On one side of the issue was Gardiner C. Means, who, as Economic Adviser to the Secretary of Agriculture, had prepared the document which was then, in his words, "commandeered" by the Senate. With him were Ralph C. Wood and, at the time, J. Kenneth Galbraith. On the other side was a more numerous host, including Willard Thorp, Rufus Tucker, Jules Backman, Edward S. Mason, Don D. Humphrey, Alfred Neal, Ernest Doblin, and others. Following the publication of the Senate Document, the controversy progressed from general arguments in the economic journals to elaborate statistical works, the most ambitious of which were prepared by Means, Thorp, and Neal. The outbreak of World War II had the effect of severing the strands of argument, and since that time the whole controversy has remained more or less in a state of limbo.

The controversy ranged over a number of economic issues, only one of which—the relationship between economic concentration and price flexibility—will be discussed in this paper. Among the issues which will not be examined are the economic effects of price rigidity, the relationship between price and production behavior, the history of price rigidity, the various statistical measures of price change, the adequacy of the data used to measure price behavior, etc. Obviously, each of these topics could, in itself, provide the subject for one or more papers. And even on the one topic to which this paper is limited, time does not permit a discussion of the general and theoretical arguments offered in the controversy.

The principal evidence was offered by Gardiner C. Means on the one side and by Willard Thorp and Alfred C. Neal on the other.[1] Putting

[1] National Resources Committee, *The Structure of the American Economy*, prepared under the direction of Gardiner C. Means, Pt. I, 1939, p. 142; Temporary National Economic Committee, Monograph No. 27, *The Structure of Industry*, by Willard Thorp and Walter Crowder, 1941, p. 357; Alfred C. Neal, *Industrial Concentration and Price Inflexibility* (American Council on Public Affairs, 1942), pp. 91, 92, and 118.

it briefly, Means found that an inverse relationship between the level of concentration and the amplitude of price change did exist. Thorp found that it did not. In Thorp's view, differences in behavior were associated not with differences in concentration but with differences in the characteristics of the product. Neal found that "price change is closely associated with direct cost change and is not due to concentration."

Means's analysis has been criticized on the grounds that the price data which he employed, the BLS wholesale price series, do not reflect hidden or secret discounts. Thorp's study is subject to criticism on two grounds: that he employed no standards, as did Means, to eliminate products which are not meaningful for this type of analysis, and that the price data which he used—the census realized prices—may fluctuate merely because of a change in the relative importance, or "weights," of the various individual products within a census product class. Neal's study, which is also based on census data, is open to the same objection, plus the additional criticism that his analysis assumes that during a downswing an industry's direct costs remain proportional to the average weighted price of its products. According to Neal, it is reasonable to assume that during the Great Depression a shift from high-priced to low-priced goods within an industry "would be reflected in the average direct cost index also"—and reflected in the same degree and extent!

What is offered here is a new approach which will bypass some of the more important limitations inherent in the previous studies. Each of these previous studies involves comparisons of different products with varying degrees of concentration. But, as was pointed out during the course of the controversy, products may differ from each other, not only with respect to concentration, but they may also vary with respect to such other factors as elasticity of demand, cost behavior, and adequacy of price data, each of which has been cited as an explanation for the appearance of price rigidity. If instead of comparing different products the same product is examined during two depressions, the effect of these other variables can largely be isolated, since it need then only be assumed that, as between the two depressions, they were affected by no significant change.

Although the Great Depression of 1929-33 obviously suggests itself as the later period to be used in such a comparison, there remains the problem of finding a depression which antedates—though not too greatly—the emergence of high levels of concentration. This problem is aggravated by the fact that in many American industries concentration has been high since around the turn of the century. It happens, though, that immediately prior to the great consolidation movement of 1897-1903 this country experienced a rather severe depression, begin-

ning in 1890 and lasting for several years.[2] In this study 1897 has been selected as the terminal year, since, like 1933, it appears to be a year in which the deflationary forces finally exhausted themselves. Although the depression of 1890-97 was a serious one, no depression in American history has approached in severity the depression of 1929-33. Thus, all other things being equal, a showing of equal flexibility is not to be anticipated since prices would be expected to decline less in the earlier than in the later depression; a showing of greater flexibility in the earlier period would be all the more remarkable.

The price data used in this analysis are the wholesale price series of the Bureau of Labor Statistics. In most cases the description of the product is virtually the same in both periods, although this is not an essential requirement. Price series covering both depressions are available from the Bureau of Labor Statistics for 168 commodities, of which 122 are used in this analysis.[3]

The first step in the analysis is to show the price changes for products which, by and large, were atomistic areas in both depressions. If Means's argument is correct, such products would be expected to show no significant change in price behavior; they certainly would not be expected to show any decrease in their flexibility. Chart I shows

[2] According to the long-term index of "American Business Activity," prepared by the Cleveland Trust Co., the general period was one of depressed economic conditions, with a downswing in 1893 and 1894 ("Panic of 1893"), followed by a very brief upturn ("Recovery of 1895"), which was in turn followed by another downswing of about equal severity in 1896 and 1897 ("Silver Campaign Depression"). Warren and Pearson's long-term index of physical production shows a decided dip ending in 1895, followed by a lesser downturn in the next two years. To the extent that price trends can be assumed to mirror the general state of economic conditions, the available information would suggest that the low point was not reached until 1896 or 1897. The all-commodity wholesale price index of the Bureau of Labor Statistics (with 1913 at the base of 100) declined from a level of 80.5 in 1890 to 66.7 in 1896, rising by only a tenth of a percentage point, to 66.8, in 1897. The majority of the individual commodity groups did not reach their low point until 1897. In describing the period, the Commissioner of Corporations used such terms as "depressed conditions," "commercial depression," "slack demand," "acute depression," etc. (*Report of the Commissioner of Corporations on the Steel Industry*, Pt. I, 1911, pp. 63, 72, 75.)

[3] The products for which price series are available but which are not included in this analysis fall into the following groups: fabricated metal products (17 items); chemicals (12 items); stone, clay, and glass, other than plate and window glass (8 items); petroleum and coal other than anthracite (5 items); nonferrous metals other than copper, lead and zinc (3 items) and miscellaneous products (1 item). The 17 fabricated metal products consist of simple hand tools and fixtures (e.g., saws, axes, chisels, doorknobs, etc.) which are relatively unimportant items and for which information on concentration in the earlier depression is lacking. The chemical and drug items were omitted because of the conspicuous unreliability of the BLS price series for this particular group. The stone, clay, and glass products were omitted because information on long-term changes in concentration is unavailable; this is particularly true of products sold on a regional or local basis within which significant changes in concentration may have taken place. Price series during both depressions were available for only two petroleum products, Pennsylvania crude oil—which is hardly representative of crude oil production—and kerosene—which by 1933 had nearly passed into economic oblivion. In addition to copper, lead, and zinc, price series were available for only three other nonferrous metals—quicksilver, tin, and silver—the prices of which are largely determined by developments abroad or by monetary policy.

COMPARISON OF PRICE FLEXIBILITY IN TWO DEPRESSIONS
1890-97 AND 1929-33

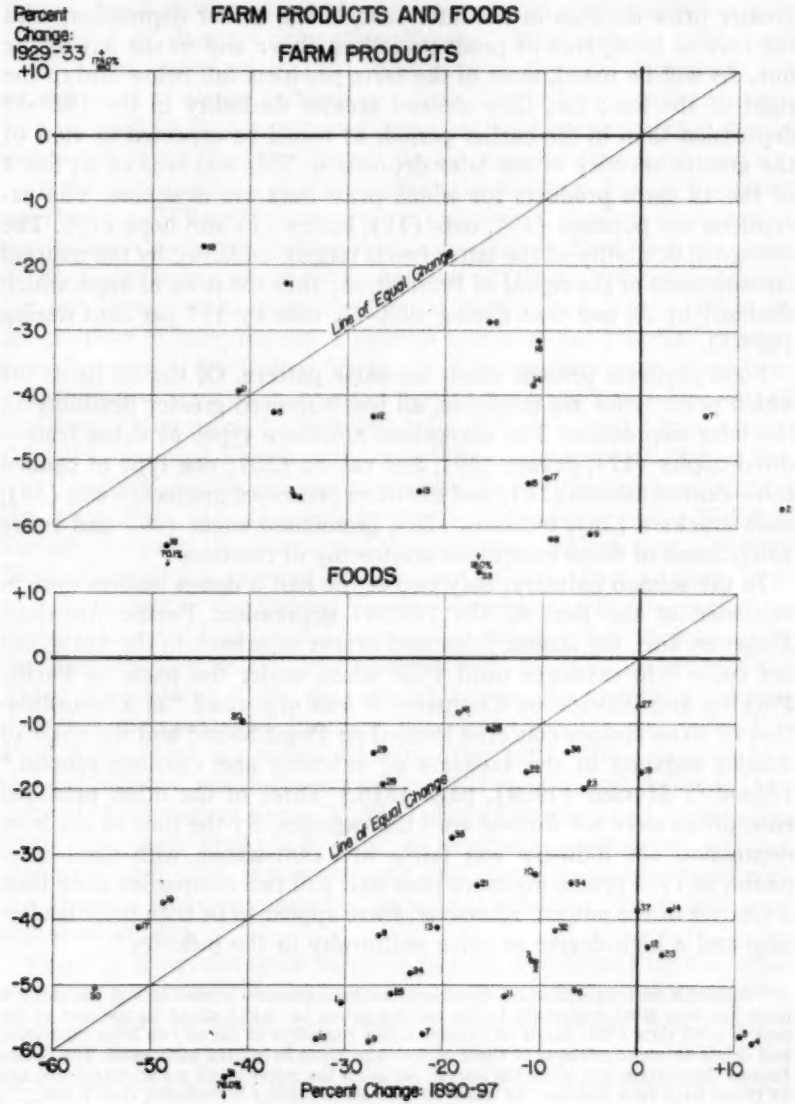


CHART I

the price changes in 1890-97 and 1929-33 for farm products in the upper grid and foods in the lower. Each grid is bisected by a line of equal change. Products falling below and to the right of the line had greater price declines in the later than in the earlier depression, with the reverse being true of products falling above and to the left of the line. As will be noted, most of the farm products fall below and to the right of the line; i.e., they showed greater flexibility in the 1929-33 depression than in the earlier period, as would be expected in view of the greater severity of the later depression. This was true of all but 4 of the 19 farm products for which price data are available. The exceptions are potatoes (18), oats (11), barley (1) and hops (10). The change in flexibility of the latter two is largely explained by the unusual circumstance of the repeal of Prohibition; thus the price of hops, which declined by 56 per cent during 1890-97, rose by 174 per cent during 1929-33.

Food products present much the same pattern. Of the 38 items for which price series are available, all but 9 showed greater flexibility in the later depression. The exceptions are three types of dried fruit—dried apples (17), prunes (19), and raisins (20); one type of canned fish—canned salmon (16); and five other processed products—salt (38), soda crackers (26), molasses (28), granulated sugar (29) and coffee (30). Some of these exceptions are worthy of comment.

In the salmon industry, only two of the half a dozen leaders were in existence at the time of the 1890-97 depression. Pacific American Fisheries, Inc., the largest fisher and canner of salmon in the world, did not come into existence until 1902 when, under the name of Pacific Packing and Navigation Company, it was organized "as a consolidation of some sixteen concerns located on Puget Sound and the coast of Alaska engaged in the business of catching and canning salmon." (*Moody's Manual* [1904], page 1516.) Three of the other principal enterprises were not formed until the twenties. By the time of the later depression, the industry was fairly well centralized, with eight companies in 1934 producing more than half and two companies more than a quarter of the output. Moreover, there appear to be both price leadership and a high degree of price uniformity in the industry.[4]

[4] "Although no cooperation or agreement has been proved to exist among the canners, there has been great uniformity in the opening prices [at which about 90 per cent of the pack is sold] since 1905; nearly all canners follow the prices of one or two large companies, and refuse to quote prices until those of the large firms have been announced. The Alaska Packers Association has taken the lead in declaring the prices of all grades except one and its prices have been followed by nearly all the other canners in declaring their prices. . . . Both the uniformity of the prices declared and the uniformity of time of declaration suggest that the prices of one or two leading firms are used as a guide." (Arthur R. Burns, *The Decline of Competition* [McGraw-Hill, 1936], p. 139.)

The same general type of situation applies to the salt industry. In that field the two leading companies—International Salt Company and the Morton Salt Company—were not formed until after 1897. The former, which now produces about 25 per cent of the industry's output (*Standard & Poor's Corporation Records,* December 8, 1949, page C-2-5), was organized in 1901, acquiring in the next four years the capital stock of eleven independent firms. Similarly, the latter was not formed until 1922, when it was organized as a successor to a line of companies originating in Chicago during the mid-nineteenth century.

In the soda cracker industry, neither the National Biscuit Company nor the Loose-Wiles Company were in existence at the time of the earlier depression, the former having been organized in 1898 and the latter in 1912. According to *Moody's Manual* of 1901: "National Biscuit Co. was incorporated under the laws of New Jersey, Feb. 3, 1898, for the purpose of carrying out a plan to purchase the principal cracker manufactories in the United States. . . . The company owns many baking plants and controls the greater part of the trade in this line east of the Rocky Mountains." (Page 985.) In 1901, it owned fifteen plants. By 1935, National Biscuit and Loose-Wiles, together with United Biscuit Company, sold 74 per cent of the biscuits and crackers produced by the more than 330 American baking establishments, with National Biscuit Company accounting for approximately 42 per cent. (Federal Trade Commission, *Agricultural Income Inquiry,* Part 3, 1938, page 41.) According to the Federal Trade Commission: "In the cracker industry it is stated that manufacturers must and do follow the lead set by the National Biscuit Co. and Loose-Wiles." (*Report on Open Price Trade Associations,* 1929, page 78.) Likewise, the two principal producers of molasses—the American Molasses Company and Pennick & Ford, Ltd.—were not organized until after 1897. The American Molasses Company was incorporated in 1905 as a consolidation of two formerly independent firms: N. W. Taussig Company and Eastern Refining Company. Pennick & Ford, as originally constructed, was not organized until 1898. It was not until 1920 that it was consolidated with the Douglas Company, another important producer, which had been organized in 1903.

Chart II shows the price behavior in both depressions for two other atomistic areas: textiles and lumber. In both, the scatters resemble those of farm products and foods. Two-thirds of the thirty textile products showed greater flexibility in the later depression. Half of the exceptions consisted of one type of commodity: wool products. Similarly, in all but two of the thirteen lumber and wool products price flexibility was greater in the 1929-33 period; the exceptions are bed-

COMPARISON OF PRICE FLEXIBILITY IN TWO DEPRESSIONS 1890-97 AND 1929-33

TEXTILES AND RELATED PRODUCTS and LUMBER AND WOOD PRODUCTS

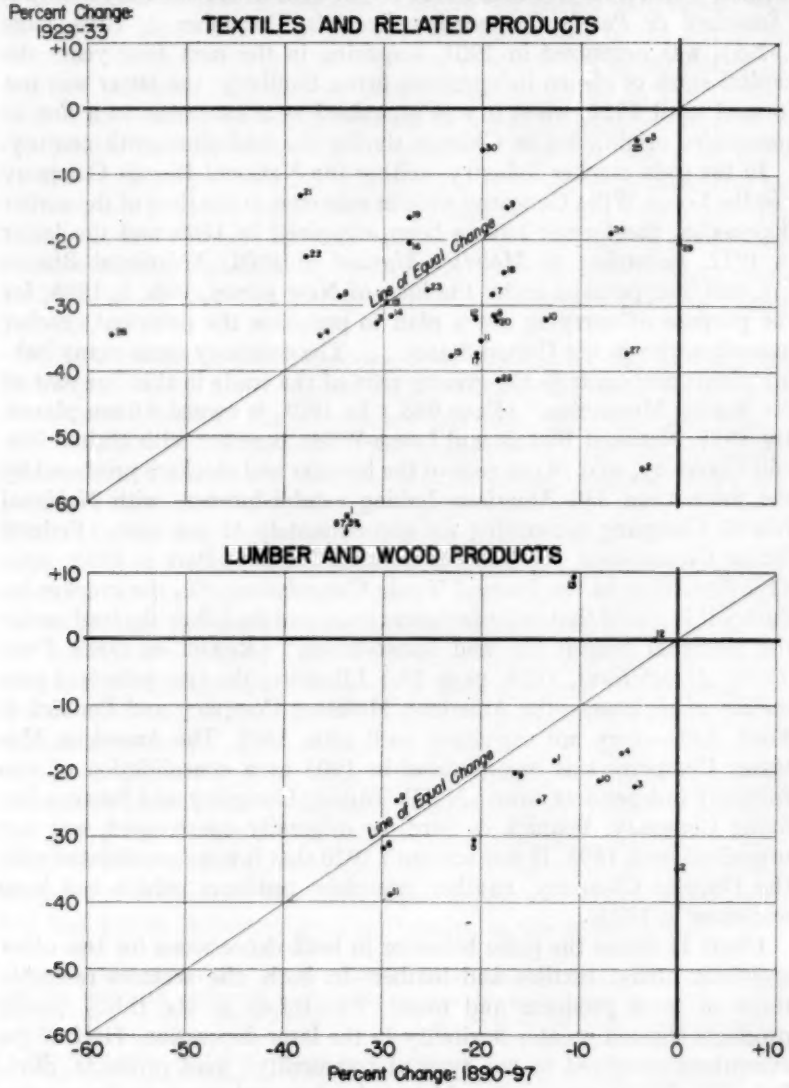


CHART II

room chairs (12) and matches (13), the latter of which is worthy of note.

While the Diamond Match Company was formed ten years before the beginning of the 1890-97 depression, it was not until 1901 that it entered into an agreement with the British firm, Bryant & May, Ltd., under which the latter agreed "to refrain from manufacturing or selling matches in North America." And it was not until 1903 that it entered into a similar agreement with the Swedish Match Company, under which the latter "agreed not to manufacture matches in the United States except by arrangement with Diamond Match Co."[5] Inasmuch as the safety match had been a foreign development and sales by foreign companies had been an important factor in the domestic market, it was not until these cartel agreements were made that the domestic market could be said to have been controlled. But during the 1929-33 depression, the Diamond Match Company exercised unquestioned control over the domestic market, not only through its position as the leading producer, but also through its holdings in other match companies. In 1939 the stockholders of the Diamond Match Company owned the shares of the Ohio, Lion, Universal, Federal, and West Virginia match companies. Diamond's president held 51 per cent and Diamond itself held the other 49 per cent of the stock of the Berst-Forster-Dixfield Company. These seven concerns, together, produced nine-tenths of the nation's output of matches. (Clair Wilcox, *Competition and Monopoly in American Industry* [TNEC Monograph 21, 1941], page 191.)

Apart from these exceptions, however, the general pattern in each of these four atomistic areas—farm products, foods, textiles and lumber—is the same. As would be expected from the unmatched severity of the Great Depression, prices tended to drop more in 1929-33 than in 1890-97. Against this background, the distribution of price changes in the steel industry becomes of special interest. Chart III shows the price changes in both depressions for nine iron and steel products.

As will be noted, the pattern is the reverse of that of the atomistic areas. With one exception, hot-rolled sheets (7), the observations fall above and to the left of the line of equal change; i.e., prices dropped less in the later than in the earlier depression. Findings pointing to the same conclusion were reached by Abraham Berglund in his study of price behavior before and after the formation of the U.S. Steel Corporation (specifically in 1898 as compared to 1902-14) and by George

[5] Report of the Commissioner, Combines Investigation Act, Department of Justice, Ottawa, Canada, *Investigation into an Alleged Combine in the Manufacture, Distribution and Sale of Matches*, December 27, 1949, p. 7.

COMPARISON OF PRICE FLEXIBILITY IN TWO DEPRESSIONS 1890-97 AND 1929-33

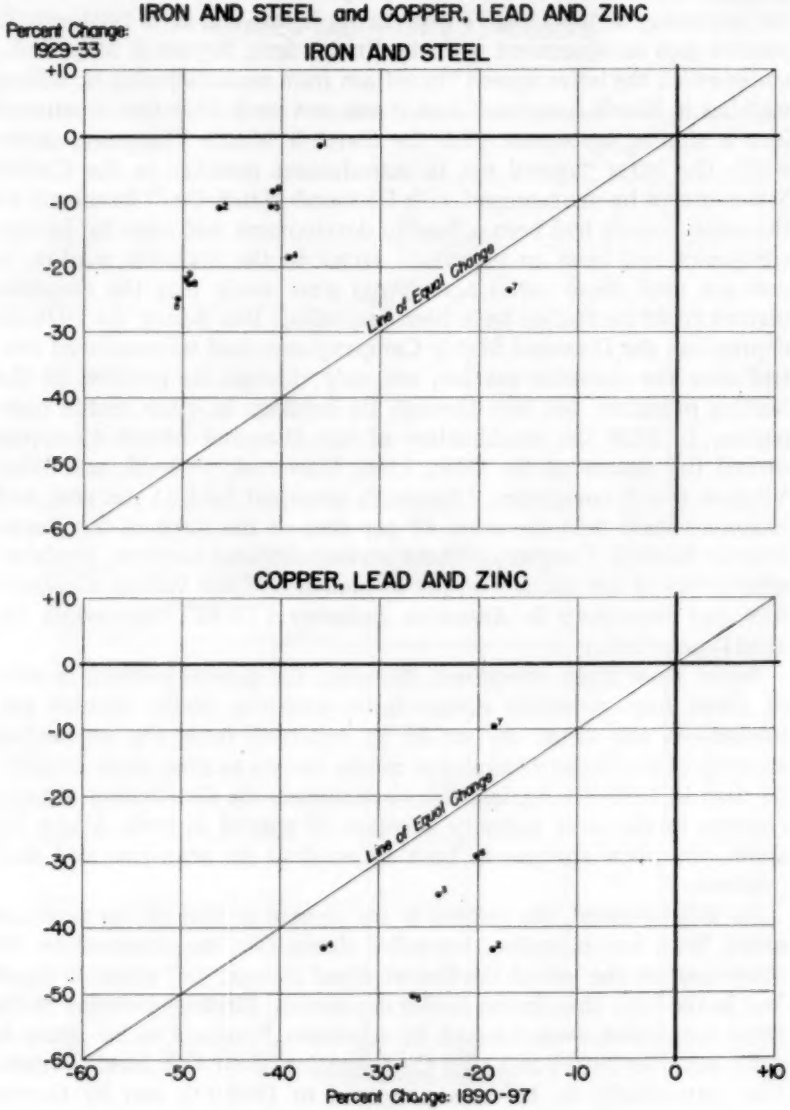


CHART III

Stigler in his comparison of the actual to the total possible number of price changes during 1898-99 as contrasted to 1939-40.[6]

Measured on any basis, concentration in the steel industry during 1929-33, while lower than in the years immediately following the formation of the U.S. Steel Corporation, was far higher than in 1890-97. The Commissioner of Corporations[7] states: "In 1890 there were scarcely any consolidations of the modern type in the steel industry. [The industry] was characterized by the competition of a large number of independent concerns." The only area in which concentration was at all noticeable was in the simpler and heavier forms of steel. But even here it took seven companies to account for half of the output, as represented by steel ingot production. According to the commissioner: "These concerns were . . . distinct entities with respect to ownership. . . ." (*Ibid.*, page 65.) In 1937 only four companies accounted for 71.7 per cent of the value of product of open-hearth and Bessemer steel. (TNEC Monograph No. 27, *op. cit.*, page 463.)

Not only was the industry in the earlier depression composed of a "large number of independent concerns"; vertical integration was generally conspicuous by its absence. The Commissioner of Corporations states: "Thus, the production of ore, broadly speaking, was a business by itself, and comparatively few iron and steel companies had extensive holdings of ore lands. . . . Moreover, a substantial proportion of the country's pig iron was produced by 'merchant' furnaces, which sold their product instead of manufacturing it into more finished articles. Even the larger manufacturers of steel bought considerable quantities of pig iron. . . most manufacturers of finished articles purchased their supply of semifinished steel instead of producing it." (*Report of the Commissioner of Corporations, op. cit.*, page 66.)

It is true that during the 1890-97 period the steel companies made repeated attempts to control the market through pools and gentlemen's agreements. But it is also true that these loose forms of control proved to be relatively ineffective, tending to disintegrate whenever demand began to fall off. Vernon Mund writes: "During the recession of 1896-97, all of the variously reported pools and price associations appear to have collapsed and the demoralization of trade became acute." (Vernon A. Mund, *Open Markets* [Harper & Bros., 1948], page 163.) (It should

[6] Abraham Berglund, "The United States Steel Corporation and Price Stabilization," *Quart. Jour. Econ.*, November, 1933; George J. Stigler, "A Theory of Delivered Price Systems," *Am. Econ. Rev.*, December, 1949, p. 1152.

[7] *Report of the Commissioner of Corporations on the Steel Industry*, Pt. I, 1911, p. 63. It should be noted that the period under examination in this study antedates the last two years of the century which saw the formation of a number of important consolidations, most of which were, in turn, absorbed by the U.S. Steel Corporation in 1901. (Cf., *ibid.*, p. 79.)

be noted that pools for a number of the industry's most important products were not formed until after the 1890-97 depression had run its course. Thus the structural steel pool and the steel plate pool were not formed until 1897 and 1900, respectively.)

In their final judgments, the authorities seem to be in complete agreement. Speaking of its early years, Myron Watkins describes the industry as "a competitive trade." (Myron W. Watkins, *Industrial Combinations and Public Policy* [Houghton Mifflin, 1927], page 123.) Charles R. Van Hise concludes that "during the period before consolidation, competition had been severe and prices alternately high and low, depending upon the trade conditions and the extent of cooperation." (*Concentration and Control* [Macmillan, 1921], page 113.) The Commissioner of Corporations is explicit: ". . . as a rule the ownership of iron properties was widely scattered and, in this, as in the manufacturing branch of the industry, competition *rather than concentration* was the distinguishing characteristic. . . ." (*Report of the Commissioner of Corporations, op. cit.*, page 66. Italics added.)

The distribution of the copper, lead, and zinc products shown on the lower grid of Chart III is in sharp contrast to the scatter of the iron and steel products. With only one exception—zinc sheets—these nonferrous metals were characterized by greater flexibility in the later than in the earlier depression. It is true that the concentration ratios of manufacturing production for these products are relatively high. It is also true that their prices are extremely flexible. But it does not follow that this association constitutes an exception to Means's argument. The reason lies in the fact that the available domestic concentration ratios for these products substantially overstate the control of the market.

In the first place, these commodities have long been sold on a world market and their prices are immediately responsive to international developments. Hence figures on domestic concentration substantially overstate the control of the market, and the cost position of the US producers accentuates the overstatement. Moreover, they are produced in scattered areas of the world under widely differing conditions, a circumstance which has given rise to numerous conflicts among the different producers.[8] In the second place, concentration figures based on domestic production overstate the actual control of the market because of the important role played by secondary metals: "Copper is

[8] For a discussion of the attempts to establish effective cartels for these products in the later depression, see *International Control in the Nonferrous Metals*, edited by William Yandell Elliott (Macmillan, 1937), Chaps. VIII, X, and XII, by Alex Skelton, and Chaps. IX, XI, and XIII by Elizabeth S. May. Similar attempts during the earlier depression are described in the US Industrial Commission, *Report of the Industrial Commission*, Vol. 1, 1900, p. 93, and *Engineering and Mining Journal*, May 18, 1895, and January 5, 1895.

practically indestructible. . . ." In 1930 scrap dealers and secondary refiners supplied 35.9 per cent of the country's copper requirements. In the lead industry, the automobile storage battery reservoir forms "a huge circulating fund"; by 1933 the proportion of secondary to domestic primary lead was 86 per cent, while in zinc the proportion in 1932 was 23 per cent. (Skelton, *op. cit.*, pages 625, 772.) Finally, much of the output of the independent lead and zinc mines can and does find its way to the market through the medium of custom smelters. Inasmuch as these smelters operate on the basis of a fixed margin between the ore price and the refined metal price, it is to their interest "to operate at a high level of capacity, regardless of price." The resulting conflict with the interests of the integrated companies "became apparent from the commencement of efforts to agree on a united policy." (*Ibid.*, pages 727, 711.)

A special factor in the great price decline of copper between 1929 and 1933 was a bit of apparent business stupidity which has few parallels in modern industrial experience: the raising of the price from around 11 cents a pound, where it had remained for several years, first to 18 cents and later for a hectic fortnight to 24 cents. Since industrial output and, with it, the demand for copper were falling rapidly, these prices resulted in a fantastic increase in stocks, and the price subsequently collapsed, dropping to a low of 5.7 cents in 1932. (Cf. May, *op. cit.*, pages 525, 567.)

The final chart, number IV, presents in its upper grid the price changes for four products of the glass industry, two for plate glass (1 and 2) and two for window glass (3 and 4). As between the two depressions, the plate glass items show a great decline in price flexibility and the window glass products a great increase.[9] There is reason to believe that in the former, concentration rose, while in the latter, it fell.

The early growth of the plate glass industry in this country was largely the work of one man—J. B. Ford—who was "an intelligent entrepreneur of the most versatile type." Up to the time when he built his first plant, there were only five plants in the industry, with a combined output of only $355,000. By 1890, there were sixteen plants and the aggregate output had increased more than tenfold, to $5,953,000. Most of this expansion came about as the result of Ford's activities. From the point of view of its effect upon economic concentration, his method of operation was both unusual and significant. After constructing his

[9] Because the consolidation of most of the industry into the Pittsburgh Plate Glass Company took place in 1895, the price comparison for the first depression is between 1890 and 1894 rather than between 1890 and 1897. These first four years of the decade constituted a period of substantial economic decline.

first plant, "Ford built one new plate glass works after another, managing each for a short time, *then selling out* to free himself for construction of larger and better plants, representing in every case successively greater capital investment." (Pearce Davis, *The Development of the American Glass Industry* [Harvard University Press, 1949], pages 165, 166. Italics added.) Toward the latter part of the depression of 1890-97, the industry witnessed a great consolidation, out of which emerged the Pittsburgh Plate Glass Company as the nation's dominant producer:[10] "In 1895, through a reorganization, it [Pittsburgh Plate Glass] acquired all but three of the factories making plate glass in the country." (*Ibid.*, page 239.) Since the turn of the century, Pittsburgh Plate Glass has easily dominated the industry. By the twenties the company accounted for approximately 50 per cent of the United States output, "at which point it appears to have been maintained." (Burns, *op. cit.*, page 224.)

In striking contrast to the plate glass industry, price flexibility in window glass was much greater in the later than in the earlier depression. Obviously, the greater severity of the 1929-33 depression would explain only part of the extensive difference in price behavior. For over a decade prior to the earlier depression the window glass industry in the United States "was ruled by the joint power of the combination of workers and the combination of employers." (Davis, *op cit.*, page 131.) Or as another authority put it: "In no branch of the [glass] industry during the period 1880-1920 did organizations of laborers, employers, and wholesale dealers wield as much monopolistic power for as long a time as did those involved in the production of window glass." (Scoville, *op. cit.*, page 217.) Through Local Assembly 300 Knights of Labor of the Window Glass Workers of America, rigid limitations were imposed as to the amount of output to be "blown" per hour, per week, per month; on the number of months worked per year, which were gradually reduced by 1885 to nine; on the amount of the labor supply, etc. "It is thus clear from the beginning of its national history, L. A. 300 assumed rigid control of window-glass production." On their part, employers were organized into the American Window Glass Manufacturers Association, which, it is said, "decided how many and what works should be closed, what wages should be paid (in negotiation with the national union) and what prices charged."[11] In 1895 a

[10] Its predecessor was the New York City Plate Glass Company, organized in 1881. The name was changed to Pittsburgh Plate Glass Company in 1883. But it was not until 1895 that the consolidation was effected. (Warren Scoville, *Revolution in Glassmaking* [Harvard University Press, 1948], p. 239.)

[11] Davis, *op. cit.*, p. 130. For a further description of the detailed methods of restriction used by the union, see Department of Commerce, *The Glass Industry*, Misc. Series No. 60, 1917, pp. 291-293.

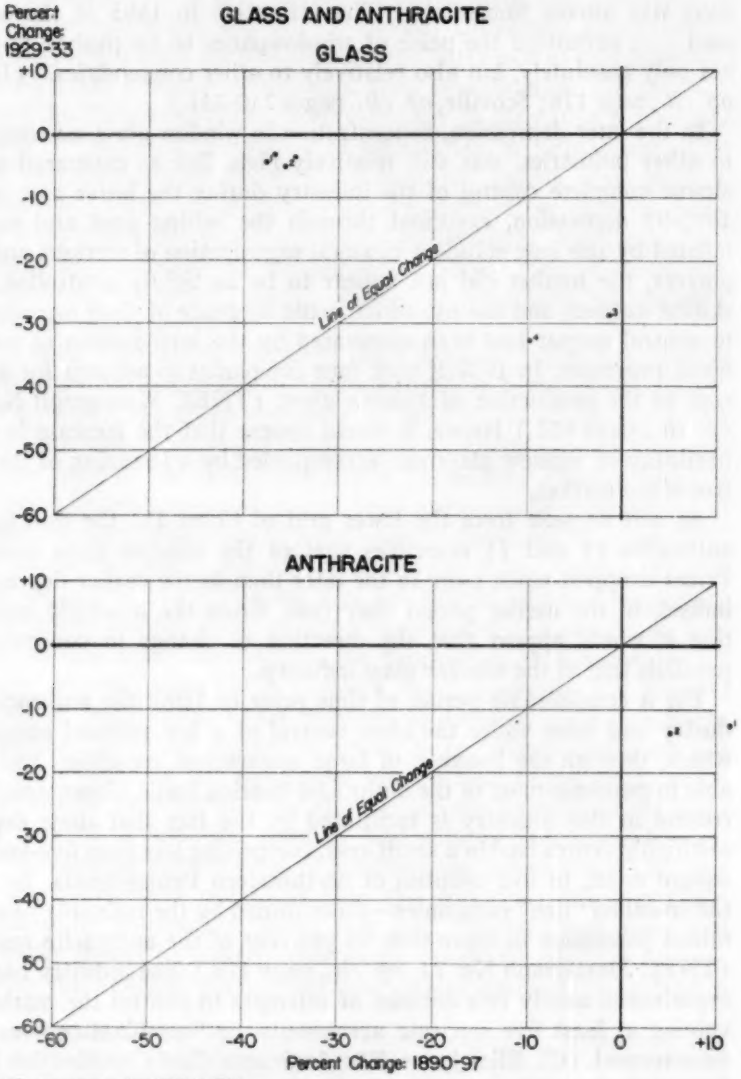
selling pool was founded, known as the American Glass Company, which handled the business of 85 per cent of the window glass factories then in existence. The effect of this new organization upon the price level was almost immediate. "The formation in 1895 of the selling pool . . . permitted the price of windowpanes to be pushed upward, not only absolutely, but also relatively to other commodities." (Davis, *op. cit.*, page 176; Scoville, *op. cit.*, pages 230-231.)

In the later depression, concentration in window glass, as compared to other industries, was still relatively high. But as compared to the almost complete control of the industry during the latter part of the 1890-97 depression, exercised through the selling pool and supplemented by the long-standing bilateral organization of workers and employers, the market did not appear to be as tightly controlled. The skilled workers and the use which could be made of their organization to control output had been eliminated by the introduction of mechanized processes. In 1937 it took four companies to account for 85 per cent of the production of window glass. (TNEC Monograph No. 27, *op. cit.*, page 455.) Hence, it would appear that the increase in price flexibility of window glass was accompanied by a lessening of the control of the market.

As will be seen from the lower grid of Chart IV, the showing for anthracite (1 and 2) resembles that of the window glass industry. Prices dropped much more in the later than in the earlier depression; indeed, in the earlier period they rose. From the available information it would appear that the direction of change in concentration parallels that of the window glass industry.

For a considerable period of time prior to 1890, the anthracite industry had been under the close control of a few railroad companies which, through the issuance of large amounts of securities, had been able to purchase most of the anthracite-bearing lands. Concentration of control in this industry is facilitated by the fact that these deposits are highly centralized in a small area, comprising less than five-hundred square miles, in five counties of northeastern Pennsylvania. By 1890 the so-called "line" companies—those owned by the railroads—had obtained possession of more than 95 per cent of the anthracite reserves. (TNEC Monograph No. 21, *op. cit.*, page 179.) The industry had also experienced nearly two decades of attempts to control the market involving at least five separate agreements, or "combinations" as they were termed. (Cf. Eliot Jones, *The Anthracite Coal Combination in the United States* [Harvard University Press, 1914], page 229.) Like most such arrangements, the history of these agreements was typically one of temporary success, followed by recurrent noncompliance and breakdown, and then by reconstitution on a somewhat modified basis.

COMPARISON OF PRICE FLEXIBILITY IN TWO DEPRESSIONS
1890-97 AND 1929-33



*Item 1 and 2 comparison is from 1890 to 1894.

CHART IV

Although other factors were occasionally involved, the root cause of their failure was invariably the same; namely, the refusal of one or more of the leading companies to remain content with its allotted share of the market. (Cf., *ibid.*, pages 43, 45, 48, 50.)

During the depression of 1890-97 there were two organized attempts to control the market, only the latter of which is relevant to the price comparison used in this analysis. This second attempt resembled in form the old allotment plan but differed in substance in that on this occasion the different producers generally did abide by their allotted shares of the market: "The division of tonnage was well adhered to by the companies. *In every case the deviation of shipments from the percentage allotted was less than one-half of one per cent.*" (Jones, *op. cit.*, page 56. Italics added.) The success of the plan is indicated by the increase in price, the average price of stove coal rising from $3.13 a ton in 1895 to $3.79 in 1896 and to $4.01 in 1897. (*Ibid.*, page 56.)

As contrasted with the earlier period, the most important difference in the later depression was the loss in position of the line companies to new large independent firms "who could not be controlled." (William R. Pabst, Jr., "Monopolistic Expectations and Shifting Control in the Anthracite Industry," *Review of Economic Statistics*, February, 1940, page 52.) With the oncoming of the depression, the line companies began to reappraise the value of their tremendous holdings: "The practice of leasing mines to independent operators, large and small, in the years after 1929 indicates the speed with which the line companies hastened to take account of the revisions in expectations." Between 1929 and 1933 the large independents increased their share of the industry's production from 24.8 to 37.7 per cent. For the most part the important independents were new entrants: "The development of these new large independent producers is the most important element of the internal change of the industry." (*Ibid.*, pages 52, 47, 48.) As compared to earlier periods the control held by the line companies had declined substantially. By 1937 the eight line companies accounted for only about 60 to 65 per cent of total production; in 1923 their proportion had been 74 per cent[12] and in earlier years considerably higher. Thus there can be little doubt but that the increase in price flexibility in the anthracite industry was accompanied by a decrease in concentration.[13]

[12] TNEC Monograph No. 21, *op. cit.*, p. 181. These percentage figures are not strictly comparable with those presented by Pabst since, following the Anthracite Coal Industry Commission, he designates ten firms as line companies, whereas TNEC Monograph No. 21 designates only eight as line companies.

[13] Pabst advanced the argument that although (*a*) "the line companies appeared to be acting in coordination," (*b*) "they appeared to be holding their prices up and letting production fall, bringing capacity in line with their potential demand," (*c*) "their adjustment

Two decades ago, one of the liveliest issues engaging the attention of economists was price rigidity—particularly the relationship between such price behavior and the structure of industry. Largely because of the war and the subsequent abnormal sellers' markets, the issue seems to have been all but forgotten. The industry-by-industry analysis presented in this paper shows that in four major areas of the economy—farm products, foods, textiles, and lumber—prices were flexible in both depressions, as would be expected from Means's argument, since these areas were generally atomistic in both periods. Moreover, the analysis also shows increased rigidity, following the concentration of the industry, in such diverse fields as iron and steel, salmon, salt, biscuits and crackers, molasses, matches, plate glass, window glass, and anthracite (the last two in an inverse manner).

Now that the abnormal postwar sellers' markets appear to have come to an end, it may be highly appropriate for the economic profession to focus its attention again on the subject of price inflexibility. If one agrees with this observer that price rigidity during a downswing is economically injurious, the reasons for examining the factors which might contribute to such price behavior are self-evident. The results of this analysis, taken in conjunction with Means's own findings, suggest that one of those factors—and an important one at that—is economic concentration. But even if one holds that price rigidity is not harmful, further examination should be made of the manner in which such prices are set, the considerations behind their determination, the economic effects of their being imposed at alternative levels, etc. If this paper has done no more than to contribute to a reopening of interest in this whole question of depression price behavior, it has more than served its purpose.

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was the kind that might be expected from the dominant enterprises in a declining industry beating an ordered, strategic retreat," and (*d*) "the independents . . . were acting on the whole in a way to justify the term 'competitive irrationality,'" nonetheless ". . . the hypothesis that the shifting control in the industry results from differences between the adjustments of competitive and monopolistic producers, in respect to prices and costs, cannot be maintained." The reason that it cannot be maintained, he holds, is that "the individual independent operators did not step up production, but generally, from the largest to the smallest, lost in about the same degree as the line operators." What he is referring to is the behavior of those independents who were in business throughout the period. His observation does not hold true, as he recognizes, of the "new" independents, who accounted for most of the gains shown for the independent group. These firms greatly increased their output and in general followed the norms of competitive behavior.

FULL EMPLOYMENT, WAGE FLEXIBILITY, AND INFLATION[1]

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I

Considerable disagreement has existed during the years since World War II over the extent to which unions have succeeded in exercising an independent effect on wages and prices. Since unions are a form of monopoly, they are in a position to affect the supply of labor.[2] Consequently it is generally agreed that wage rates under unionism are bound to be different from what they would be under a system of unilateral wage changes granted by employers to an atomized labor force. But this measure of agreement does not carry us very far in evaluating the inflationary effect of unions. What we want to know is (1) whether or not union policy contributes to the intensity of the inflationary spiral during a period of partially or entirely deficit-financed military spending, (2) whether it will do so during a peacetime boom in the private sector of the economy, and (3) whether union policy can contribute to a creeping inflation independently of either peacetime or wartime boom spending, and in the absence of a legacy of liquid assets from a previous inflationary period. The present paper is concerned primarily with the third of these alternatives, and thus considers the impact of a rise in cost on the level of spending. Care must be taken to bear these three cases separately in mind when facing up to the question whether or not the existence of unionism injects an "inflationary bias" into the economy.

The effect of unionism on the price level is one aspect of the broader question of the impact of changes in costs on prices and sales. The four

[1] I am indebted to the Graduate School of Northwestern University for aid for research and to the Brookings Institution for its interest in a broader project of which this is a part. Allen Vander Meulen, Jr., was very helpful in the preparation of this paper.

[2] The literature is full of warnings that unless the monopoly power of labor is curtailed or unions use their power with restraint, free enterprise will be jeopardized. Cf., for example, J. M. Clark, *Alternative to Serfdom;* the papers by G. Haberler, E. Chamberlin, and J. M. Clark in D. M. Wright, *The Impact of the Union* (Harcourt, Brace, 1951); and C. E. Lindblom, *Unions and Capitalism* (Yale University Press, 1949); and the criticism by J. Dunlop, *Am. Econ. Rev.*, June, 1950. The evidence for this contention does not seem very strong. Demand inflation is responsible for much of the upsetting effect on democratic institutions that has often been attributed to the power of the unions. Labor may on occasion find itself in a strong position politically, but in a healthy democracy any tendency to make excessive use of a strategic political position ought to produce its own corrective.

links in the chain connecting changes in wage rates with changes in the demand for labor are the relation of wage rates to labor costs, of the latter to total cost, of total cost to price, and of price to sales and output. (Cf. A. M. Ross, *Trade Union Wage Policy* [University of Chicago Press, 1948], page 80.) Clearly there is great variation in the free play in these links from firm to firm and from industry to industry as well as over time.

In assessing the likelihood that unions can force the pace of inflation, the first step is to weigh the effect of a rise in costs on prices. It is accepted that if cost rises are to be reflected in prices, assuming productivity and the volume of transactions unchanged, the rise in cost must cause an increase in the quantity or the velocity of money or both. It has often been pointed out that the world's major inflations have not originated on the cost side. They have resulted either from a bank-financed boom in private investment or from government deficit spending in time of war. Subsequent further inflationary movements, after the initial burst of private or public spending, are likewise to be attributed to the demand side. The public, in drawing upon its liquid assets amassed during the boom, is merely perpetuating a demand inflation. Experience in past inflations, however, is not relevant to the question whether, under conditions of long-continued full employment, the economy would have an inflationary bias in the absence of a legacy of liquid assets from a period of bank-financed inflationary spending.[3] Much of the discussion of inflationary bias in recent years is of limited application, because it tacitly assumes some such period as the decade following World War II. Certainly we are interested in the extent to which the existence of unions and other monopolistic organizations act to accelerate price rises during such a period, particularly if circumstances should dictate years of inflationary government and business spending. But we are also interested in the role of costs—in particular, wage costs—during periods in which demand inflation is not present.

In the present context, an economy is said to have an inflationary bias if, in the absence of a demand inflation, increases in costs set in motion a train of events that lead directly or indirectly to the increases in liquid assets and spending which are necessary to permit the rise in costs to be passed on in higher prices. The bias is much more marked if the initial rise in costs and prices establishes an expectation of a still further rise. A cost rise that is limited to a particular firm or industry is likely at most to encourage a temporary increase in borrowing to meet higher working capital charges. If, however, the rise in cost is

[3] Cf. W. A. Morton, "Trade Unionism, Full Employment and Inflation," *Am. Econ. Rev.*, March, 1950, and the comment by M. Bronfenbrenner, *ibid.*, September, 1950.

widely diffused throughout the economy, firms might be prepared to increase borrowing (and banks to permit them to do so), on the assumption that a higher rate of borrowing is appropriate to a higher level of aggregate investment. Consumers likewise may respond to the rise in prices by reducing savings or by expanding installment borrowing. Whether savings are reduced or installment buying increased, the result may be an indirect rise in commercial bank lending, in the former case to businessmen who had previously been borrowing these savings and in the latter to the merchants who are enlarging their credits to customers. These responses, however, do not point in the direction of a spiral of inflation, or even of creeping inflation. Indeed they may be merely temporary, in which case the price level must fall back again to its previous level.

The probably rare appearance of a pure cost inflation in the private sector, however, does not eliminate the possibility that the reaction of the government to cost rises may be calculated to freeze higher costs into higher prices, or even to precipitate an inflationary spiral. If the government feels constrained to counteract any extensive cost-induced unemployment with work-creating fiscal measures, prices may reach equilibrium at a higher price level or may conceivably interact with costs to set up an inflationary spiral. Is there evidence that there has been such an inflationary bias in this country?

If the consequence of a rise in costs were known beforehand to be an immediate and fairly substantial decline in employment, there would seem to be no doubt that an inflationary bias could exist. For no government would dare to refrain from forestalling this unemployment by creating deficits. In practice, however, unemployment usually makes its entrance gradually. Thus a period of doubt is ordinarily experienced, during which debt-conscious administrations are likely to be hesitant to undertake what may prove to have been a premature program of deficit spending or tax reduction. If they wait very long, however, the downswing may become so pronounced that the problem would be one of avoiding deflation rather than inflation.

It seems likely that the view prevalent during and after the depression—namely, that henceforth governments must take remedial steps immediately on the appearance of "Beveridge" unemployment (fewer vacant jobs than unemployed workers)—has been significantly tempered by fears of inflation during recent years. The argument could be made that even if the effect of excessive wage demands were rather considerable unemployment, the probable response in terms of government fiscal policy is hardly likely to have significant purchasing power effects. The unemployment is obviously not "Keynesian," and thus

would not call for action under a Keynesian public works spending program. This type of unemployment is in any case difficult to remedy through aggregative fiscal measures.

Experience in the recessions of 1948-49 and 1953-54 indicates that both Democratic and Republican administrations prefer to react cautiously to moderate downturns. Other issues dominate unless unemployment rises above the figure that is generally accepted as "tolerable." But if this is so, presidents would hardly be likely to recommend immediate action against the relatively small and widely distributed amounts of unemployment that may result from an aggressive wage policy. This view receives support from both the Truman and Eisenhower recession programs. Unemployment stood at 6.0 per cent in June, 1949, as compared with 3.4 per cent in June, 1948; and consumer prices had declined by 3 per cent from the August, 1948, high. The President and Congress, however, were more concerned with the impact of tax relief on the level of the public debt than with the elimination of all vestiges of unemployment. Truman's policy was to bank on an early upturn, while merely laying plans for public works in the event of a more serious decline in business activity. It was stated in the "Midyear Economic Report" of July, 1949 (page 6) that "the kind of government action that would be called for in a serious emergency would not be appropriate now." One reason that the Truman administration was loath to adopt antidepression policies in the spring of 1949 was, of couse, the constant fear of a continuation of the inflationary rounds of wage increases that were experienced in 1948. In the absence of such a fear, the possibility does indeed exist that the appearance of even a small amount of unemployment might produce a quick response in the form of intentionally created federal deficits.

The reaction of the Eisenhower administration to the recession of 1953-54 was very similar. Here again unemployment, according to President Eisenhower's statement of August 12, 1954, had risen from 2.4 per cent in July, 1953, to 5.1 per cent in July, 1954. Prices, however, remained virtually stable during this period. The attitude of the administration toward moderate increases in unemployment has been that "the need for constant vigilance and preparedness by government does not, however, justify constant stirring or meddling. Minor variations in activity are bound to occur in a free economy. . . ." (*Economic Report* of the President, January, 1954, page 113.) Evidently both political parties are torn between a desire to avoid an increase in the national debt and the necessity of keeping public reaction toward unemployment below the political boiling point.

As the argument stands thus far, it is contended that unemployment resulting from a rise in costs is not likely to be so quickly remedied by

deficit-financed public spending or tax reductions that the economy can be said to have a cost-inflationary bias. If, however, unemployment can be eliminated by a rise in government spending without a rise in the debt, an inflationary bias may nevertheless exist. The fear of deficits as a reason for delay in adopting counteracting measures would disappear. An increase in the size of a balanced budget may be inflationary at full employment, provided the prospect is for a continuing high-level aggregate demand. A part of the additional taxes falls on savings, and it can be assumed that the firms which are denied the opportunity to borrow these diverted savings will turn to the commercial banks. Assuming Federal Reserve and commercial bank co-operation, the probable result is a rise in total public plus private spending under inelastic conditions of factor and commodity supply and an upward pressure on prices. The situation is different, however, when unemployment has already appeared or even if a downturn from full employment conditions seems imminent. Taxpayers are indeed still likely to attempt to maintain consumption levels by reducing savings; but it is now probable that at least a part of these savings would have been voluntarily relinquished by businessmen who anticipate a further downturn. Consequently the hoped-for increase in bank-financed private borrowing will not materialize, and no basis exists for the contention that in these circumstances a rise in costs would generate the additional purchasing power necessary to sustain it.

II

A rise in wage cost at full employment in the absence of a demand inflation may or may not establish the conditions for a cost inflation. It could have one of four different effects on the price level.[4] Prices might fall. They might remain at the same level as before. They might rise uniquely with the rise in wage rates. Or an inflationary wage-price spiral might be set in motion. The first of these alternatives might occur if under stable full employment conditions a wage rise suddenly occurred which caused business losses and initiated a contraction of investment, incomes, and prices. In both 1921 and 1930 union wage rates

[4] Much disagreement exists over the consequences of creeping inflation. One view is that continuously rising prices and incomes cause distortions that must relatively soon lead to a crisis and depression. Opposed to this is the contention that we must resign ourselves to a long-term upward price trend, and that a major problem is to avoid hyperinflation. The view that a persistent inflation is sure to build up gradually a resistance that will halt the upswing stresses such factors as changes in the level of productivity, the rate of new investment, the foreign balance, money market liquidity, the rate of interest, and the elasticity of price expectations. Cf. D. Copland, "The Full Employment Economy, with Special Reference to Wages Policy," *Oxford Econ. Papers*, October, 1953; G. Haberler, *op. cit.*; and W. G. Broehl, "Trade Unions and Full Employment," *So. Econ. Jour.*, July, 1953. For the view that in a full employment economy a moderate inflation might be expected for years to come, see S. Slichter, *Business Week*, October 25, 1952.

continued to rise after the downturn. (Cf. W. H. Chartener, *Wages in Deflation*, Editorial Research Reports, June 13, 1949.) The second alternative is a possibility when a consequence of the higher wage rates is either a decline in the rate of return to other factors or a reduction in costs in response to the pressure on firms to increase efficiency. The third, which has been called "static inflation," can occur when, assuming an elastic money supply, price policy is geared to changes in cost. It can also occur as a result of a conscious fiscal policy designed to retard inflation. The fourth—the inflationary spiral—could be initiated by a price rise associated with a wage rise if the increase in worker consumption spending were not offset by a decline in investment and if an interaction were set up between costs and prices. It is proposed to say a few words about the third and fourth of these cases.

The literature is well stocked with models that demonstrate the conditions under which, when wage rates rise, prices must rise by the same amount.[5] Moreover, the frequent publicity accorded in the postwar years to the quick price responses to wage increases has appeared to give empirical content to the findings of these models. Inferences from neither the models nor the empirical evidence, however, have general significance. The former are limited either by the restrictive assumptions of the static cost and demand curves of the individual firm or by those of equally restrictive aggregate economic models. Inductions from the empirical evidence, again, are usually complicated by the presence of demand inflation and are susceptible to the dangers of reasoning *post hoc, ergo propter hoc*. Many instances can be found in which price rises in response to wage increases have later been followed either by price reductions or by the omission of price rises that would otherwise have been made. The wide range of possibilities is illustrated by two examples selected almost at random from recent experience. In France, wholesale prices and wages responded quickly to the 15 per cent rise in minimum wages enacted in September, 1951. Because the rise was general and the underlying situation inflationary, increases in cost could be quickly passed forward and the entire wage-price

[5] Reference may be made *inter alia* to the treatment of wage-price-profit relationships in the *Treatise on Money* and the *General Theory*. See also A. Lerner, AEA *Papers and Proceedings*, May, 1946, p. 331, and the comment by S. Slichter, *The Challenge of Industrial Relations* (Cornell University Press, 1947), p. 79; M. W. Reder, "The Theoretical Problems of a National Wage-Price Policy," *Can. Jour. Econ. Pol. Sci.*, February, 1948; A. Flanders, *A Policy for Wages*, Fabian Tract #281, pp. 31 ff.; A. T. Peacock and W. J. L. Ryan, "Wage Claims and the Pace of Inflation," *Econ. Jour.*, June, 1953; and, with reference to Harrod's model, A. G. Pool, *Wage Policy in Relation to Industrial Fluctuations* (Macmillan, 1938), pp. 116-117. Peacock and Ryan are concerned with the cushioning effects of lags under collective bargaining and under wage escalator arrangements. For an examination of the assumptions under which a wage change will cause an equal change in prices, see T. Mayer, "The Effects of a Wage Change Upon Prices, Profits, and Employment," *Econ. Jour.*, September, 1951.

structure tended to be restored at a higher level. By contrast, because of the highly competitive nature of the American textile industry, New England textile companies in the same year were not able to pass on wage increases escalated by the rise in the consumers' price index. After the introduction of the cost-of-living escalator in 1951, textile prices dropped, some by as much as 20 per cent between the years 1950 and 1952.[6]

The immediate price response to a rise in wage rates will depend on the evaluation by the firm of the market situation, in conjunction with the pricing practices of the industry, as well as on the importance the firm may attribute to the impact on public opinion of a price rise made directly in response to a rise in wage cost. One element in public opinion is the reaction of the union, whose attitude in part reflects its opinion of the effect of higher prices on real wages and employment. Its objections, if any, are likely to have their greatest effectiveness in a delicately balanced political situation, when business may be encountering difficulty in explaining a high rate of profit. Even then firms may be able, in some industries at least, to protect themselves with labor-cost and materials-cost contracts, which permit automatic price increases with a minimum of publicity. However, it is likely to be true that in the absence of a demand inflation the passing forward of cost increases is not likely to become a political issue. The price increases are simply too unimportant.

For several reasons a rise in cost may not be followed by a rise in price. For example, if full employment has continued for some time, markets are likely to have become more favorable to buyers. Unions may, however, have the power to achieve further wage increases. The effect on investment may be negative and prices may fall. Again, if a wage bargain is settled on the expectation of a price rise, the disappointment of this expectation may mean that wage increases have been made that cannot be passed on in higher prices.[7] Gains in productivity make possible wage increases without the need for corresponding price rises.[8] It is probable that the spread of industry-wide

[6] H. C. Eastman, "The Economic Effects of the French Minimum Wage Law," *Am. Econ. Rev.*, June, 1954, pp. 375-376; company testimony, Bates Arbitration case, March 3, 1952.

[7] Although cases are rather rare in which explicit account is taken of anticipated price rises in wage agreements (an example is the Austrian 5th price and wage agreement, July, 1951; cf. Bert Zoeteweij, "The Adjustment of Wages to Changes in the Cost of Living," *International Labor Review*, July-December, 1952, p. 101), the expected course of prices is a factor which is always present in wage bargains.

[8] During the defense period, up to the spring of 1941, wage increases appear to have been more than offset by increases in productivity. (BLS Bulletin 1009, Chap. III, "Development of Wage-Price Policies," H. M. Douty.) For an attempt to test the proposition that it is difficult in competitive industries for workers to get productivity increases translated into higher wages, see A. M. Ross and W. Goldner, "Forces Affecting the Inter-industry wage Structure," *Quart. Jour. Econ.*, May, 1950.

bargaining, if characterized by pattern bargaining, would result in many wage increases that could not be followed by compensating price rises. Competition from foreign producers may prevent price markups. The conclusion seems to be warranted that the variety of circumstances in which wage and other cost increases may occur is so wide that wage and price flexibility at full employment has to be studied largely on the basis of individual cases.

In the event that a rise in wages or other costs is reflected in increased products price, the possibility exists that an inflationary spiral may be initiated. An important cushion against the spiral effect, however, is the absorption of part or all of any induced increase in the money supply by the initial rise in prices. This could result in equilibrium at some higher price level. The effect can be produced artificially by taxing the desired amount of purchasing power out of the income stream, provided allowance is made for the possible inflationary effects of the additional taxes.

Equilibrium at a higher price level would not result, however, if two or more escalators interacted rigidly (for example, a cost-of-living escalator and agricultural price parity) at each step in the process inducing the increase in the money supply required to permit an inflationary spiral. The marked increase, in recent years, of escalator devices designed to guarantee shares in the national product multiplies the chances of a cost-push inflation that is separate from the demand-pull of an inflated money and asset supply. It is quite possible that in an economy characterized by the widespread use of escalators, the monetary authorities would find it difficult to resist political pressure to adapt the money supply to the requirements of a constantly rising price level.

Yet it is questionable that the general adoption of escalators would in practice contribute significantly to a spiraling inflation. A striking feature of these devices is the readiness with which, in some foreign countries, at least, steps have been taken to soften their rigidities in the event that they threaten to produce unwanted economic effects. For example, particularly when governments participate with firms and unions in developing an escalator agreement, a number of escape clauses and limitational devices have been concocted to retard its operation in the event of the threat of an inflationary spiral, though of course by no means all have been used in any one escalator agreement. Among these devices are the restriction of escalation to the minimum wage or to the basic wage, escalation of only a percentage of the basic wage, ceilings on escalation, limitation through price controls, devices for delaying escalation, lags between the collection of

price data and the application of the consumers' price index in the escalator formula, and so on.

In addition to those limitations on escalators that are in part intended to minimize the danger of an inflationary spiral, cushioning may also occur by virtue of the heterogeneous purchasing power effects of cost-price rises. The more numerous the escalators, the more ambiguous are the possible purchasing power effects. The probability increases that they are not all in the same direction, but by partially neutralizing each other, tend to damp the upward impulse. Some escalators tend to encourage consumption, but at the same time they may cause an increase in cost that discourages investment. Other escalators may preserve profit rates at the expense of consumption. The multiplicity of escalator arrangements makes it very difficult to draw conclusions. These mechanisms range from wage cost-of-living and productivity escalators, through agricultural parity formulas and the formidable array of business price markup devices, to more novel contrivances like price-index bonds and proposals to escalate social security benefits for changes in the cost of living. In addition, there are a number of special types of rigidities which probably should not be classed as escalators, such as price leadership, horizontal wage (or pattern) escalation, equalizing tariffs, and the like. Thus we cannot say for sure that an escalated cost rise, even when supported by a co-operative monetary policy and magnified by interaction with at least some other types of escalators, will necessarily give rise to a spiral of inflation. Incidentally, it would be interesting to study the circumstances in which escalators tend to breed escalators. The more widespread the practice of escalation, the smaller the reservoir out of which the protected groups can hedge themselves against inflation and the greater the pressure on all groups to participate in the protection. Under certain assumptions, the result could be an accelerated rate of adoption. Again, rising real incomes and the increasing importance of the small saver provide fertile ground for fostering the popularity of parities and escalators.[9]

III

We come now, very modestly, to the question posed at the beginning of this paper: Has there been a tendency either to exaggerate

[9] For the Swedish plan for hedging pensioners against inflation as well as assuring them participation in productivity increments, see Alan Peacock, "Social Security and Inflation," *Rev. Econ. Stud.*, 1952-53, pp. 169-173. The Christiansen Corporation has recently offered employees an issue of cost-of-living debentures, with a ceiling of 150 and a floor at the issue price of 100. (*Management Record*, June, 1953, p. 200.) These are merely random examples of the wide variety of experiments that have been made in many countries, a number of them going back to the early twenties.

or minimize the effects of unionism on wages and prices in the propagation of a cost inflation? If the effects have been exaggerated, have they nonetheless been significant? One is struck by the difficulty of answering this question unambiguously. It is common to make specific comparisons that may have little or no general application. In individual instances it can sometimes be asserted that the direct effect of the union was either to accelerate or retard wage increases. It is more difficult to understand what this proves about the over-all impact of unionism. In arguments intended to minimize the importance of union power to influence wage decisions, the procedure has often been to consider a situation in which, if the union had been suddenly dissolved, wages would have risen more rapidly. This ignores the role of the unions in events up to that moment, and attributes no importance at all to the threat of unionism as a factor affecting wages in nonunionized industry.

An illustration of this type of thinking is the suggestion that collective bargaining may be a less effective way of achieving wage increases than is reliance on the free play of market forces.[10] It is true that unilateral wage increases can be made rapidly, while the reopening of contracts and the process of collective bargaining are more time consuming. But this is no valid basis for evaluating the general effects of unions on wages. Would the same unilateral increases have been made if unions did not exist? Again, wage increases are sometimes made in order to prevent unions from getting a foothold in the industry. Although such raises appear in the statistics on the nonunion side, they are actually attributable to the existence of unions. Another point is that firms are said to dislike to grant wage increases in organized industries because unions will resist wage reductions even if the price level subsequently falls. But, as Slichter points out, even in unorganized industries account must be taken of the effect of a wage decrease on worker morale. More generally, there is no reason to minimize the influence of unions merely because they crystallize employer opposi-

[10] Cf., for example, M. Friedman, in Wright (ed.), *The Impact of the Union*, pp. 204 ff.; A. Rees, "Postwar Wage Determination in the Basic Steel Industry," *Am. Econ. Rev.*, June, 1941; W. A. Morton, "Trade Unionism, Full Employment and Inflation," *ibid.*, March, 1950; Clark Kerr, "Labor Markets: Their Character and Consequences," AEA *Papers and Proceedings*, May, 1950, p. 228; K. Boulding, "Collective Bargaining and Fiscal Policy," AEA *Papers and Proceedings*, May, 1950, pp. 306 ff. The opposite position is taken by Haberler, *op. cit.*, pp. 34 ff.; S. Slichter, "Do the Wage-fixing Arrangements in the American Labor Market Have An Inflationary Bias?" AEA *Papers and Proceedings*, May, 1954, p. 331. For an examination of some of the statistical work done on the effects of collective bargaining (not of the institution of unionism as a whole) on wage rates, see M. Bronfenbrenner, "The Incidence of Collective Bargaining," AEA *Papers and Proceedings*, May, 1954, pp. 297-299. He concludes that the evidence, though partial and contradictory, is in favor of "some slight tendency" for union wages to lead nonunion wages, but that collective bargaining has not had any important effect on aggregate income distribution.

tion to wage increases. Still another argument is that by taking account of local conditions in bargaining, unions are often really accepting the nonunion wage level. But unions could hardly do otherwise than take account of local conditions, along with other considerations.

The fact that union leaders may pursue a go-easy policy has been used to discount the impact of the unions on wage rates. Variations in union aggressiveness have indeed often resulted in important effects on wage rates, and it is possible that union policy could be so moderate that its relative effect is deflationary. (An example that might be cited is the "New Wage Policy" adopted by the AFL when Green succeeded Gompers as president. The tying of high wages to high productivity was associated with a decline in union militancy.) If inflation proceeds at a rate in excess of what is regarded as politically tolerable, union leaders may have to be satisfied with trivial face-saving gains that do no more than preserve their power with the rank and file. In this reaction they are simply yielding, as they must, to the complex of economic and political forces which determine the degree of public support for their policies.

It has often been pointed out that a union characteristic that can contribute to the alleged inflationary bias in the American economy is the fact that labor policies are the product of the internal politics of the unions, and that immediate monetary gains are uppermost in the minds of the leaders, virtually to the exclusion of the welfare of the country as a whole.[11] The argument often goes on to state that union leaders have to bargain for the concessions that will call forth the greatest measure of support from the rank and file. This means, for example, that the employment effects of wage bargaining may be assigned little importance when the leaders are concerned only to command the approval of employed union members or when, in a declining industry, the union is trying to reduce membership. The reverse, however, could just as easily be so and will be so when the leaders have reason to believe that employment considerations are uppermost in the minds of the rank and file. The extremes are illustrated in two recent cases. In their race with the steel workers to upset the government's wage stabilization program during the period 1950-52, the United Miners matched aggressiveness with the United Steel Workers, ignoring the unemployment and short time already existing in the coal industry. On the other hand, the Massachusetts shoe industry

[11] This issue is discussed by, among others, S. Slichter, "The Government of Trade Unions," Chap. 4 in *The Challenge of Industrial Relations;* Broehl, *op. cit.;* J. Dunlop, "The Economic Model of a Trade Union," *Wage Determination under Trade Unions* (Macmillan, 1944), Chap. III; and Arthur M. Ross, *Trade Union Wage Policy* (University of California Press, 1948).

provides an instance in which a union has agreed, under the so-called "grade system," to wage reductions in the interest of reduced unemployment.[12] A more recent example occurred in the summer of 1954 at the Studebaker plant. In intermediate instances, unions have often justified an aggressive bargaining policy on the ground that the employment effects of higher wage rates could be neither predicted nor estimated.

A familiar criticism of the American labor movement is that it lags behind European unions in the appreciation of the effects of its actions on prices and on economic welfare as a whole. There is ground for believing that part of the reason for American labor's self-centered view toward economic matters is the difficulty of arguing convincingly that an aggressive union policy is more inflationary than a supine one. This difficulty is not likely to be removed until more detailed models of the determinants of income and prices have been developed than are now at hand. Another factor that is partly responsible for aggressive union activity in this country is the high productive capacity and relatively favorable international economic position of the United States, which provide a certain amount of slack in the bond between wages and prices. Despite the high level of personal income, the consumer price index has been virtually stable since the end of 1951, and a number of markets have become buyers' markets, as the growth of the merchandise discount houses testifies. The situation has been quite different abroad. In some foreign countries, where the balance-of-payments or investment programs seemed to require it, unions have been very effective in holding down wage rates despite the protests of the rank and file. In assessing these cases, it has to be borne in mind that if the unions had not shown a policy of restraint, direct controls would probably have been imposed. Therefore we cannot say surely that even in these instances the net effect of the unions on wages and prices has been negative.

An illustration of fluctuating union aggressiveness is offered by the experience of Britain. Rising living costs after World War II had led to a more aggressive union policy and in 1947 to the breakdown of the wage-stabilization policy inaugurated in 1940. Yet the unions responded favorably to the government's plea for wage stability in order to avoid worsening the balance of trade. The failure to halt the rise in the cost of living, however, resulted in another shift in union policy a few months later. On balance, over the entire period, the policies adopted by the unions played a part in restraining inflation. (Cf. J. T. McKelvey, "Trade Union Policy in Post-war Britain,"

[12] S. Slichter, "Do the Wage-fixing Arrangements in the American Labor Market Have an Inflationary Bias?" AEA *Papers and Proceedings*, May, 1954, p. 342; J. P. Shultz and C. A. Myers, "Union Wage Decisions and Employment," *Am. Econ. Rev.*, June, 1950, pp. 364 ff.

Industrial and Labor Relations Review, October, 1952.) In Germany, too, the unions have for many years supported a policy of low wages in the interest of both the export market and a high rate of capital formation. Although (as the strikes of August, 1954, have indicated) the attitude of the unions necessarily stiffens after many years of abnormally low wages, the high proportion of Germany's national income that has been saved and invested in recent years is in considerable part testimony to the modest demands of the trade-unions.

Many statistical comparisons between union and nonunion wage rates have been made in an effort to draw general conclusions on the impact of the unions on wages and prices. Unfortunately they may suffer from the fact that union and nonunion wages are interdependent. Partial studies, restricted to a particular industry or period of time, cannot tell us how high wage rates would have been if labor had never been organized or what they would be if labor policy were integrated under a unified labor movement.

There is, to recapitulate, one set of circumstances in which creeping inflation, which might develop into an inflationary spiral, would be definitely attributable to the power of the unions. At full employment, even in the absence of a demand inflation, unions would be strong; and widespread, aggressive wage demands could be responsible for adversely affecting cost-price relationships to the point at which unemployment developed. If the unemployment were very substantial and if the government responded with deficit-financed re-employment measures, full employment might be preserved at the cost of a constantly rising wage-price level. But experience under neither Democratic nor Republican administrations provides a basis for a fear that this is likely to occur. The opinion is ventured here that even if a labor-oriented administration were to adopt this policy, long before the inflationary stage was reached the opposition of nonlabor political elements would have forced its surrender.

IV

Whatever doubts may be raised over the likelihood of a pure wage-cost inflation, past experience with demand-cost spirals indicates that under certain circumstances aggressive union policy can be expected to play a part in the chain of events that contribute to inflation under full employment conditions. A number of proposals have been made to eliminate this inflationary effect of unions. In general, however, they are unsatisfactory, for either they do not hold forth much promise of success in the foreseeable future or they threaten to produce undesirable economic or political effects. One suggestion is to protect labor through cost-of-living subsidies, avoiding inflation by using progressive taxation to prevent effective demand from outrunning supply.

Apart from the narrow limits to the further use of redistributive taxation and the possibility that investment and output in the future would be adversely affected, this plan does not provide unions and union leaders a reason for existing. Many economists believe that in a free enterprise economy, unions are an indispensable element in the multilateral monopoly struggle for shares in the national product.

Another suggestion is to control prices, leaving it to the employers to deny the wage increases that would eat into profits. (Cf. Gösta Rehn, in Ralph Turvey, editor, *Wages Policy Under Full Employment* [Hodge, 1952], page 49.) Why should the business community give political support to such a suggestion, unless employer resistance were backed by a far severer Taft-Hartley than advocates of the proposal would probably be prepared to countenance? A milder version is for the public in some sense to "reserve its approval" for the employers who oppose excessive wage claims. This proposal has no operational usefulness. It would be better to permit the public to act in the only way it can; namely, by turning away from labor at the polls. Some proposals make their appeal to the union itself: learn to use monopoly power with restraint and thus avoid government control; do not make the union leader too dependent on the favor of the rank and file and thus grant him a measure of freedom for statesmanship; or deposit part of the responsibility for wage policy in the hands of the government. In cases of national emergency, to be sure, an appeal can be made to the union on the basis of public interest; for example, acceptance of a policy of wage and price restraint in order to avoid devaluation or of a low wage policy to encourage a rapid rate of business saving and investment. The general criticism of these suggestions is that except during an emergency they are difficult to implement. Where should the line be drawn? If union leaders are too independent of the membership, the spirit goes out of the unions; and if too dependent, a highly aggressive wage policy may be unavoidable. And what record is there of a power group imposing restraints on itself?

More novel, and probably not very seriously meant, proposals are such devices as: a series of sudden price deflations, engineered in order periodically to get down the whole wage-price structure without the discouragement to investment and consumption that is associated with a slow deflation; the stabilization of the prices of basic commodities by the commodity money plan; and the wage-money plan. (See Boulding, *op. cit.*, pages 318-319.) Still another expedient would be to immobilize a part of increases in wages through some form of profit sharing, thus increasing the proportion of national income devoted to saving and investment. A. Vermeulen ("Collective Profit Sharing," *International Labor Review*, June, 1953) offers a plan for restraining

labor costs in the interest of long-term investment acceleration, while avoiding the increasing inequality of wealth which he believes is ordinarily associated with unaggressive union policy.

V

Because of the rather limited prospects for the acceptance and fruitfulness of any of the above proposals as a permanent defense against cost inflation, it might be worth while for economists to take a second look at the circumstances in which a prolonged creeping inflation, not attributable to demand, is possible or probable. Observers are divided on this point; but there is little in our past experience that leads one to suppose that in the absence of wars, prices would be likely to rise indefinitely. If, however, prices are not likely to do so, the case is weakened for taking seriously any of the doubtfully viable proposals that have been made to soften the wage demands of unions.

Again, there is need for a greater measure of agreement on what is meant by the "limits of tolerable inflation." The concept has no precise meaning, either economically or politically. Within these limits, such inflation as might occur would, by definition, not be worth combating. At the same time the undesirability of an excessive amount of creeping inflation is beyond dispute. The evaluation of alternative meanings of the concept is outside the scope of this paper, but it is suggested that no meaningful definition is possible without allowing for the institutional changes that can be expected to occur as a result of a gradual inflation. The limits of tolerable inflation tend to be extended by the manufacture of devices for hedging various groups against a secular price rise. Thus the argument that bondholders, for example, would resist a gradual expropriation by a fall in the purchasing power of money has to be evaluated in the light of the possibility of escalating debt obligations with a cost-of-living index. Similar defenses might be erected to protect insurance policy holders, social security beneficiaries, and persons with relatively rigid earned incomes. However unlikely the general adoption of these devices in this country may appear on the basis of past experience, the picture might alter considerably if we were confronted with continuous inflation. Quite a different view is that the tremendous growth in fixed claims in recent years, when projected into the future, may strengthen inflationary sentiment among the active earning segments of the population, as well as on the part of those politicians who are concerned to bring about a rise in the proportion of national income taken by government factor purchase expenditures. In a society in which some groups favor inflation and others oppose it, the outstanding characteristic of the limits of tolerable inflation would appear to be their variability.

DISCUSSION

GIDEON ROSENBLUTH: Mr. Blair's revival of an old controversy is most timely. With the decline of agricultural prices in recent years and the appearance of "rolling readjustment" in business conditions generally, there is renewed public interest in the apparent rigidity of certain prices. The paper is also timely in terms of the history of economic thought. Gardiner Means argued in the thirties that concentration of market control had increased, that price rigidity had increased, and that the latter trend was due to the former. Critics soon produced evidence to show that price rigidity had not increased since the nineties or even earlier times, and more recent studies have suggested that concentration has not increased in the last fifty years. Surely the time was ripe for someone to point out that the denial of the first two parts of Means's thesis could be construed as support for the third. If neither concentration nor price rigidity had increased, the two might well be related.

Blair's method is novel and ingenious, and he has given us a number of most interesting case studies. Information, both qualitative and quantitative, from a great variety of sources, has been skillfully combined and brought to bear on the difficult question of the trend in concentration. Of particular interest is the discussion of the degree of market control in the period preceding the great merger movement—an area still largely unexplored.

Can we regard Blair's findings as establishing that concentration is a significant factor making for price rigidity in the American economy? To answer this question I should like to discuss briefly the interpretation of the findings and the nature of the sample.

The essence of Blair's new approach is described in his paper as follows: ". . . contrasting a product's price behavior in two depressions—before and after the industry had become highly concentrated."

This method is applied directly to only two industries: steel and plate glass. Other industries are discussed, either in order to provide a backdrop (farm products, food, textiles, and lumber) or to provide a contrast (copper, lead, zinc, window glass, anthracite). It is doubtful whether results based on a sample of two industries can safely be generalized. Moreover, the interpretation of the results for steel and plate glass is not beyond question. The steel industry is well known for discrepancies between quoted prices and actual terms of sale, and Nelson found evidence of a similar discrepancy (though probably less important) for plate glass (Appendix 1 of the National Resources Committee study, *The Structure of the American Economy*, 1939, pages 182, 183). Blair presents evidence to show that secret discounts were not unknown in the earlier depression. But if discounts are important and variable, the quoted prices used by the Bureau of Labor Statistics are not a reliable guide, and the fact that we do not know whether discounts were more important in the second depression than in the first surely cannot increase our confidence in the reliability of the price data.

The price behavior of plate glass suggests an additional difficulty. It is common practice to measure price flexibility, as Blair does, by the price decline between two reference dates common to all the price series studied, and not by the decline in each price between its own specific peak and trough. This must mean that a spurious impression of rigidity is created for those series that do not closely coincide with the general business cycle in their fluctuations. Surely a price should not be called rigid merely because the market forces that cause it to fluctuate do not exactly coincide in their timing with those influencing other prices. Plate glass would appear to be a case in point, since the two price indexes which declined by only 4.3 per cent and 4.8 per cent, respectively, between 1929 and 1933, fell by as much as 30 per cent and 32.4 per cent between 1929 and 1935.

It may be argued that this is too pessimistic a view of the general significance of the findings. The number of industries in which increasing concentration is found to be associated with increasing rigidity is, after all, in fact greater than two. In addition to steel and plate glass, there are canned salmon, salt, soda crackers, molasses, and matches—all of which are discussed in some detail. But these five industries were selected by a process that involves some bias. They were singled out for special study because they displayed increasing rigidity, in contrast to the majority of industries in the foods and lumber groups. If all the industries in these groups (and in the textiles and farm products groups) had been examined with equal care, one might have found a considerable number with increasing concentration and constant or decreasing rigidity. There are seventy-five products in the study with constant or decreasing price rigidity for which concentration change was not specifically examined. One cannot simply assume that none of them would show increasing market control on closer study.

It may be further argued that acceptance of the hypothesis that concentration and price rigidity are related should not be made to depend on cases of increasing concentration alone. Are there not cases in which decreasing market control is associated with decreasing price rigidity?

Evidence is presented suggesting decreasing market control in two industries: window glass and anthracite. These two industries also display a greater price decline in the second depression than in the first. But, as pointed out by Blair, we cannot regard this as evidence of decreasing rigidity, owing to the greater severity of the second depression. We have, in fact, no way of distinguishing between cases of constant price rigidity and cases of decreasing price rigidity.

The great majority of the industries included in the study have constant or decreasing price rigidity and the author suggests that there has been no significant change in concentration. (This applies to the farm products, foods, textiles, and lumber groups, as well as to the copper, lead, and zinc products.) These findings are of course consistent with the hypothesis that market control leads to price rigidity. But they are equally consistent with the hypothesis that market control has nothing to do with price rigidity, and that neither of them changes much over time.

I have said enough to suggest that the findings of this paper cannot be

generalized unless a larger number of industries can be found in which decreasing or increasing concentration and price rigidity can be clearly identified and the association between them studied. There is room for further research.

If further investigation confirms the existence of a correlation between increasing concentration and increasing price rigidity, other possible influences on the amplitude of price fluctuations would have to be considered in order to determine whether there is in fact a net influence of concentration on rigidity when other factors are held constant. For example, the studies that have demonstrated a close relation between price rigidity and rigidity of direct costs cannot be ignored (e.g., A. C. Neal's study, *Industrial Concentration and Price Inflexibility*, 1941). Hence the movement of direct costs in 1890-97 and 1929-32 ought to be compared in the industries under study. I shall consider some other factors later in this discussion.

There would perhaps be less resistance on the part of many economists to the acceptance of the idea that concentration leads to price rigidity if they were given a good theory to explain why this should be so. It is true, of course, that price rigidity would not exist under conditions approximating perfect competition. At least this is true if rigidity of price is measured in relation to average costs. Hence some degree of market control is necessary for price rigidity. But what has not been shown is why those possessing market control should find price rigidity desirable.

There have of course been attempts at such a demonstration. One old favorite, the kinky oligopoly demand curve, has, I think, been adequately dealt with by Stigler in his article in the *Journal of Political Economy* in 1947. Another is the idea that monopolists dislike the unfavorable publicity that would result from a rise in their selling prices, and do not lower prices in depression for fear of having to raise them again. This hardly seems plausible when one considers the great volume of unfavorable publicity accorded the alleged price rigidity of products made by large corporations in the thirties.

I find it more reasonable to believe that some of the conditions that make price rigidity desirable (i.e., profitable) have nothing to do with market control (though of course firms can only adjust to them if they have some market control) while others, which are related to market control, apply only to a limited number of cases. Hence one would expect the correlation between concentration and rigidity of price in relation to cost to be significant but very weak. This is in fact what A. C. Neal's careful study shows and is consistent with Means's findings and those of the present paper.

Among the first group of conditions, I think the desire to minimize cyclical fluctuations of demand is important. This would help to explain not only actual cases of price rigidity but also the many cases in which rigidity of the quoted price is combined with flexibility in the actual terms of sale (by varying discounts and allowances, credit terms, freight absorption, etc.). The effect of price changes on expectations has often been discussed. If a price reduction, made in response to falling demand, leads to the expectation by buyers of further price adjustments, demand will fall even more rapidly.

Where fluctuating demand imposes heavy costs on the firm (because of heavy overhead and difficulty or cost of producing for inventory), both upward and downward price rigidity may therefore be desirable. Even more desirable may be the policy of keeping quoted prices rigid while varying the actual terms of sale. The rigid quoted price serves to influence buyers' notions of what is normal and to discourage the expectations of further concessions, while changes in the terms of sale accomplish the static adjustment to changing demand.

Empirical studies of price rigidity suggest that this concern with buyers' expectations may be an important factor. Very strong contrasts are found between the rigid prices of durable goods and the flexible prices of nondurable goods. Similarly the rigid prices of capital goods contrast with the flexible prices of consumer goods and producers' materials. (See, for example, F. C. Mills, *Price-Quantity Interactions in Business Cycles* [New York, 1946], pages 46, 47, 54.) Purchases of durable goods, such as capital goods, can be postponed, so that the buyers' price expectations are important, and the cyclical variability on the demand for these products is well known.

Neal has suggested that the sellers' expectations as to future market conditions may promote price rigidity by raising user cost in depression. Since user cost is a more important factor in capital intensive industries, it may be worth while to investigate whether capital intensity is correlated with price rigidity. Perhaps part of the weak correlation between concentration and price rigidity simply reflects the fact that both are related to capital intensity.

Among the second group of conditions, the cases where market control involves collusion have been frequently pointed out. Every revision of a price agreement opens the door to controversy and thus imperils the continuance of the agreement. Hence frequent revisions are avoided. Collusion is, however, probably not typical of situations of market control in the American economy today.

Mr. Blair deserves our thanks for reopening this important controversy, which has never been satisfactorily settled, and for suggesting promising new lines of research.

FRANK C. PIERSON: The paper presented by Mr. Poole, to which I will confine my attention, steers a middle course in an area where extreme positions were once the order of the day. He makes a three-way distinction between inflation in periods of deficit-financed military spending, in peacetime booms in the private sector, and in periods of creeping inflation independent of either peacetime or wartime boom spending, which I find both helpful and dangerous —helpful in avoiding the tendency to generalize too broadly from the special circumstances surrounding such severe inflations as 1946-48, but dangerous in suggesting that long-run inflation can be anything other than the net result of a series of short-run situations. Thus, any long-term inflationary bias which union wage policy may impart to the economy cannot be disassociated from the role which unions play in different phases of particular, presumably successive, cyclical periods.

The fact that, currently, the subject of union wage policy as a cause of

inflation may strike some as rather quaint, if not downright quixotic, is symptomatic of the rapidly changing world in which we live. Even the more optimistic projections foretell a further rise in unemployment in 1955 over 1954, despite an expected increase in real national product. Similarly, discussions of longer range prospects are giving increasing attention to the problem of structural unemployment, suggesting that long-term growth influences in the private sector may well not be strong enough to keep the economy close to its full employment potential. In this kind of environment, the question of union wage policy as a cause of inflation becomes something less than the burning issue of the hour.

Reference to the current position and longer term prospects of the economy underscores the importance of distinguishing between different types of full employment or high-employment situations in analyzing possible inflationary effects of union wage policies. If it is assumed that the economy, in a single leap as it were, suddenly achieves full employment, that labor and physical resources are literally fully utilized or even overstretched, and that supply inelasticities are much in evidence, analysis of the role played by unions leads to one set of answers. If, on the other hand, the economy is assumed to be moving rather gradually towards full employment and that severe labor and other resource scarcities are not developing, a quite different set of answers is obtained.

Economists—quite rightly, in my judgment—have given increasing attention to the possibility of inflation developing under conditions of less than full employment. The general rationale of this view is that wages and prices may rise rather quickly in conjunction with relatively small increases in aggregate demand and employment. (The 1933-37 period is sometimes cited as a case in point.) The converse of this proposition also deserves attention; namely, that despite the continuation of generally prosperous conditions, small increases in unemployment are quite likely to occur and when they do, they quickly lead to a slackening off, if not complete cessation, of inflationary influences. Speaking particularly of union wage policy, this is one of the main lessons to be derived from the last two recessions, 1948-49 and 1953-54. In both periods, wage increase pressures attributable to unions declined markedly and the increase in the general level of money wage rates, even allowing for gains in fringe benefits, was hardly more than the oft-cited 2 or 3 per cent average annual increase in physical output per man-hour.

The other side of the coin is the long-run effect on wages and prices which can be attributed to union policy during successive contraction periods. Since it seems clear that under collective bargaining wage rates are less likely to fall in periods of declining demand than under conditions of nonunionism and, therefore, that any rise in wage rates will start from a higher base than would otherwise obtain, the effect of union wage policy over the long run is to make prices generally higher than they would be in a world without trade-unions. Indeed, it is in periods of falling demand or of continuing general unemployment that union policy doubtless has its greatest effect on wage rates and prices. As already noted, in the 1948-49 and 1953-54 contractions, the level of money wage rates was not only maintained but was even increased some-

what. Isolated cuts occurred, but only as last-resort measures to cope with extreme situations. Lacking collective bargaining, there can be no doubt but what wage reductions, and hence price reductions, would have been more widespread in these two periods. If account were taken of all the other price-sustaining influences operating in periods of declining business activity, the possibility of any significant drop in the general level of wages and prices seems remote indeed.

For a variety of reasons which need not to be enumerated here, most economists would not recommend widespread wage cutting as a way of dealing with cyclical declines. I share this view and presumably Mr. Poole does too. But are we prepared to deal with the consequences of the alternative policy; namely, that wages and prices will follow an upward trend over time, rising in expansions considerably more than they fall in contractions? Mr. Poole notes that in both the 1948-49 and 1953-54 recessions the government was slow to resort to deficit financing as a way of dealing with the decline in business, suggesting that any inflationary pressures stemming from this quarter need not be taken too seriously. As a bit of contrary evidence, he might well have mentioned the fact that in the 1954 Congressional elections the Democrats were on the whole singularly successful in exploiting the small increase in unemployment which had occurred in the immediately preceding period. From the viewpoint of long-run relationships, however, the point deserving greatest emphasis is that the general level of wages and prices showed little tendency to decline in either of these two contractions. With wage and price cuts removed as a way of coping with business recessions, the long-term trend in the money-price level can hardly be anything else but up.

This makes Mr. Poole's analysis of union wage pressures in periods of expansion or high employment doubly important. If unions not only prevent money wage rates from falling in contractions but also push them up markedly in expansions, the long-term prospect for the value of the dollar would seem to be decidedly dark. Again, Mr. Poole is inclined to take an optimistic view, arguing there is no convincing evidence that unions exert an independent influence on wage rates in expansions and that a rise in wage rates need not necessarily mean a corresponding rise in the total quantity and/or velocity of money. Those who take issue with these two propositions have to go pretty far afield, citing political or other indirect influences exerted by unions of an equally vague or tenuous nature to buttress their argument. Compared to such factors as heavy replacement needs, large accumulations of liquid assets, bursts of investment spending, consumer buying on credit, and the like, union wage policy can hardly be more than a minor element in peacetime inflations. This being so, proposals designed to hamstring unions on the grounds they are a major influence making for inflation in expansions would seem to fall far short of the mark.

The foregoing suggests that the only significant long-run inflationary influence that can be properly attributed to unions results from their efforts to hold up wage rates in periods of decline or depression. The inflationary effects of union wage policies in periods of expansion appear to be relatively minor and it is in these periods, of course, that inflationary pressures are most active and

of greatest consequence. This is not to argue that unions can or should escape the effects of measures taken to ward off or control inflationary developments in periods of cyclical or longer term expansion. On the contrary, if a vigorous counterinflation program is undertaken by the monetary and fiscal authorities, the unions are likely to be among the first groups to feel its repercussions in terms of increasing job insecurity, employer resistances, and antistrike sentiment among the membership. Nor would many argue that the unions should be immune from the consequences of policies required to preserve general economic stability, especially since the unions' continued existence as free institutions clearly depends on the successful execution of such policies. What is important is to keep inflation control measures on as broad and impersonal a basis as possible, with all groups carrying their share of the burden and no one group benefiting from the weaknesses or difficulties of any other. A program to check inflation by singling out a particular group for punitive action is hard enough to defend in terms of social equities, but it is even more questionable in terms of effective economic policy.

PRICING IN TRANSPORTATION AND PUBLIC UTILITIES

SOME IMPLICATIONS OF MARGINAL COST PRICING FOR PUBLIC UTILITIES

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As a preface to a discussion of the role of marginal cost pricing, it is perhaps well to state explicitly that in common with any other theoretical principle, the principle of marginal cost pricing is not in practice to be followed absolutely and at all events, but is a principle that is to be followed insofar as this is compatible with other desirable objectives, and from which deviations of greater or lesser magnitude are to be desired when conflicting objectives are considered. On the other hand, I propose to maintain that marginal cost must play a major and even a dominant role in the elaboration of any scheme of rates or prices that seriously pretends to have as a major motive the efficient utilization of available resources and facilities.

Some of the conflicting considerations may be mentioned briefly. On a mere mechanical level, there is always the cost involved in the determination, publication, and administration of a rate structure. The relative importance of this consideration obviously declines as the value of the unit of sale becomes larger, so that, for example, one could expect that more refinement would be justifiable in the case of overseas airline fares than for short-haul train trips. Ability of the consumer to respond intelligently is also a limiting factor of a somewhat similar force. Thus a service used frequently by the same individual, such as local transit, will bear relatively more complexity than a service used rarely, such as parcel checking. And services used primarily by business firms, whose choices among alternatives are in the hands of relatively expert specialists, will also bear more complexity. Thus freight tariffs can stand being fairly complex, though this is by no means a justification of the present jumble.

Another consideration that may in some cases operate to require modification of pricing policies based on marginal cost is the desirability, other things being equal, of minimizing inequalities in income. In an ideal world where there existed an ideal income tax capable of producing any desired distribution of income without administrative expense or deleterious effects on incentives, it would be possible to leave

to this measure all redistributions of income, and determine rates on the basis of securing the best possible allocation of resources. In practice, however, such measures of redistribution always have, at the margin, some deleterious effects, so that if modifications of the pricing scheme can be shown to have significant effects on the distribution of income, it will in general be desirable to depart at least to some extent from the strict marginal cost price system. For provided that the departure is small enough, it will in general be possible to provide for some improvement in the distribution of income with a cost in terms of impairment of incentives and misallocation of resources that is less than if the same improvement had been achieved through additional direct redistributive measures, such as increased income taxation. In many cases, however, the relation of the pricing structure of a particular utility service to the distribution of income will be slight or uncertain; in which case the distributional considerations will be correspondingly irrelevant. It is indeed likely that in most cases the magnitude of the modification that would be appropriate on this basis would be small. A major exception may be in the pricing of local transit services, where the incidence of a fare increase, regarded as a tax, is far more regressive than any substantial revenue for which it might be considered a substitute. This consideration would argue for pushing transit fares below the level that would otherwise be appropriate and even, possibly, below marginal cost.

Another point at which prices based on marginal cost may require modification in practice is where such prices come into too violent conflict with popular notions of equity. For example, it is clear that on marginal cost principles, transit fares should be substantially higher during rush hours than during off-peak hours. Yet a proposal of this sort is likely to be considered inequitable by many if not most of the lay population on such grounds as that rush hour riding is less comfortable, is more of a necessity, is more heavily concentrated among low-income groups or at least among working people; or is, according to some naïve method of cost allocation (as by first computing a cost per vehicle-mile and then dividing this by the average load at various times), less costly. Some of these notions of equity may be considered to be valid in their own right, as the above consideration for the relative income level of the rush hour riders would be if the facts as to the income distribution of riders actually bore out the assumption. Other notions of equity may be so strongly held as to require, as a matter of practical politics, some consideration in the design of rate structures if the proposals are to succeed. But while the economist may have a role to play in the design of such compromise measures, these compromises should in most cases be regarded as inferior solutions to be

superseded by better ones as soon as public opinion can be educated to a more rational view.

Other considerations of a general nature, such as the desirability of promoting decentralization, health, intersectional and international enlightenment, and so on, may have a bearing on the selection of rate structures. But the modifications that might be justifiable on such grounds are in many cases minor and of an uncertain general direction, and as very little of the opposition to marginal cost pricing stems from such considerations, I will leave them with this brief acknowledgment.

By far the most important of the considerations that conflict with the strict application of marginal cost pricing is the need for revenues. Many of the more extreme advocates of marginal cost pricing for decreasing-cost industries seem tacitly to assume that the government has some perfectly costless and neutral source of revenue that is capable of very substantial expansion without ill effects. Such a state might be approached, for example, if we had an income tax free of its multiple defects, evasion proof, with no marginal costs of administration or compliance, and including in its base not only money income but all forms of direct income in kind, including an imputed value for leisure. Needless to say, this is far from the case. A convenient way to express the degree of departure from the ideal, for present purposes, is to estimate a marginal cost of public funds, which will be a percentage indicating the added administrative costs, compliance costs, and indirect losses through the misallocation of resources and the distortion of incentives that will result from increasing the level of public receipts. Such a cost will of course vary from one governmental unit to another. Purely as an offhand indication of the orders of magnitude involved, one may hazard the guess that in small units with a good community spirit or in large units operating with a good civil service and efficient administration, under favorable conditions and with low over-all tax levels, it is conceivable that the marginal cost of public funds might get below 10 per cent, while under unfavorable circumstances, say in a corruptly administered medium-to-large city, excessively circumscribed in its taxing powers, the figure might well exceed 50 per cent.

Obviously, it will not pay, on any concept of the public welfare, to carry a marginal cost pricing policy all the way to the limit, so that the last dollar of subsidy barely yields a dollar's worth of benefits to the using public, when the securing of this dollar for the subsidy imposes burdens of more than a dollar. In the simple case where all demands are perfectly independent, the indicated solution is to have all prices exceed marginal cost by a percentage equal to the marginal cost of public funds divided by the elasticity of the demand controlled

by each particular price. Another way of arriving at substantially the same result is to consider prices made up of a base price at marginal cost plus an excise tax. Such excises on public utility rates would receive special consideration as possibly involving lower administrative costs, in the case where their imposition would take the form of the removal of a subsidy that might otherwise be indicated, or where administrative costs would be reduced by reason of the public ownership of the utility. But other than this there would be no justification for higher excise taxes on utility services than on other goods and services.

There is, however, very little to be said for the shibboleth that the rates should be above marginal cost by an amount just sufficient to produce revenues just covering average cost, however this may be defined. To be sure, there are often political and administrative advantages to having operations of a public utility carried on by independent agencies, whether private or public, rather than as governmental departments on a par with other nonrevenue producing agencies of government. It is often argued that this can only be done if the agencies are financially self-supporting. But to me this would be taking a far too defeatist attitude towards our ability to develop appropriate political institutions. Surely the fact that excise taxes are levied on a number of particular products, such as automobiles, gasoline, photographic equipment, and the like, will not be held to have seriously impaired the independence and efficiency of management of these industries, nor, conversely, if for good and sufficient reasons the taxes should be replaced by corresponding subsidies, would one expect the performance to be notably worse. To be sure, abuses occur in the distribution of subsidies, but except where the basis of the subsidy is ill-conceived, these abuses are but the obverse of tax evasion that occurs with excise taxes.

Similarly, if an independent public authority, instead of being required merely to meet its own expenses, is in addition required to turn over to the appropriate governmental unit a stipulated percentage of its gross receipts, either as an explicit excise tax or as an "in lieu" payment, I do not think anyone would imagine that this in and of itself would operate to impair the efficiency and independence of the agency, provided only that the amount is computed in a suitable manner and that the rates and methods of computation are fixed on a reasonably permanent basis. There seems to be no inherent reason why a subsidy cannot be handled in the same manner. There would, to be sure, be a somewhat greater temptation to attach conditions to the subsidy and thus open the door to interference with the independence of the management, and individuals may differ as to the extent to which and the occasions on which this would be desirable. But there is no necessity for this, and if independence is indeed considered a sufficiently impor-

tant objective, there should be no great difficulty in specifying the manner of payment of the subsidy in such a way as to slam the door on such political interference. There need be no greater political bickering over the level of such a subsidy than over the rate of an excise tax.

This is not to say, however, that there are no impediments to the pursuit of a thoroughgoing marginal cost pricing policy by independent agencies. In particular, attempts to adjust prices to variations in marginal cost produced by the installation of large units of capacity or by unforeseen scarcity or surplus may, in the case of a public agency, produce financial difficulties of a sort that may well give rise to demands for revision of the subsidy provisions that would be too strong to resist. Even so, if this breakdown of independence could be resisted and the substantial fluctuations in earnings weathered by appropriate financial measures such as borrowing or the accumulation of reserves, such fluctuations would yield better resource utilization than a policy of keeping earnings as even as possible from year to year.

In the case of a regulated private utility, the incentives that would result from such a short-run marginal cost pricing policy for delaying expansion so as to raise marginal costs and with them the rates that would be warranted and consequently profits, would produce additional difficulties. If to prevent this the regulatory agency is empowered to dictate the installation of capacity, little discretion indeed would be left to private management, even if regulation could be kept from breaking down under the added administrative burden. Thus whether the agency be public or private, it does appear that it would be difficult to preserve political independence and still provide for the variations of rates in accordance with short-run excess or deficiency of capacity.

But whatever the conclusion as to the desirability of actual payment of subsidies, there can be little question that to require utility users to bear tax burdens in addition to paying the entire average costs is undesirable. The situation is bad enough when utilities must pay income taxes, property taxes, and other general taxes on the same basis as other industries. To impose in addition special discriminatory taxes and excises is completely unjustifiable under any but war emergency conditions. A first step in the direction of marginal cost pricing would be the removal of all such discriminatory tax burdens, such as the federal excises on transportation and telephone service. A second step would be the exemption of public utilities from the payment of such general taxes as can readily be abated without creating difficult administrative problems. This would include all in-lieu taxes, as well as property taxes on property improvements and movables specifically designed and used for utility service, and possibly taxes on land as well, though where property such as land is convertible to other uses,

its exemption when normally devoted to utility uses might give rise to incentives for artificially extending the utility operation to cover an unnecessarily large area of land for the sake of extending the scope of such an exemption. Exemption from income taxes raises rather troublesome issues of allocation and accounting where utility companies carry on nonutility activities or otherwise compete directly in subsidiary operations with nonutility companies. But in general there is room for a very substantial step in the direction of marginal cost pricing through tax adjustments without getting involved in outright subsidy. Of course, such a program need not be carried out all the way to the limits suggested by the above considerations if for other reasons it is found desirable to keep prices further above marginal cost than this would make possible. Moreover, this argument would not preclude special taxation of utilities exploiting a scarce natural resource such as water power or natural gas, as a means of retaining for public use the pure economic rent arising in such situations.

Another argument that is often made for avoiding subsidies is that only by requiring customers in the aggregate to pay the full cost of the service can it be made absolutely certain that the service was worth providing. To do this, however, would be somewhat analogous to a playgoer who always rushes home before the end of the last act in order to see whether the TV show that he missed by going to the theater seemed better or worse than the play. Destructive testing is occasionally necessary, but to adopt a policy of applying an injurious test in every case is surely wasteful. Even if only those services are provided that could, it is estimated, pay for themselves, it will ordinarily be far better to operate such services at marginal cost. Even so, policy would err on the conservative side and many worth-while services would fail to be provided because they could not be made to pay for themselves in this way. There is no point in aiming at the wrong target merely because it is easier to tell if you hit it. Moreover, in practice, decisions as to whether to provide a service must be made fairly far in advance with a more or less permanent commitment of capital involved; so that, in practice, whether a service can be made to pay for itself must be estimated in advance and these estimates are almost as uncertain as estimates of whether the service is worth while on an over-all basis. Even if a service cannot pay for itself, this is no indication that it is not worth while. If a mistake is made in such forecasting, marginal cost pricing at least minimizes the resulting waste and salvages what can be retrieved, while an attempt to recover the full costs as nearly as may be often compounds the original error by allowing excess capacity provided in error to remain unused.

Another important consideration to be kept in mind is that marginal

cost pricing will not be adopted everywhere simultaneously, and where closely competing services continue to be rendered at rates substantially above marginal cost, reduction of the rates under consideration to marginal cost might well produce a poorer allocation of resources than somewhat higher rates. Indeed, if the only consideration were the allocation of a given demand between two closely competitive forms of service, the proper price for the service under consideration would be one that makes the differential between the prices of the two services reflect the differential in marginal cost, at least where the application of this principle leads to a unique result. It should be noted that it is an equal absolute differential and not an equal percentage relationship that is sought. This may cause difficulty where the units are not directly comparable, especially where different consumers would have different substitution ratios and discrimination between them is not feasible. For example, if air express rates are to be set so as to preserve the proper cost differential over established surface handling charges, some allowance should properly be made for the lighter packaging that would often be practiced for air shipment, and this allowance might vary considerably from customer to customer.

There may also be several competing services, differing in the degree to which their rates exceed marginal cost. In which case some sort of compromise will be necessary, with the influence of each competing service weighted according to the appropriate cross-elasticity of demand. For example, if rates for water transportation are to be set, it may not be possible to do this in such a way as to cause the rate differentials to agree with the marginal cost differentials for both rail and truck shipments, if the rates for these latter are not to be disturbed. Of course, where a single agency, such as the ICC, has control over the rates of all of the competing services, all of the rates can be adjusted together to a common margin above marginal costs and there is no difficulty in preserving the competitive relationships among the various services whatever the general level of rates decided upon, except possibly where it is necessary to distort the relationship in order to give the agency providing each service a fair return and no more on its investment. In such a case the problem might better be met by the imposition of appropriate discriminatory taxation on one or more of the operating agencies so as to bring their rates into an economical relationship with the highest rather than to permit traffic to be uneconomically diverted because of differing relationships between the marginal and average cost of the several competing services.

Of course, in a general sense every service competes with all other commodities and services for purchasing power and for the use of resources, and it is sometimes claimed that, since imperfect and monopo-

listic competition prevail fairly widely in the private sector of the economy, producing prices considerably above marginal cost on the average, therefore public utility prices should likewise be above marginal cost by a like percentage. This argument must be used with caution, however. At most, the presence in the economy of a substantial sector that appears to be genuinely competitive in the relevant sense would bring the average discrepancy between marginal cost and price down to a more moderate figure than would be thought of if attention is focused primarily on the imperfectly competitive sectors. Moreover, in applying such an argument it must not be forgotten that self-produced commodities and services, not produced through the market, also compete in this way. Such nonmarket production, including the utilization of leisure time, constitutes a very large area where there is no market friction, monopoly behavior, or taxation to drive a wedge between the marginal cost and the marginal value to the individual. Services that compete with such self-produced products would need to be priced particularly close to marginal cost if the best attainable allocation of resources is to be reached.

On the other hand, complementarity relations must be considered, also. It might be desirable to price at less than marginal cost a service that happens to be strongly complementary with services that have established prices unusually far above marginal cost. A somewhat inadvertent example of this is the practice of allowing the use of feeder highways at charges often substantially less than marginal cost, balanced in some instances by the charging of tolls substantially in excess of marginal cost on bridges, tunnels, and the like. Conversely, services that are strongly complementary with leisure or nonmarket production might warrantably be priced further above marginal cost than otherwise. This proposition might be held to indicate higher rates for recreational travel, amusements, and the like, except for the fact that there are also strong substitution relationships to be taken into consideration as well and that accordingly the demand for such services is often highly elastic.

The taking into consideration of complementarity and substitution relationships in the application of marginal cost pricing is thus an extremely complex matter that cannot be reduced to any simple rule of thumb. Except for a few special situations, however, it does not seem that complementarity relationships would in themselves require any but relatively minor modifications of marginal cost pricing practices.

But whatever arguments may be advanced for departing in various degrees from a strict marginal cost pricing policy, no sound pricing policy can be developed without using marginal cost as one of the prin-

cipal determinants. Indeed, while marginal cost pricing has been discussed most frequently in a context of decreasing cost situations and subsidies, marginal cost has an important role to play even where economies of scale are absent and there is no problem of self-liquidating versus subsidized operation. Adequate consideration of marginal cost in the setting up of a price structure often leads to structures quite different from those in general use, wholly aside from the question of the proper general level of rates. Even where economies of scale are substantial, marginal cost considerations may be much more important in relation to the structure of rates than to the level.

To illustrate the structural importance of marginal cost pricing, let us consider the following example. A daily train is to be operated from A to B, and possibly to continue on to C, and return. Constant returns to scale prevail over the range of operations contemplated and costs are of three kinds: 20 cents per seat per day for capital and similar charges on the equipment, independent of the distance operated; 30 cents per seat for operating expenses for each leg of the trip; 10 cents per passenger for each leg of the trip for wear and tear on the equipment, cleaning, and service to passengers. It is assumed that equipment can be found to make the train up to any desired number of seats at strictly proportional costs but that it is impractical to change the consist of the train at B and that if it is to run through to C at all, the entire train must be run. Demand for the AB leg is linear, ranging from 2,000 at a price of zero to a maximum price of $2.00 that the most eager passenger is willing to pay; demand for the BC stretch is one-fourth as great, ranging up to 500 passengers at a zero price, with the same maximum price.

Some simple calculations will show that if the train is operated between A and B only, with the capacity exactly adjusted to the traffic, average and marginal costs are 50 cents per trip, and that at this fare there will be 1,500 riders each way, yielding a total consumer's surplus of $2,250.00 per day for the round trip. On the other hand, if the train is operated through to C, marginal cost for the BC portion of the trip is only 10 cents, since there will inevitably be empty seats, while the marginal cost for the AB trip will be 80 cents, the cost of providing additional seats for this trip having risen because the additional seats, if provided, will now have to be carried all the way to C. At fares equal to these marginal costs, there will be 475 BC passengers, whose consumer's surplus for the round trip will be $902.50, and 1,200 AB passengers, with consumer's surplus amounting to $1,440.00, or a total of $2,342.50. As this is a case of constant returns to scale, we can expect that revenues at marginal cost rates will just meet costs, and this is indeed the case. The net welfare product is thus in each case

the consumer's surplus generated, and accordingly the second mode of operation is to be preferred to the first, and this is, indeed, the optimum mode of operation.

Now in principle, in a case where there are constant returns to scale, it should be possible to allocate costs in such a way as to produce allocated costs identical with marginal cost. And yet I venture to suggest that in a case such as the above the optimum result shown would not be reached by any of the usual cost allocation procedures unaided by marginal cost concepts. The concept of marginal cost cannot be dispensed with as a tool for making cost allocations even in the constant returns to scale case.

To be sure, the above example is a little unusual in that although it is not strictly speaking a case of increasing returns, it is a case where indivisibilities enter through the assumption that it would not be possible to provide the service in two or more trains, with only some of the trains operating through to C. Thus the rather unusual result arrived at would not occur in a case where the production process is perfectly divisible and where accordingly competition is possible. But such cases do occur in varying degrees of complexity embedded in a context of decreasing costs. Yet in many cases the more dramatic results of the application of marginal cost pricing will not be a reflection of the degree of decreasing cost, but rather of the pattern of cost interrelationships produced by the indivisibilities.

Indeed, if the only implication of marginal cost pricing were that in decreasing-cost industries the price structure should be reduced more or less uniformly by a percentage reflecting the amount by which the elasticity of the long-run production function falls short of unity, one might be justified, in many cases, in feeling only a very restrained enthusiasm. For example, studies I have made of the marginal costs for railroad freight service indicate a level in the neighborhood of 80 per cent of average costs, on a long-run basis. Subsidies of the order of 2 billion dollars a year, which would be indicated in order to enable rates to be cut to a marginal cost level, would produce a net gain in national product of some 250 billions, assuming an elasticity of demand of about unity. In view of what has been said above concerning the marginal costs of public revenues, this would seem to have relatively little to promise. Actually, in such a case one could probably rest content with recommending that taxes bearing on freight transportation in one way or another be abated as far as administratively possible.

Passenger service is a somewhat different matter, however. While if we measure output of passenger service in terms of passenger car miles it is possible to arrive at a figure somewhat comparable to that for freight service, this is not the end of the story. The degree to which this

service is actually utilized is positively correlated, to a considerable degree, with the density of traffic: a 10 per cent increase in traffic generally requires an increase of considerably less than 10 per cent in the space that must be provided to give an equivalent quality of service. Moreover, as traffic density increases, the quality of the service tends to increase, in terms of frequency, variety, speed, and sometimes in quality of equipment. Or to put it another way, the fact that passengers will not wait as readily as will freight for the accumulation of trainloads of minimum cost size means that trainloads are generally smaller than would produce a minimum average cost per passenger and thus there are substantial economies of scale from increasing the number of passengers per train to be compounded with the economies of having more trains per year per route. All of this is rather difficult to evaluate, but it seems not impossible to place the marginal cost of passenger service as low as one-third of the average cost, particularly on lines where the density of traffic is fairly low and the average haul short. Some support for the more extreme point of view may be found in Gilbert Walker's figures comparing main line with local and branch line passenger service costs in England, given in an article in the *Oxford Economic Papers* for March, 1953.

It is fashionable in railroad circles to bemoan the present deficit shown for passenger operations as constituting one more unfair burden thrown upon railroad freight traffic, placing it at an improper disadvantage as compared with truck traffic. But if it is true that the elasticity of costs for passenger service is substantially lower than for freight, this would justify a substantial discrimination in favor of passenger service. The existing relation between passenger and freight rates may thus be more desirable, on the whole, than one where passenger service is required to meet all of the costs for which it is incrementally responsible as a whole, to say nothing of a share in the costs that are inextricably joint as between passenger and freight service. Of course, an even better allocation of resources could be obtained if railroad passenger services could be subsidized from general tax funds rather than, in effect, from a special tax on railroad freight service.

In another area, it is often claimed that telephone service is rendered under conditions of increasing rather than decreasing costs. It is claimed, with some justice, that the central office equipment required to furnish interconnections between subscribers increases more rapidly than in proportion to the number of subscribers. Comparisons of costs between large and small communities also seem to indicate increasing costs. But this is not the whole story. Much of the outside plant of telephone companies can be installed, under given outside conditions,

at much lower unit costs at high densities of service than at low; long distance service in particular seems to have a strong decreasing cost characteristic. The economies of scale in the outside plant would seem to be sufficient in most cases to much more than offset the diseconomies of scale in the switching plant. Nor is the urban-rural or large town-small town comparison quite appropriate. While it is true that a large exchange will usually find that the conditions under which the outside plant must be installed are more costly than a small exchange, this is not caused by the size of the exchange but is only an associated condition. Actually, if in some given city such as Detroit, for example, one were to imagine that nine-tenths of the customers taken at random had never installed a telephone, I would be very much surprised if it would have been possible to design and construct a telephone system to serve the remaining tenth of the subscribers at as low a cost per subscriber as is obtainable with the existing pattern of service, particularly if the amount of use per telephone were to be held approximately the same. Moreover, even were it true that costs under given circumstances tended to go up more than in proportion to the number of subscribers, this might be considered offset by the increased value of the service rendered when a subscriber can reach a larger number of other phones in the community. Thus telephone service is by no means to be excluded from the candidates for subsidy if marginal cost pricing is adopted as a general principle.

One of the major characteristics of marginal cost structures is the rather marked variation in such costs as between-peak and off-peak demands, whether the peak be in terms of daily, weekly, or seasonal fluctuations, or even as between congested and uncongested sections of route. In the ideally simple case, where demand is exactly predictable and capacity is a sharp and rigid limit, the appropriate marginal cost price is one which covers only the utilization or service costs as long as demand at that price is below capacity, and which is at other times or places just sufficient to keep effective demand at the capacity level, capacity being in turn adjusted so that the amounts obtained over and above the service costs during such periods of capacity use are just enough to cover the marginal costs associated with an increase in capacity. The marginal cost of a peak period unit may be thought of as the opportunity cost involved in depriving the next most eager consumer of the service, or of a cost arrived at by taking the marginal cost of an increment of capacity and deducting as a credit the excess of the value of all other units of service which the increment of capacity makes it worth while to produce and sell over their respective direct variable costs.

The picture is of course often complicated by the fact that various

different capacity limits often operate on the same service at different times and in different ways. In the very short run, for example, the number of seats scheduled to be operated on a given train may be fixed; with some advance notice, cars can be added or dropped, but perhaps only by shifting equipment from some other service; over a longer period, schedules can be adjusted and eventually adjustments to track layout can take place. The appropriate measure of the peak also varies: where the timing is crucial, as with transit service, limitations imposed by track or roadway capacity operate on the basis of a volume measured over an interval of say five or ten minutes; with respect to the availability of equipment, the peak is determined by integrating over a period equal to the time taken for a round trip, say ninety minutes.

Capacity limits on output of nonstorable services often operate to produce rather drastic fluctuations in marginal costs that are regular and predictable enough to be used as the basis for rate making. For subway service in New York, for example, it has been estimated that the marginal cost of a fairly long rush-hour trip would be on the order of 30 to 50 cents, while a comparable nonrush-hour trip would have a marginal cost of only 8 to 10 cents, and rush-hour trips in a direction opposed to the major flow of traffic would in most cases be a surplus by-product service with a marginal cost substantially zero. Costs per passenger mile will vary equally drastically for different portions of a single trip on a single train. An evening rush-hour trip from Coney Island to Bedford Park in the Bronx would have a zero marginal cost for the Brooklyn portion of the trip, a cost rising to about 5 cents per mile for the upper Manhattan portion of the trip and falling off to 1 or 2 cents per mile for the Bronx portion of the trip.

Capacity limits are, however, not always rigid. The usual way of expressing a somewhat elastic capacity limit is in terms of a more or less sharply rising marginal cost curve. In many cases, however, while the capacity limits on the volume of service offered are quite rigid, elasticity at the capacity limit is provided by impairment in the quality of the service offered. It may not be possible to run more trains or longer trains on a given route, at least in the short run, whether from lack of track capacity or of equipment; an increase in traffic will be accommodated by having a larger proportion of standees, or more intense crowding. Costs to the operating agency may be very little affected, and in this sense one might say that marginal cost is very low; the cost, however, is to be measured in terms of the deterioration in the value of the service to the former passengers. It is not often realized how great a marginal cost computed in this way can easily be. If the addition of 20 passengers to a car already containing 200 passengers creates increased crowding, that the original passengers would on the

average pay 3 cents each to avoid, this is a cost of $6.00 to be divided among the 20 passengers causing it, resulting in a marginal cost of 30 cents each. Or as demand approaches the theoretical capacity, the slack necessary to insure taking care of customers as they apply may vanish, with the result of longer and longer queues or delays in getting service. The queue or the delay may operate to cut down to some extent the demand for service, but in general this will be an inadequate deterrent without substantial increases in the rate as well, since the operative deterrent for the individual is the average delay, not the much greater increment in total delays that will be caused by the addition of one person to the demand: not only will the additional customer have to wait while he works his way to the head of the queue, but all customers after him will have their waits increased by the time required to service the added customer, until such time as either the queue is entirely worked off or some potential customer is discouraged by the increased length of the queue.

In an extreme case where the control of the queue may require diversion of resources from the providing of the service, as for example may occur in some situations in telephone service, overload may even reduce the amount of service that can actually be rendered below what it would have been had demand been more moderate. For example, repeated attempts of thwarted subscribers to place calls may absorb the time of operators or of certain types of common equipment in an automatic exchange such as registers, senders, markers, and the like. Considerable additional complexity of central office equipment is often necessary to prevent paralysis in such contingencies.

Fairly extreme results also tend to occur in poorly controlled situations such as curb parking. Very marked differences in the amount of time which a would-be parker will have to spend in finding a place to park, and also in the distance that he will have to walk to get to his ultimate destination, on the average, according to whether, out of say 1,000 parking spaces in a given area, 999 are, again on the average, occupied, or whether the average occupancy is only 995 or 990. Again, the deterrent to say the 990th parker is only the amount of time he will have to search, and he takes no account, ordinarily, of the added searching time that his occupancy of a space will impose on those who come after. Metering of curb parking space on marginal cost principles would thus require rather substantial fluctuations in the rate per hour as the degree of occupancy fluctuates in the neighborhood of 100 per cent.

In summary, then, marginal cost pricing must be regarded not as a mere proposal to lower rates generally below the average cost level but rather as an approach which implies a drastic rearrangement of the

patterns and structures of rates. Indeed, it is this restructuring of rates that is likely to be the greatest contribution of marginal cost pricing to the improvement of the over-all efficiency of our economy, while the further gains that might be obtainable from the reduction of rates from a self-sustaining level to a marginal cost level, once the pattern of rates has been made to conform as closely as possible to marginal cost, are likely to be relatively small. The issue is thus not primarily one of subsidized versus nonsubsidized operation, though this is still an important issue. The dominant issue is one of whether the pattern of rates should be based on tradition, inertia, and happenstance, or whether it is to be developed by a careful weighing of the relevant factors with a view to guiding consumers to make an efficient use of the facilities that are available.

Perhaps some indication of the outstanding absurdities that occur in present utility rate structures may be worth while in conclusion. For example, in New York a new vehicular tunnel was opened a few years ago from the Battery to Brooklyn. Since it is a new facility and undoubtedly much more easy and pleasant to use than the old East River bridges, it must, forsooth, be made to pay for itself by the imposition of tolls starting at 35 cents, the practical consequence of which is to encourage continued heavy use of the Manhattan bridge for all trips for which that route is shorter than the tunnel, with the result that the streets near the Manhattan end of the bridge are the scene of some of the worst traffic congestion in the city. Marginal cost considerations would call for the collection of a substantial toll on the old East River bridges, at least during hours of heavy congestion, and a smaller toll or none at all for the tunnel, even though this might mean that the users of the bridges might be "paying for" the tunnel.

In suburban railroad service, the lowest fares offered are almost invariably the weekly and monthly commutation tickets used predominantly by commuters who travel almost exclusively in the rush hour when marginal costs are highest; next highest are multiple-ride tickets used by family members, often at the rush hours, but somewhat less frequently so. Users of one-way or round-trip tickets, on the other hand, are more likely to be off-peak or even counter-rush riders with very low marginal cost. Nor can this pattern be defended on the basis of elasticity. The daily commuter rides from almost absolute necessity, in most cases, whereas the occasional trips of other family members are often dispensable trips; further, the use of the family car for such trips is much more often a strong competitor, since the car is then not needed at home, the schedules are less frequent and convenient than in the rush hour, and many persons may be riding together. To be sure, in the long run there is elasticity to the daily commuter traffic in that

the fares influence the decision to move to the suburbs, but even to the extent that this is a significant factor, it will be some weighted average of the commutation and multi-trip ticket that would be the relevant rate to be considered by the prospective suburbanite. Political pressures seem to be partly responsible for this state of affairs: regular commuters have both a sufficient stake and a sufficient appeal to the public sentiments to bring effective pressure to bear on regulatory bodies. Some roads have been of late making tentative progress towards putting in a reduced rate type of ticket good only during the nonrush hours, but the attempt in most cases seems to have been rather half hearted, as the rate offered is usually still higher than that available in the multi-trip family ticket. Marginal cost pricing here would go much further than this and just about reverse the entire pricing structure.

The same delusion often gets a foothold in the local transit field. Philadelphia, probably on the basis of political pleas, has recently adopted a plan for selling strips of ten tickets, good for one week only, which of course are used predominantly by the high-cost rush-hour riders.

The rapid obsolescence of that great American institution, the open-section Pullman car, is in many ways the work of an inefficient pricing policy, itself in large measure the product of inefficient working arrangements between the Pullman Company and the railroads. No serious attempt seems ever to have been made to vary rates so as to balance the demand for upper and lower berths, so that what was and could still be a device for furnishing a satisfactory low-cost service is fast being relegated to the scrap heap.

And so it goes. One may, for various good and sufficient reasons, hesitate to embrace marginal cost pricing in all of its ramifications as an absolute standard. But no approach to utility pricing can be considered truly rational which does not give an important and even a major weight to marginal cost considerations. And when adequate weight is given to such consideration, important changes in present pricing practices will be indicated in many areas.

APPLICATION OF MARKET PRICING FACTORS IN THE DIVISION OF TRAFFIC ACCORDING TO PRINCIPLES OF ECONOMY AND FITNESS

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The basic issue here under consideration would appear to be whether the transportation pricing mechanism, as it now operates, gives recognition to the inherent advantages of each mode of transportation. Does the pricing mechanism guide the traffic to the agency most fit to do the job? The annual bill for services rendered the nation by its regulated carriers reporting to the Interstate Commerce Commission now runs to nearly 20 billion dollars, and to nearly double this if one includes all other for-hire transportation and private trucking. Could a redistribution of the traffic among the agencies of transportation, as effected by pricing, reduce this sum total cost to the economy?

It seems desirable to first look at the economic motivations, principles, and structure of transportation pricing; secondly, to sketch out some of the problems and experiences bearing on the division of the traffic according to fitness and economy; and, thirdly, to try to formulate some working principles for the recognition of inherent advantages in competitive situations.

Definition of Fitness and Economy. It may be said that the shipper who buys transportation services seeks to employ his funds most effectively. He seeks to minimize his total transportation costs; and by total costs is meant the combined total of the carrier's tariff charges and the shipper's own expenses incident to the receipt, delivery, packaging, etc., of the freight. Fitness and economy to him mean rates and services.

Taking the economy as a whole, however, fitness and economy are believed to be achieved when the nation's traffic is so distributed among the agencies of transportation that the aggregate costs for all transportation (including shipper supplementary expenses) are lower than they would be under any other basis of distribution. Taking the nation's transportation plant—rail, highway, water, etc.—substantially as we find it, this objective would be achieved when each shipment is transported by that agency whose long-term out-of-pocket costs to handle it, when combined with an evaluation of the shipper's costs and conveniences (packaging, loading, etc.) produce the lowest sum total expense. The long-term out-of-pocket or incremental costs here imply a period

sufficiently long for changes in traffic volume to have had their full effect on the carrier's operating practices and plant. When the traffic is handled by the carrier with the lowest incremental costs, the portion of the carrier revenues carried down to "contribution" and "net" is maximized and that consumed in the coverage of out-of-pocket costs is minimized, assuming identical rates.

But the shipper knows little or nothing about carrier out-of-pocket costs, and in any event would be guided not by such costs but by the tariff rates which he must pay, factors of service and convenience considered. However, where two agencies of transportation compete, each carrier's effort to find the rate most profitable to itself will normally bring about a set of rate relationships between agencies which will roughly reflect relative fitness and economy. The urge to maximize profits will normally cause the carrier having significantly lower out-of-pocket costs, and hence the greater flexibility in adjusting its charges, to drop its rates to a point where it can get a very large share, if not the preponderance, of the competitive traffic.

Carrier Motivations. Carrier motivation centers around the maximizing of net revenues. Maximizing profits means maximizing the contribution to burden earned from each kind of traffic or, indeed, from separate movements of a given kind of traffic where grounds exist for differentiating one haul from another on the same traffic.

The mental arithmetic the carrier's traffic man is presumed to go through in pricing out each of his "differentiated" transportation services is set out in Table 1. It is merely the application of the universal endeavor of all who sell to determine that level of the price which, volume considered, will be most profitable to themselves. It is explained most descriptively as maximizing the contribution (over out-of-pocket costs), or as the marginal revenue-marginal cost analysis (equating marginal revenues to marginal costs).

It is a matter of arithmetic that when price reductions made to induce new business reach a point where the increase in total revenues resulting from the last few units added does no more than equal the increase in expenses incurred thereby, one has reached the point of diminishing returns. At this point the contribution to overhead burden (be it high or low) is at its maximum as Table 1 illustrates. (See line 6, columns 5, 6, 7.)

This whole proposition was neatly summed up fifty years ago with the explanation that the traffic manager lowers rates whenever it will increase net earnings; i.e., as long as gross earnings continue to increase faster than operating expenses. (A. T. Hadley, *Railroad Transportation* [1900], page 261. The schedule of figures in Table 1 sums up the end product of all the rate-making elements encompassed in the all-inclusive but rather ambiguous terms "value-of-service" or "ability to

TABLE 1

SCHEDULE OF PROJECTED RATES, VOLUME, CONTRIBUTIONS TO BURDEN, MARGINAL REVENUES AND MARGINAL COSTS*

Line	Rates Per Carload	Projected Volume Carloads	Total Revenue 1×2	Total Out-of-Pocket Costs†	Contribution to Burden 3−4	Marginal Revenues per Carload‡	Marginal Cost per Carload§
	1	2	3	4	5	6	7
1.....	$50	0	$ 0	$ 0	$ 0	$—	$—
2.....	48	10	480	320	160	48	32
3.....	46	20	920	640	280	44	32
4.....	44	30	1,320	960	360	40	32
5.....	42	40	1,680	1,280	400	36	32
6.....	40	50	2,000	1,600	400	32	32
7.....	38	60	2,280	1,920	360	28	32
8.....	36	70	2,520	2,240	280	24	32
9.....	34	80	2,720	2,560	160	20	32
10.....	32	90	2,880	2,880	0	16	32

* For extended analysis of the marginal revenue concept see *Economic Analyses*, Kenneth E. Boulding, 1948, page 526. It is important to note the assumption in Table 1 that added business can only be generated by price reductions, and that because of an assumed inability to differentiate between markets, the rates on all traffic which would move at $48, $46, etc., per carload, must be reduced to maintain uniformity. At rates near $40 to $42 per car the "contribution" to overhead burden (and net) becomes maximized at the figure of $400 (column 5). At a rate of $40 per car, column 1, the marginal revenue of $32 has been equated to (equals) the marginal cost of $32, columns 6–7. At this level the traffic in question does not burden other traffic; indeed, at any rate other than $40–$42 the ability of the traffic to "contribute" would not have been fully exploited, and to that degree, an unnecessary burden would be cast on other business of the carrier. The difficult problem, of course, is the determination of the volume of traffic which one may reasonably expect to induce with any given rate adjustment. Were it not for this factor, rate making could be reduced to a combination of cost finding and arithmetic. Obviously, a working knowledge of the variable or out-of-pocket costs is necessary. These latter are taken as those expenses which are found to vary directly and proportionately with volume at the level of the firm's output most pertinent to pricing for the future. This is basically a matter of cost behavior within the industry. As J. M. Clark pointed out (*Economics of Overhead Costs*, 1923, page 223), one can estimate the effect of added increments of business by observing the behavior of costs for a group of firms whose volume of output has been fluctuating over a period of time, or by taking a single period of time, comparing the costs for firms with a wide range of output. The Cost Section of the Interstate Commerce Commission has made wide uses of both approaches (Statement 4–54).

† Computed at $32 per carload.

‡ The marginal revenue is the added total revenue derived from selling one more unit. Thus, divide each increase in revenues, column 3, by the corresponding increase in carloads, column 2.

§ The marginal cost is the added cost incurred in producing one more unit, $32. Thus, divide each increase in column 4 by the increase in column 2.

pay.") Referring to the Act, Table 1 represents an application of the rate making rule, Section 15(a); that is, a giving of consideration to the effect of the rates upon the movement of the traffic. This latter, a basic ingredient of all market pricing, is no doubt inherent in the concept of a just and reasonable rate under Section 1.

Equilibrium Position Toward Which Transportation Rate Structures Tend. The existing rate structure of each carrier or mode of transportation presumably has evolved after much trial and error, through the

so-called "contribution to burden" or "marginal revenue" analysis, as illustrated in Table 1. It is as though the carrier's traffic manager took each piece of traffic and, starting with out-of-pocket costs as a floor, added such margin, considering the responsiveness of the traffic to the rates charged, as would maximize the contribution. The influence of regulation has here been to place such rate ceiling on the traffic with an inelastic demand as is necessary to no more than make the carrier come out whole (including return); also to prevent exploitation of traffic "wedded" to one carrier or one mode of transportation and literally at the carrier's mercy. The end product is subject to "smoothing out" to avoid undue discrimination between competing shippers.

In short, a rate structure tends toward an equilibrium position under which the rates on each commodity having an elastic demand settle to that margin above out-of-pocket costs as will maximize that commodity's contribution toward the overhead burden and the net. (The fully distributed cost concept does not provide a norm for setting the specific level of a rate. Where fixed or joint costs are present, "full costs" represent a philosophically insoluble problem. If arbitrary statistical apportionments are made, they do not reflect shipper response to the rate set.) As the contribution of traffic with an elastic demand is thus brought to its maximum, the burden resting on the remaining traffic having a comparatively inelastic demand is lessened, thus permitting the rates on this traffic also to settle to a lower level than would otherwise be possible.

The rate structure is the end product, in principle, of an endless equating of marginal revenues to marginal costs in each differentiated transportation market. Each class of freight constitutes a separate market which may be further differentiated by areas, directions of movement, or even point-to-point, where the intensity of the demand (volume, intercarrier competition, etc.) differs. Piecemeal breakdowns of markets permit differential pricing; i.e., the fixing of prices to attract specific additional tonnage and net revenue without sacrificing needed revenue on traffic of the same general kind presently moving freely elsewhere. Different ratings on live dry cell batteries from dead batteries is a classic illustration.

As one might expect, the universal application of the principle of "maximizing the net" in a myriad of highly differentiated markets has created an enormous range in the relation of rates to the out-of-pocket costs. Of the 199 commodity classes of rail freight traffic for which the relation of revenues to out-of-pocket costs was available in a 1951 study made by the Interstate Commerce Commission staff, twenty classes moved at rates significantly below (over 10 per cent below) their long-term out-of-pocket costs, nineteen classes moved at or near

out-of-pocket costs (within a range 10 per cent below to 10 per cent above), fourty-four classes moved at rates ranging from 111 to 149 per cent of out-of-pocket, eighty classes at levels 150 to 200 per cent, and thirty-six classes at levels 200 to 300 per cent or higher of out-of-pocket.[1] Revenues on individual commodity classes ranged from 20 to 30 per cent below out-of-pocket to 600 per cent above. Rates on the same commodity might vary from out-of-pocket to near 200 per cent above out-of-pocket depending on the intensity of competition on individual point-to-point movements or different lengths of haul. Revenues averaged 150 per cent of out-of-pocket costs for all carload traffic combined.

Probably the most significant comparisons are those based on the dollar contribution which each class of freight makes toward the overhead burden, for it is this factor which the carrier seeks to maximize.

Some commodity classes produce tens or hundreds of millions of dollars of contribution toward overhead burden and the net (see Appendix). Other traffic creates millions or even hundreds of millions of out-of-pocket losses. The rail passenger-train service accounting deficits, now running to over 700 million dollars, result in out-of-pocket losses estimated at between 400 and 500 millions. The rail less-carload out-of-pocket losses were computed by the Commission's Cost Section at over 100 million dollars in 1952.

In the motor carrier field the range in the relation of rates to out-of-pocket costs is not so extreme, though very substantial variations are to be found. In a southern motor carrier study made by the Commission's Cost Section[2] the revenues on single line minimum shipments within the South ranged from some 70 per cent of out-of-pocket costs on short hauls to nearly 140 per cent on long hauls. They averaged 98 per cent on all single line minimum shipments. In the heavier weight brackets revenues on single line shipments ranged from nearly 100 per cent of out-of-pocket costs to 150 per cent and, in a few instances, to nearly 200 per cent of out-of-pocket costs. The average for all single line and interline shipments was 119 per cent. Similar wide ranges were found in an earlier study (1945) covering ninety-five motor carriers in Western

[1] Based on ICC Statement 7-53, December, 1953. In terms of net ton-miles, a little over 4 per cent of the carload freight traffic moved at charges significantly below (over 10 per cent below) out-of-pocket; 12 per cent of the ton-miles moved at rates within striking distance of long-term out-of-pocket costs (10 per cent plus or minus); 44 per cent at charges 111 to 149 per cent of out-of-pocket; 25 per cent moves at charges 150 to 200 per cent of out-of-pocket; 14 per cent moves at charges 200 to 300 per cent of out-of-pocket; and about 0.5 per cent moves at rates which are 300 per cent or more of out-of-pocket.

[2] Statement No. 2-53, April, 1953. Out-of-pocket motor carrier costs taken as 90 per cent of the following: operating expenses, rents, taxes, and a 5 per cent return on the depreciated investment.

Trunk Line Territory when the traffic was broken down by weight brackets, mileage blocks, individual commodities, and directions of movements. (Supplemental report of the Federal Manager of Motor Carriers Transportation System and Properties.)

The purport of the foregoing is that the marginal revenue concept can produce a very wide range in the relation of rates to costs. On traffic where large out-of-pocket losses are incurred other contributing forces are obviously present.

Incentive Pricing as an Inducement to Economy and Fitness. In approaching the question as to whether transportation pricing, as it now exists, fully exploits the inherent advantages of the various modes of transportation, some useful contrasts can here be drawn between the utilities and the transportation agencies. A major key to the mass markets in electric power and natural gas has no doubt been the wide-scale use of incentive pricing. Block rates, volume rates, and other forms of quantity discounts are widely resorted to. One recalls the forecast of a utility executive that electricity would some day become so cheap that users would not exert the effort to turn their lights off. Perhaps this is far-fetched, but it no doubt represents the spirit underlying the electric power industry's approach to the marketing of its services, and natural gas is not far behind.

The basic motivation in utility pricing is to obtain the fullest possible utilization of existing plant and distribution facilities and, even more important, the far-reaching benefits of an increasing scale of output with its corresponding reduction in unit costs. Such pricing programs rest on a full and complete exploitation of the marginal revenue principle. Utilities not only differentiate between residential, commercial, and small and large industrial consumers, but each customer's consumption is so differentiated (by steps or blocks) as to encourage his maximum utilization of the company's plant and facilities. Even the small household consumer gets lower rates as his use increases.

In the illustration of the application of the marginal revenue concept in transportation (Table 1), the scope of the rate reductions was limited by the fact that any discount given to attract volume consumption was applied "across the board" to all the traffic. This method of attracting volume business is inevitably accompanied by an increasing "erosion" of net revenues on the business already at hand. The end effect in the illustration given was that it became unprofitable to charge less than $40 to $42 per carload even though it would be in the carrier's interest to take on added pieces of business at rates as low as $34 per carload. This would still exceed the out-of-pocket costs of $32 per carload. Referring to Table 1, column 3, the total revenues at $34 per carload totaled $2,720, the out-of-pocket costs $2,560, and the contribu-

tion to burden only $160. Under block pricing, the first ten carloads would continue to pay $48, the next ten $46, etc. The total revenues, if carried through to line 9, would have been $3,280, and the contribution $720. This is $320 more than the $400 economically attainable under a flat unit price of $42 per car.

Generally speaking, the rate problem appears to be one of so differentiating between markets, or users of the service, as not to unjustly injure one shipper in relation to another shipper in a common market. Where the business operations of each are dissimilar or the large shipper is in a position to exercise the advantages of bigness in any event, a failure to differentiate the markets may accomplish little purpose. Rather, it may operate to limit fitness and economy and the full exploitation of the carrier's capacity for mass output with its attendant possibilities for a reduction in unit costs and charges to all users of the service.

The application of block pricing principles in the transportation field is evidenced by the existence of less-carload or less-truckload rates versus carload or truckload rates; also by the application of LTL breakdowns of, say, 0-2,000 pounds, 2,000-6,000 pounds, etc. Beyond the carload or truckload rate level carrier efforts to apply the principle of incremental pricing have taken several forms. In the rail field they include heavier carload minimums, incentive rates which provide a lower charge per 100 pounds on that portion of the carload loading in excess of a given minimum loading, multiple-car rates, trainload rates, differentiation in the commodity itself (fine coal versus other coal), and special commodity point-to-point rates. Fourth Section relief is part and parcel of this market differentiation.

The shipper is under the same pressure to minimize his transportation costs as the carrier is to maximize his net. From his standpoint, the next lower "rate block" beyond the published carload or truckload rates may mean not only the foregoing rate possibilities but a turning to pipelines, conveyor belts, barge transportation, plant relocation, or the operation of the shipper's own highway or water operations. All factors considered, it is believed that in the transportation sector of the economy research has lagged in the application and use of incentive pricing as a means of realizing on inherent advantages.

A Case History—New Automobiles in Interstate Commerce. The recognition of inherent advantages among competing modes of transportation and whether such recognition could be obtained short of the exercise of the minimum rate-making power, were matters in issue before the Interstate Commerce Commission in Docket 28190, New Automobiles in Interstate Commerce. (259 ICC 475. The Commission report follows verbatim as to facts and discussion, the proposed re-

port of Examiner William A. Disque.) The Commission had launched a general investigation into the rates, charges, and practices in the interstate haulage of new automobiles by rail, by highway, and also by water when in conjunction with rail or highway. The rails had made sweeping and drastic rate reductions, particularly from assembly points for relatively short hauls, to overcome a heavy diversion of the traffic to other carriers. Whereas in the early thirties new automobiles had moved at first class, competition had brought the charges down to levels ranging from 30 to 100 per cent of first class, depending upon the severity of truck competition. Such of the rail rates as were below first class were frequently arrived at by deducting from motor charges certain amounts, generally some eight cents, estimated to offset the rail service disabilities.

The record developed that the shipper's routing was governed by the carrier's rates or charges, the shipper's own costs and conveniences (as related to each form of transportation), and even by subjective factors. Aside from rates, the items listed affecting mode of transportation included expedition, overnight delivery, nighttime deliveries, ready availability of service, regularity, dependability, flexibility, low-cost terminal storage, cleanness of automobiles on delivery, promptness in paying claims, and the cost of local haulage and unloading attached to rail movements. The rails sought to be left free to meet their competition and to publish rates necessary to obtain their "share of the traffic." They contended that they had spent millions of dollars in terminals, equipment, and other facilities to develop the traffic and intended to do their utmost to attract and retain all of it they could handle, at compensatory rates no further below a reasonable maximum level than necessary. They held that the Commission's staff cost studies demonstrated that their rates were generally between the out-of-pocket and fully distributed cost levels and could not, therefore, be condemned as noncompensatory (259 ICC 475, 533).

The motor carriers contended that inherent advantages in respect of service should be held paramount to other factors and should not be offset by other carriers through lowered rates. The motor common carriers argued that the time had arrived when the Commission should exercise its minimum rate-making power to save the revenues of all forms of transportation from further depletion; also to prevent the extinction of the motor common carriers. (*Ibid.*, page 530.)

As to contract highway carriers, the Commission noted that General Motors had helped develop seven contract motor carriers which hauled exclusively for it, that each of these seven carriers made its rates based on the transportation conditions under which it operated and without regard to the rates of other carriers (*ibid.*, pages 490-491),

and that these carriers co-operated closely with General Motors almost as if they were departments of its distributing organization. The contract carriers were assured a definite minimum volume of traffic so long as their rates were satisfactory to General Motors, a factor giving them a definite advantage over common carriers whose business was uncertain. These contract carriers felt that their real competition was with General Motors which was capable of setting up a highway transportation organization as a department of its own company.

The Commission found that under the interplay of competitive forces there had gradually evolved a distribution of the automobile traffic among the various modes of transportation which seemed to reflect the inherent service advantages of each agency more than it did the rates for the service. The water carriers held practically all the traffic from Detroit to Cleveland and Buffalo because they provided specialized mass transportation as well as good service at the lowest available rates.

The rails carried the major portion of the long-haul traffic because they provided the best mass transportation readily available for such traffic at lower rates than other agencies, except possibly the coastal and intercoastal water lines. The motor carriers, for very short hauls, had the advantages of generally lower costs, reflected in the lower rates than the other agencies maintained; also the best service. The latter factor, the Commission said, weighed heavily as a competitive factor in the motor carriers' favor within areas of distribution beyond those where their costs were the lowest (*ibid.*, page 555). For example, from the Michigan manufacturing points to Ohio, most of Indiana and Illinois, outside of Chicago, rail transportation of automobiles was virtually nonexistent. The Commission held that unless future rate changes were substantial in amount they would not appear capable of altering in any substantial degree the then existing general distribution of the traffic among the different modes of transportation (*ibid.*, page 539).

As previously noted, the highway common carriers, squeezed by rail rate reductions and contract carrier competition, asked the Commission to exercise its minimum rate-making powers. The Commission denied the application, observing that its minimum rate-making powers had been used sparingly (*ibid.*, page 535); that the application of such powers had been confined to instances in which it had found that a rate was so low as needlessly to sacrifice a carrier's revenue; that methods employed by a carrier to meet competition are primarily a matter of managerial discretion rather than regulation, so long as they are lawful (*ibid.*, page 531); and that "competition often is the process by which the public obtains reasonable rates." (*Ibid.*, page 532.)

The case history of Docket 28190 demonstrates, it is believed, a full-scale application of the contribution or marginal revenue concept to the pricing of the carrier services. It portrays the wide-open operation of market pricing factors in transportation with the fittest mode of transportation taking the overwhelming share of the traffic in the areas where it clearly had the inherent advantages, rates, and services considered. By such division of the traffic, economy and fitness were effectuated with the monetary benefits passed on to the users of the service and to the national economy as a whole. Wasteful transportation (by the less fit carriers) was minimized. Broadly put, the results would seem to be in accord with the national objectives of conservation of resources.

Before terminating this discussion, two additional factors bearing on fitness and economy should be noted. The provision by government of transportation facilities—highways, waterways, and airways, etc.—enormously facilitates entry into the transportation market. To the extent that the rates and charges of the for-hire carriers fail to reflect the economic cost of the public facilities used, their prices are deficient as a test of inherent advantages. Secondly, where heavy out-of-pocket losses are incurred, such as in the rail passenger and LCL services, the imposition of such burdens upon freight traffic seriously affects the carrier's ability to realize on its inherent advantages in the haul of the freight traffic. The rail passenger and LCL out-of-pocket deficits alone require freight rates on the carload traffic which contributes toward such deficits (i.e., more than covers its own out-of-pocket cost) to be 7 per cent higher than otherwise would be necessary to yield the same rate of return.

Summary. From the points developed herein, there may be deduced the following suggested working principles applicable to interagency competitive situations:

1. Fitness and economy are achieved on any given traffic movement when the shipment is so moved as to occasion the lowest sum-total incremental costs (carrier out-of-pocket costs plus shipper supplemental expenses). Carrier rates and services obviously are both involved.

2. The contribution or marginal revenue concept provides a wide scope for differential and incentive pricing through which the inherent transportation advantages of any given mode of transportation may be given recognition, its cost structure considered.

3. Taking the case history of new automobiles in interstate commerce, it appears that market pricing factors when given free play are capable of doing a pretty fair job in dividing business according to fitness and economy, at the same time preserving essential transportation services.

4. Any exercise of the minimum rate-making power by a regulatory agency inevitably results in a "government-created" division of the traffic between the carriers affected. This follows, inasmuch as traffic flow inevitably adjusts itself to any given rate relationship prescribed.

5. The justification for minimum rate orders or restraints would appear to be limited to situations where a "destructive" rate war clearly promises to worsen rather than strengthen the contribution or net revenue position of the fitter mode of transportation, the end result being but to purposelessly shift burdens to other users of the service. A similar problem arises where no one carrier has clear inherent advantages (rates plus service) over another. This situation most often obtains when rail competes with rail or truck with truck. The primary concern here is with the financial soundness of the fitter carrier. Such carrier should not find its net revenues worsened by the operation of the minimum rate authority when compared to that obtainable if price cutting had been allowed to run its normal course.

6. Whenever the "equality of opportunity" concept is invoked by a regulatory agency in the exercise of its minimum rate-making power, this term should be used in the sense of creating an equality of opportunity to fully develop one's economic capacities.[3] The term should not be used, it is believed, in the sense of creating an equal opportunity to divide the traffic or to "keep everybody in business." Minimum rates, if prescribed, should be such as to clearly encourage the traffic to move by the fittest mode of transportation.[4]

The foregoing working principles assume the pursuit of fitness and economy with a single-minded purpose and a minimum of government intervention in the operation of free market forces. There are, however, certain other considerations to be faced, including a reconciliation of fitness and economy with other parts of the national transportation policy.

One practical problem is the adequacy of the information, on costs in particular, upon which inherent advantages as between modes of transportation are determined. Another is the degree of restraint (and respect for out-of-pocket costs as a rate floor) which competing carriers or modes of transportation can be depended upon to voluntarily exercise in their rate cutting. (To the extent that any traffic fails to make the full contribution of which it is economically capable under the marginal revenue concept, it unjustly forces rates upward on other

[3] For a discussion of this theme, see *Big Enterprise in a Competitive System* (Brookings Institution, 1954), A. D. H. Kaplan, p. 40.

[4] In I. & S. 6172, Manufactured Tobacco, Va. and N.C. to Official Points (7/19/54), Division 2 permitted trucks to meet only the rail rate based on 36,000 pound minimum, but not the lower rail incentive rates. This seems to be an important precedent in the recognition of inherent advantages.

traffic upon which the carrier believes it has a monopoly.) The volume of the traffic now being handled at, or below, long-run out-of-pocket costs is disturbing.

Still another issue raised, and one going to national policy, is the consideration which should be given to the need for maintaining some surplus in transportation services and facilities for purposes of defense or simply to sustain some minimum measure of interagency competition. While these and other considerations no doubt are to be weighed, there does not appear to be any reason why they need deter an effective recognition of inherent advantages.

APPENDIX

U.S. RAIL REVENUE CONTRIBUTIONS ABOVE OUT-OF-POCKET COSTS—1951

Commodity Classes	Aggregate Dollar Contribution Above Out-of-Pocket (U.S. Total) (000)	Revenue as a Percentage of Out-of-Pocket Costs
1	2	3
Individual Commodity Classes		
Soybeans	$ 14,669	215
Soybean oil cake	4,835	127
Lettuce	6,933 Def.*	83
Cattle, calves, single deck	8,954 Def.*	83
Meats, fresh, NOS	19,404	134
Bituminous coal	286,494	135
Ore concentrates	4,112	131
Iron ore	16,702	109
Petroleum crude	598 Def.*	96
Logs, butts, bolts	10,511 Def.*	60
Lumber, shingles, lath	110,665	145
Gasoline	14,584	133
Chemicals, NOS	64,258	210
Furnace, slag	493 Def.*	96
Manufactured iron and steel	179,997	225
Explosives	72,988	681
Cement, Portland	65,906	213
Liquors, alcoholic	13,167	311
All carload commodities	2,578,356	150
Commodity Groups		
Products of agriculture	266,370	134
Animals and products	38,392	117
Products of mines	424,747	127
Products of forest	119,501	129
Manufactures and miscellaneous	1,677,697	181
Forwarder traffic	51,649	162

* Out-of-pocket loss. SOURCE: Columns 2 and 4 from ICC Cost Section Statement 7–53, December, 1953. Column 2 figures expanded to 100 per cent.

NOTE: Commodity classes herein selected to illustrate some of the range in the relation of rates to long-run out-of-pocket costs.

DISCUSSION

E. W. CLEMENS: The distinction between marginal cost pricing as advocated by Professor Vickrey, price discrimination in which part of a firm's output is priced at marginal cost, and multiple-product production in which some few products are priced at or near marginal cost is essentially a distinction of degree and method rather than one of substance. Marginal cost pricing is a method by which production is carried to the point where marginal cost equals demand, the deficit, if any, being made up by subsidy. Price discrimination is a method by which production is carried to the point where marginal cost equals demand, the deficit, if any, being made up by a tax, in a manner of speaking, on those customers whose demand is more inelastic. Multiple-product production likewise tends to carry production to the point where marginal cost equals demand price in some limited number of markets. This is done by the normal management procedure of shifting resources between various products, restricting output on those products with a high inelasticity of demand, reducing prices and increasing output on those products with a high elasticity of demand, and "dumping" any idle capacity on the market by producing some products at a negligible profit margin; i.e., producing at marginal cost. (See E. W. Clemens, "Price Discrimination and the Multiple Product Firm," *Review of Economic Studies*, 1950-51, page 1.) In this connection, I might say parenthetically that Joan Robinson's two chapters on price discrimination represent a much more rational approach to the theory of the firm than the more conventional theories of monopolistic or imperfect competition, particularly since in all probability there is in our entire economy no firm which confines itself to a single product or establishes a single price for its output.

So complex are the production and demand functions that marginal cost pricing schemes seldom involve the establishment of a single price as the theory of Professor Hotelling originally assumed. Vickrey's paper contains numerous illustrations of this. In fact, the theoretical benefits of marginal cost pricing can be attained only under a multiple-pricing system. Thus marginal cost pricing and price discrimination both emerge as multiple-pricing systems. The difference lies in the fact that in one case prices are proportioned to some calculated measure of cost or marginal cost and in the other prices are graduated according to elasticity of demand. Professor Vickrey's method is the first and Dr. Edwards' the second. We thus have four general alternatives to average cost pricing: (1) subsidized marginal cost pricing at a single price, a case that can be ignored, (2) subsidized marginal cost pricing at a series of prices, (3) price discrimination without subsidy, and (4) multiple-product production.

Once this is established, marginal cost pricing does not appear as esoteric as it is sometimes made to appear. It represents the norm rather than a deviation from it. Despite the fact that I have been for a long time making this point, I think that the real problems are not the problems of theory but the practical

problems of application, many of which have been pointed out by Professor Vickrey and Dr. Edwards. Differences in view stem primarily from the differing significance we would attach to the many practical considerations involved.

Some of these considerations can be compressed into the single problem of determining marginal cost. The determination of marginal cost is a complex matter. The blithesome simplicity of Lerner's marginal cost concept in the *Economics of Control* must appear almost fantastic to anyone with the slightest familiarity with industrial techniques. Marginal costs in general and in specific situations are extremely difficult to measure and when calculated can be used only under elaborate and cumbersome assumptions. There is at the outset the problem of the proper units to be used (passengers, passenger-miles, cars, trains, etc.). Proper computation is difficult, notwithstanding Professor Vickrey's ingenious formulas. There is also the problem of the size of the price group. In many instances, the selection of one large enough for administrative economy precludes any of the gains of marginal cost pricing. A primary objective of practical rate making is that of keeping the number of groups as small as possible. There is the problem of joint costs and of the degree of jointness. There is the problem of determining and measuring peak demand (hourly, daily, yearly) and the significance that can be attached to random fluctuations of the peak. There are costs of entry and costs of withdrawal. There is the question of the distinction between short-run, long-run, and various series of intermediate-run marginal costs. Is depreciation a function of time or of use? Is it to be based on the past, present, or future costs? The incidental calculation of expected obsolescense resolves itself into a glorified guess at the future. Conceivably you might also have to guess at future interest rates. There is the problem of the proper share of administrative overhead to be included in marginal cost.

There is another consideration which to my knowledge has not been considered. The marginal customer is the customer who will first abandon service with any unfavorable change in circumstances. Service to this customer requiring the commitment of capital, or the incurring of fixed costs, carries with it a high degree of risk. This risk represents a concealed cost that is commonly ignored. The telephone industry, to which Professor Vickrey alludes as a possible further subject of marginal cost pricing, maintains that increasing saturation of telephone service has greatly increased the risks incident to the telephone industry.

The problems of demand are almost as knotty. Price is not an interpersonal measure of utility and there is question as to how far out of its way society should go to see that the marginal user receives service. One can question the social propriety of taxing citizens in New York to subsidize a deep freeze unit in Tennessee—or for that matter, the Aluminum Company of America, if such is the case. On the other hand, discriminatory pricing, or "maximizing the net," and to a lesser extent marginal cost pricing are established on the implied premise that the customer who needs the service most should be charged the highest price. For example, in the transit industry considerations of both demand elasticity and marginal cost would dictate the establishment of peak-time rates that would be highest for the customers least able to pay.

To some extent, the choice boils down to a price system where costs are recovered from users of service as a group, or from society at large through taxes. The ultimate and comparable incidence of a tax on selected users and a tax on society in general would be difficult to trace. I think it would be generally admitted that the efficiency of tax collection, all factors considered, would be pretty low, and the costs involved might well swallow any of the social benefits of marginal cost pricing. This would be particularly true if marginal costs, properly computed, were only slightly below average costs. Apart from this, discriminatory pricing and true marginal cost pricing appear to the customer as one and the same thing and are subject to somewhat the same afflictions.

The load accepted as the marginal load today may be the peak load of tomorrow. If service is at all uncertain, as it would have to be under strict marginal cost pricing, there would be a high degree of risk attached to any commitment to the service made by either buyers or sellers. Abandonment of service would be difficult without serious disruption of buyer economics. This would constitute an unseen element of marginal cost. Buyers are willing to pay a premium for certainty of supply, and producers will take less with certainty of demand. The administrative costs involved in establishing and administering a multiple-price system are considerable. Discriminatory pricing systems, whether based on costs or on elasticity of demand, create consumer resentment.

Opposition to discriminatory pricing policies may be based on something more than uninformed resentment. They may be injurious to the firm practicing them, to the competitors of that firm, and to the customers of that firm. If the weight of bargaining power rests with customers and particularly if the demand for the product of the entire industry is inelastic, the competitive battle between discriminating firms may ruin all, as in the early history of the railroad industry. If the discriminating firm is powerful, it may by a process of discrimination eliminate its competitors one by one, as the old Standard Oil Company once did. If some customers are larger or more powerful than others, they may ruin their rivals by extorting more favorable prices from price discriminating firms, as shippers have done in the past. These situations in multitudinous form have always been matters for public concern. It would be significant, but not necessarily controlling, that price differentials were or were not justified by costs.

I am not taking a stand against the position of either Professor Vickrey or Dr. Edwards, but I am saying that in almost all instances "other considerations" are more important than either "maximization of net" or "marginal cost pricing." Other considerations (to name only a few at random from many industries) include traffic control, urbanization, national defense, the maintenance of several sources of supply, preserving competition, conserving natural resources, and improving the general social and economic welfare of the community. The application of the marginal cost pricing principle, for example, to the utilization of exhaustible natural resources results in a serious perversion of social policy. The many considerations vary with each situation to such an extent as to preclude establishing general principles except in closely prescribed areas of the economy.

JAMES C. NELSON: Dr. Edwards states that relative economy and fitness within transportation are achieved "when the nation's traffic is so distributed among the agencies of transportation that the aggregate costs for all transportation (including shipper supplementary expenses) are lower than they would be under any other basis of distribution." With that concept of best allocation of resources within transport, I am in agreement. The emphasis here upon the historic problem of transport pricing for social economy echoes the fundamental and long-needed re-examination of that subject which carriers, shippers, the trade press, government agencies, and economists are now conducting both at home and abroad. Much good can emerge from this activity, but it should be recognized that pricing for a best allocation, in addition to dividing traffic according to relative cost and fitness, would also, so far as transport is a factor, have to locate agriculture, industry, markets, and population in such manner that no different locational pattern could reduce aggregate costs of production. (For relevant theory, see Tjalling C. Koopmans, "Optimum Utilization of the Transportation System," *Proceedings* of the International Statistical Conferences, Vol. V, 1947, pages 136-145; and Ralph L. Dewey, "Criteria for the Establishment of an Optimum Transportation System," AEA *Papers and Proceedings*, May, 1952, pages 644-653.)

Critical scrutiny must be given the main theoretical principle mentioned by Dr. Edwards as governing rate making. Carriers seeking to maximize their profits undoubtedly engage in mental arithmetic which, if extended to its logical conclusion, would equalize marginal revenues and marginal costs. Equalizing those things will clearly maximize profits where profits can be earned or minimize losses where profits cannot be earned. Assuming that the carrier's demand curve frequently is not horizontal and at different prices may have different elasticities, the rule urged by Dr. Edwards should receive close attention in pricing or in regulating to maximize profits, at least in the short run. The question which arises, however, is whether that rule is equally applicable in dividing traffic according to relative economy and fitness.

The railroads as a whole have not earned exceptional profits in the current high-level economy and they have returned to an excess capacity condition, particularly in passenger operations. More selective and competitive application of the marginal revenue-marginal cost principle—with its accompaniment in railroading, rate discrimination—might well increase volume and reduce excess capacity and also enhance profitability if care is taken not to quote rates below marginal costs. Assuming that most present rail facilities will be needed, to reduce excess capacity by incremental pricing to the extent that demand proves elastic would bring economy in use of durable and other transport resources to that extent. The added profitability (up to a point) would not seem objectionable in view of the demand for greater cost-reducing investment in improved motive power, special and larger freight cars, terminals for trailers on flat cars, new low-cost passenger trains of the Talgo type, and roadway improvement. Hence pricing effort should not only concentrate on demand as influenced by alternative transport agencies but also upon prices that will fully stimulate cost savings from volume and off-peak shipments.

However, as recognized by Dr. Edwards, whether carriers have adequate

knowledge of demand elasticities and of the behavior of costs to apply such pricing very perfectly must be faced. Though accounting and statistical data are favorable in transport, the long hesitation of the rails to quote truly low coach fares even though demand is elastic suggests that interpreting the demand curve is still fraught with uncertainty and risk.

Of greater concern in the problem at hand are the cases which justify doubt that a profitable application of the marginal revenue-marginal cost principle will always divide traffic according to relative economy and fitness. It cannot be overlooked that the existing traffic division has been influenced by the discriminating rate structure which Dr. Edwards has summarized. Thus, if the traffic division is failing to distribute traffic to the agency with the lowest long-term out-of-pocket costs, it might be well to look at the inducement which high discriminating rates, based on the marginal revenue principle, has offered to traffic movements by high-cost agencies.

After neglecting the discriminating effect in developing his example of how marginal revenue-marginal cost equality leads to a maximum revenue contribution, Dr. Edwards subsequently explicitly recognizes that rate discrimination will continue to result from marginal revenue pricing. In addition to bringing maximum justifiable profits, he implies that such discrimination will protect the existing regional pattern of industry. And he also implies that more enlightened discriminatory pricing may bring about an economic traffic division between road and rail. But does he recognize sufficiently the historical wastes that may in part be attributed to discrimination? And does he examine closely enough the cases where gaining the maximum revenue contribution may conflict with a redistribution of traffic to the low-cost agency?

Dr. Edwards properly notes the significant cases where railroads have been rendering services at less than variable costs; hence unprofitably if the cost assignment procedure is correct. If through improved pricing, new equipment, pooling of services, or more efficient management the unprofitable passenger and LCL services cannot be made to yield revenues above variable costs, they should be abandoned even under marginal cost pricing. So long as passenger trains are operated, every effort should be made to stimulate traffic as marginal cost must be extremely low. But it can be argued that the railroads' continued ability to render important areas of service at a loss and still make a profit on all operations has been dependent upon maintenance of high discriminating rates on important segments of traffic. Had the rate structure been less discriminating, perhaps shippers and consumers might not now have to pay millions additional freight revenues to support unprofitable services and society might have gained through an improved allocation of resources.

The possibilities that discrimination may underwrite an inefficient resource allocation do not end there. It is common knowledge that truck lines, particularly on long hauls, have concentrated on classes of traffic on which railroad rates are far above rail out-of-pocket or even fully distributed costs. Hence, to a considerable extent, the current problem whether to lower rail rates drastically below truck rates to encourage traffic flows by the low-cost agency has its origin in the great faith in discriminating pricing in the rail industry. This does not necessarily give cause for casting out discrimination, but it does raise

doubt that the particular discrimination pattern of the past can solve present-day problems. If there is some model of discrimination from marginal revenue pricing that would both maximize railroad profits up to the point that such profits are required and also shift the traffic between agencies so that it would flow over the least-cost agency more frequently than at present, that pattern should be articulated. (*Ibid.*, pages 144-145 and 648, respectively.)

Viewing the allocation problem as it exists today, the serious pricing questions concern the intimate details of how the low-cost agency carriers can reprice their services to encourage traffic for which their agency is low cost and to discourage traffic for which it is high cost (when rates are unprofitable).

Here, a conflict of interest inevitably arises: It concerns whether low-cost carriers would be better off profitwise without some of the competitive traffic and with high rates on remaining traffic than with all competitive traffic at rates low enough to attract it from the high-cost agency. Considering relative service advantages, it may often require marked reductions of present rates to achieve a shift in traffic to the low-cost agency. But if management thinks it more profitable with high rates and less traffic of a given kind, the profit-maximizing rule does not bring carriage of traffic, in the short or even considerably long run, by the agency with lowest marginal costs. Or if management decides to sacrifice present profits with rates low enough to shift all competitive traffic, then traffic flows by the low-cost agency but at low general profitability unless noncompetitive traffic can stand, and regulatory authorities will permit, very high rates upon such traffic.

One possibility for resolving this conflict is to reduce the low-cost agency's rates on competitive traffic as low as necessary, even as low as short-run marginal cost, and to subsidize low-cost carriers for any sacrificed revenue contribution up to a reasonable level. This method should effectively divert traffic to the low-cost agency except where service considerations are controlling and, if other classes of traffic continued to contribute existing revenues over variable costs, it would hold total subsidies below the level under across-the-board marginal cost pricing. Thus the advantages of some discrimination to minimize subsidy and some marginal cost pricing to facilitate relative economy would be combined. But this scheme has some obvious political and economic frailties. The railroad situation is hardly serious enough for public approval of this type of subsidy. And rates as low as marginal costs might not be required to attract traffic to the low-cost agency. Thus, traffic-diverting rates might make positive but varying contributions toward overhead costs and return. Hence, marginal revenue pricing with reasonable discrimination might be preferable after all—but only where it succeeds in shifting traffic to the low-cost agency.

The recent selective rate reductions suggest that in certain cases railroads must assume that enough traffic has been lost that it may pay them to reduce rates sufficiently to regain the traffic. In those cases, the demand curves faced by them may be horizontal down to a price under the high-cost agency's lowest price that will effectively divert the traffic. If the traffic-diverting rate is still above variable cost, it may pay the low-cost agency to lower rates and adhere fully to relative economy pricing. Below the traffic-diverting rate, low-

cost carriers might select rates that maximize the net above variable costs. But what about the many competitive situations in which railroads have not acted to reduce their rates drastically, such as in transcontinental markets with respect to high-rated manufacturers? Here traffic diversion mounts—on long hauls where mass carriers are thought to experience lower costs. Presumably it is not profitable for railroads to stop that diversion, though in time diversion may have a disastrous effect upon their traffic and profitability. In this difficult situation, the devil gets you if you do and he gets you if you don't!

It seems questionable that public support will develop for sufficient restriction of for-hire and private trucking to protect the security of the railroads in the numerous situations where they still carry such a large share of competitive traffic that it is more profitable to continue present discriminatingly high rates than to reduce them to stop diversion. Thus, it may pay the railroads to consider going all the way to a rate system that fully reflects their cost advantages and disadvantages in their rates. Professor Gilbert J. Ponsonby has proposed such a policy for Great Britain, where the railways have been freed of all regulation except maximum rates. Briefly, his proposal is that carload freight be quoted, regardless of commodity, a flat rate per car-mile (recognizing differences in cost occasioned by size of cars, etc.) plus a flat terminal handling charge per car irrespective of distance. Under this plan, rates would still taper and rates on light-loading commodities would be high and those on heavily-loading goods would be low. But discriminatingly high rates would no longer invite truck competition on dense routes where rail costs are lower and would no longer make it possible for rails to continue unprofitable services on light routes and over short hauls where truck costs are lower. Professor Ponsonby doubts that marginal costs are low enough under average costs to make low discriminating rates profitable except in joint-cost cases of return hauls and off-peak travel. He believes such a simplified rate system would automatically shift traffic in accordance with relative economy and fitness and that railroads would still command adequate traffic volumes to operate profitably and at satisfactory utilization rates ("Towards a New Railway Charges Policy," *Journal of the Institute of Transport*, September, 1954, pages 427-433).

This sweeping junking of historic policies may be regarded as too drastic for this country where regional economies have been built upon low value-of-service rates. And some economists will argue that adoption of cost rates would reduce utilization to unprofitable levels even in a growing economy. But at a time when traditional pricing and regulation is under attack and after years of muddling without a solution to the relative economy and fitness problem, it would seem appropriate to consider all major alternatives. (See the comment on page 27 of the *Railway Age*, November 29, 1954, that the Ponsonby "proposal is one that certainly has enough merit in it to warrant a lot of study and discussion.")

Whatever rate policy emerges, vigorous action on several fronts is essential. Otherwise, railroads may face the bleak prospect of continued traffic erosion, higher unit costs from underutilization, higher rates on traffic still wedded to the rails, eventual diversion of that traffic and a continual repetition of that vicious circle, with dismal long-term disinvestment and unprofitability effects.

Fortunately, some encouraging signs of rail action to avoid this outcome exist, such as the selective rate reductions under way, the rapid introduction of trailer-on-flat-car pricing (flat rates on trailers reflecting the rail cost advantage for line hauls), interest in low-cost passenger trains, and improvements in cost control. In addition to such moves, the public must be prepared to have government do its part by assessing adequate user fees for all public transport facilities, by reasonably adjusting regulatory standards, and by insisting upon allowing transport markets to do their important allocation work.

With Dr. Edwards, I believe that rivalry is sufficiently vigorous and widespread that the inherent advantages of all agencies will get a real working out under the competitive organization of transport. Unless a case can be made for the transport company approach, there can be no turning back from recent market developments, only a more enterprising prosecution of market factors—both cost and demand—in pricing.

AMERICAN ECONOMIC ASSOCIATION

PROCEEDINGS

OF THE

SIXTY-SEVENTH

ANNUAL

MEETING

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DETROIT, MICHIGAN
DECEMBER 28-30, 1954

PROCEEDINGS OF THE AMERICAN ECONOMIC ASSOCIATION

ANNUAL BUSINESS MEETING, DECEMBER 30, 1954
HOTEL STATLER, DETROIT, MICHIGAN

The Sixty-seventh Annual Business Meeting of the Association was held in the Wayne Room, Hotel Statler, December 30, 1954. The meeting, which was well attended, was called to order at 5:00 P.M. by President Simon Kuznets. After his introductory remarks, outlining the order of business, Professor Kuznets called for the approval of the minutes of the business meeting of December 30, 1953, and the ratification of the Executive Committee actions and acceptance of the reports of the Association's officers and committees, all of which are published in the "Proceedings." A formal vote of approval was passed.

The chief events of the year and major activities of the Association as well as the financial operations, investment holdings, and present status of the Association's finances were presented by the Secretary-Treasurer, James Washington Bell, and his report was accepted.

The Managing Editor of the *American Economic Review*, Bernard F. Haley, summarized the work of the Editorial Board and the Editorial Office in producing the *Review*. He noted some of the many problems concerning the size and contents of the *Review* and again called attention to the usefulness of his panel of advisers, whose names appear at the end of the Report of the Managing Editor.

Only the work of two committees was selected for presentation at this meeting; namely, that of the Committee on Economics in Teacher Education and the *Ad Hoc* Committee on the Implementation of the Bowen Report. Because these matters are of general concern to our members, the business meeting was considered an appropriate time to publicize them. A brief discussion of the relation of the Association to economic education from the floor followed the presentation of these reports. Brief mention was also made of matters relating to the status of the profession. Academic freedom in its various aspects continues to be of concern to the officers of the Association. Attention is being given to the subject of freedom of teaching, research, and publication.

Information concerning the work of committees may be found in comments in the Secretary's Report or reports of the committee chairmen. Reports of officers, committees, and council representatives are printed below, as follows: Reports of the Secretary (page 646); Treasurer (page 659); Finance Committee (page 662); Auditor (page 665); Managing Editor (page 671); of the Committee on Research and Publications, Simeon E. Leland, Acting Chairman (page 675); of the Committee on the Status of the Profession, I. L. Sharfman, Chairman (page 677); of the *Ad Hoc* Committee on Economics in Teacher Education, B. W. Lewis, Chairman (page 685); and of our representatives to the International Economic Association, H. S. Ellis (page

687), the Social Science Research Council, G. W. Stocking (page 689), the American Council of Learned Societies, F. H. Knight (page 692), and the National Bureau of Economic Research (page 694).

The Secretary reported that a preference ballot was being circulated with the membership dues bills, asking members to express their choice of time and place of future meetings beyond those presently committed.

The Committee on Elections report and the certification of the election of new officers for the year 1955 were presented by the Secretary, as follows:

I hereby certify, in accordance with the bylaws on election procedure, the results of the recent balloting and present the reports of the Nominating Committee and the Committee on Elections.

The Nominating Committee, consisting of Morris A. Copeland, Cornell University, Chairman, Howard R. Bowen, Williams College, Mary Jean Bowman, University of Kentucky, Ewan Clague, Department of Labor, Martin R. Gainsbrugh, National Industrial Conference Board, and William J. Fellner, Yale University, presented to the Secretary the list of nominees for the respective offices:

For President

John D. Black

For Vice-Presidents	*For Executive Committee*
Earl J. Hamilton	Milton Friedman
Abraham D. H. Kaplan	Everett E. Hagen
Paul A. Samuelson	Ruth P. Mack
George W. Terborgh	Lawrence H. Seltzer

The Committee on Elections (Frank G. Dickinson, American Medical Association, Chairman, S. Morris Livingston, Standard Oil Company, and James Washington Bell, Northwestern University) prepared biographical sketches of the candidates and ballots were distributed early in November. The canvass of ballots was made on December 13, 1954, and the results were filed with the Secretary.

From the report of the Committee on Elections, I have the following information:

Number of envelopes without names for identification	52
Number received too late	24
Number of defective ballots	—
Number of legal ballots	2,879
Number of returns from the mail ballot	2,955

On the basis of the canvass of the votes cast, I certify that the following persons have been duly elected to the respective offices:

President (for a term of one year)
John D. Black
Vice-Presidents (for a term of one year)
Earl J. Hamilton
Paul A. Samuelson
Members of the Executive Committee (for a term of three years)
Milton Friedman
Ruth P. Mack

The results of the election having been announced, retiring President Kuznets presented President-elect John D. Black, who responded with brief remarks. He expressed appreciation of the great honor and also of the heavy responsibility of this office. He remarked that his predecessors had set high standards of accomplishment and that only one with a strong sense of duty and obligation could hope to do as well in making the work of the economist important and useful in the world. Referring to his own field of specialization,

he reminded us that he will be the fourth president who has also served as president of the American Farm Economic Association, the others being T. N. Carver, E. G. Nourse, and J. S. Davis.

The lateness of the hour prevented discussion of any new business from the floor, and President Black called for the final item on the agenda; namely, the Report of the Resolutions Committee. The resolutions which follow were read by Professor Blodgett and were approved unanimously:

The Association is greatly indebted to President Simon Kuznets, who bore the major responsibility for the preparation of the program of this annual meeting, and to the members of the Executive Committee and all others who so ably assisted him in this difficult and time-consuming task.

The Association expresses its thanks to Edward L. Cushman, Chairman, and Ruth C. Barnes, Secretary, of the Committee on Local Arrangements, and their several associates on this committee for their effective attention to the innumerable technical and practical problems associated with the holding of this large gathering. Special appreciation is also due to the various local institutions and agencies, governmental and private, including the Automobile Manufacturers' Association and General Motors Corporation, for their most helpful assistance in making arrangements for these sessions. The Association is grateful to the management and personnel of the Statler Hotel for ministering to the comfort of the delegates and visitors to these meetings and to the press and other agencies of communication for their coverage of the proceedings.

Acknowledgment is also made of the co-operation of the ten other professional organizations, meeting simultaneously in this city, in the making of a co-ordinated and constructive joint program.

Finally, the Association extends its sincere thanks to its able Secretary, James Washington Bell, and his staff for their continued and effective contribution to the arrangement of the annual meetings over many years.

Ralph H. Blodgett, *Chairman*
Archibald M. McIsaac
James C. Nelson

The meeting was adjourned at 6:00 P.M.

JAMES WASHINGTON BELL, *Secretary*

REPORT OF THE SECRETARY FOR THE YEAR 1954

The official acts of the Association during the year are recorded in the minutes of the Executive Committee. These are presented herewith, followed by a summary of the year's operations and activities, with comments and interpretations.

Minutes of Executive Committee Meetings

1. Minutes of the spring meeting held in Rye, New York, April 2-3, 1954:

The *second meeting* of the *1954 Executive Committee* was held at the Westchester Country Club, Rye, New York, April 2-3, 1954. The following were present: Simon Kuznets, presiding, and J. W. Bell, Roy Blough, K. E. Boulding, Gerhard Colm, B. F. Haley, L. R. Reynolds, Arthur Smithies, and G. J. Stigler. Absent were: N. S. Buchanan, C. B. Hoover, J. H. Williams, and D. M. Wright. Attending as members of the Nominating Committee were: H. R. Bowen, M. J. Bowman, Ewan Clague, M. A. Copeland, and W. J. Fellner (M. R. Gainsbrugh absent); and as guests, Pendleton Herring, F. H. Knight, and J. P. Miller.

1. *President's Remarks* (Simon Kuznets). The meeting was called to order at 10:00 A.M. Professor Kuznets outlined the agenda and the procedure to be followed.

2. *Minutes*. The minutes of the December 28 and 30, 1953, meetings were reviewed and, with minor corrections, were approved as presented in page proof.

3 and 4. *Reports of the Secretary, the Treasurer, and the Finance Committee* (J. W. Bell). Although the final report of the Local Arrangements Committee had not yet been received, enough details were at hand to enable us to appraise the results of the Washington meeting. Attendance was especially large. All available facilities were taxed to the full, and despite a little friction here and there, the arrangements proved highly satisfactory. Financial results exceeded expectations. Our experience was reviewed in the light of the prospective problems we face in planning the Detroit meeting.

Membership statistics were discussed and some concern was expressed at the regressive or flattening-out character of these figures, for both members and subscribers. The Secretary presented possible explanations of these curves. Our exchange and complimentary lists were reviewed and the Secretary's proposal to revise our complimentary subscribers list was approved.

The *Papers and Proceedings* volume for May, 1954, promises to be even larger and costlier than last year's volume. Plans were discussed for publishing an annual supplement to the *Handbook* and for a revised *Directory* in 1955 or 1956.

The financial results of the "Readings Series" and other sponsored publications were reported, and the Secretary was authorized to go ahead with plans to co-operate with Richard D. Irwin, Inc., in disposing of surplus inventory of Volumes I-V of the "Readings Series."

There were no significant changes to report in financial conditions since December nor in our investment portfolio. At present, we are in a strong cash and investment position.

A summary of committee activities indicated the status of standing and *ad hoc* committees upon which reports were forthcoming or, in the case of committees which have been discharged, where action might be called for.

5. *Report of the Managing Editor* (B. F. Haley). Professor Haley spoke briefly about the work of the Editorial Board, the flow of manuscript, the exchange list, and his plans to use foreign correspondents. He submitted a panel of names from which two appointments are to be made at the December meeting to fill the vacancies that will be created on the Editorial Board by the expiration of the terms of J. S. Bain and H. M. Somers.

6. *Reports of Standing and Special Committees.*

6*a*) *Research and Publications* (J. P. Miller). The following items, which had been taken up by this Committee at a meeting held in New York City, March 28, were discussed and where action was taken, it is so indicated:

Progress was reported on the fiscal policy volume of the "Readings Series" (Arthur Smithies and J. K. Butters, Editors).

The William Jaffé translation of Walras is now in the hands of the printer and the English edition should be out soon. The American edition will be announced for fall distribution.

The Committee recommended that the George Allen & Unwin, Ltd., proposal to undertake the translation of Silvio Gesell's *The Natural Economic Order* be rejected. The Academic Reprint proposal of joint sponsorship of a reprint of the Hull edition of the *Economic Writings of Sir William Petty* was also rejected. It was agreed that such a reprint of Petty would be desirable but that the market would not justify our becoming financially involved.

Proposals to compile economic bibliographies were considered, and there was agreement with the Committee's conclusion that general bibliographies should not be undertaken but that exceptions might be made for special bibliographies in connection with other projects sponsored by us; for instance, in connection with the proposed project on economic growth and development (see below).

On recommendation of Professor Miller's Committee, it was agreed that the Executive Committee authorize the preparation of a review article on bibliographical sources for research in economics which would be published in the *American Economic Review*, either as an article or a supplement. Reprints of this annotated "bibliography of bibliographies" would then be made available for the use of librarians and graduate students. An honorarium of $300 was appropriated to be used if necessary in order to get this job done.

To facilitate the use of the *American Economic Review* and the *Papers and Proceedings*, the Committee recommended that a cumulative index, by author and subject, of Volumes I-XLIV be prepared and that we contemplate future cumulative indexes every decade or so. The Committee was asked to draw up plans and estimate costs of this project, with the aid of B. F. Haley.

A proposal that the Association embark on a series of republication volumes on economic growth was discussed at length and the consensus seemed to be that the Committee be encouraged to reformulate its proposals, not along the line of publishing a series of six volumes, but of presenting a prospectus of one volume of readings or possibly two. This proposal, in effect, would widen the purposes of the Committee in that it involves not only the encouragement of research efforts but their direction as well. The Committee was asked to report a detailed plan for presentation at the December meeting.

In connection with the operations of the SSRC, Professor Miller and Dr. Pendleton Herring reported that the small research grants program for which Ford Foundation support was sought had become part of a larger move, involving the several social sciences. Also, the proposed handbook on the use and application of empirical techniques in economic research is now a project of the Agricultural Committee of the SSRC.

New appointments to the Committee were made as follows: R. A. Gordon, to succeed G. W. Stocking (term, 1955-57); M. F. Millikan, to fill the vacancy due to expiration of S. E. Leland's term (term, 1954-56); and Alexander Gerschenkron as an additional member of the Committee (term, 1954-56).

An additional sum of $500 was appropriated for operating expenses of the Committee.

6b) International Co-operation; the IEA (Gottfried Haberler). A communication from Howard S. Ellis was read. Professor Ellis, President of the IEA, requested the Association's recognition of an IEA-UNESCO volume on *Recent Trends in Economics*, embracing five or six essays prepared by representative authors familiar with trends of economic research in their respective countries. Under pressure for prompt action, Professor Ellis invited Rutledge Vining, of the University of Virginia, to prepare the essay on recent trends in economics in the United States. He did this in consultation with colleagues accessible at the time and now asks the officers of the AEA to express their approval of the selection of the author and requests, furthermore, that the President of the AEA write a short introductory note to this essay. No formal action was taken on this request, but the Secretary was instructed to write Professor Ellis, suggesting that his appointment of the author of the essay in this volume is in keeping with the implicit powers delegated by the Association to its representative to the IEA but that the preface to the article be revised so as to indicate merely that the author has been selected by the President of the IEA and the AEA delegate to that organization on his own judgment and responsibility.

The IEA-UNESCO sponsored report, *Teaching of Economics*, should soon be ready for distribution.

It was VOTED to ask a small *ad hoc* committee to prepare an appraisal of the activities of the IEA and of the AEA's participation in the international organization and report its findings to the Executive Committee at its next meeting.

It was VOTED to continue for another year the annual support of the IEA in the amount of double the annual dues; namely, $400.

The next IEA conference will be held at Seelisburg, Switzerland, in August-September, 1954, and the subject will be on wages. L. G. Reynolds, J. T. Dunlop, and Clark Kerr have been invited to attend.

The method of selection of AEA representatives to IEA was discussed. The general sentiment seemed to favor a year-to-year appointment rather than the six-year terms provided for in the IEA charter, but since the appointment of alternates is already provided for, no action was deemed necessary. (See *Papers and Proceedings*, May, 1950, page 594.)

6*c*) *Honors and Awards* (N. S. Buchanan). No formal report was received from the Committee. After a protracted discussion of the problem of selecting candidates for the J. B. Clark award, it was agreed that we should ask the Committee to reconsider its procedure and it was suggested that sufficient preparation be made in advance of their December meeting to enable them to complete a panel of nominees from which definite recommendations can be submitted to the Executive Committee at the time of its spring meeting in 1955. No award will be made in 1954, and the next award will follow the original schedule.

Professors J. Douglas Brown and Jacob Marschak were selected to succeed Professors G. L. Bach and E. S. Shaw at the expiration of their terms at year-end, 1954.

6*d*) *Foreign Honorary Members* (W. W. Leontief). No report was submitted by this Committee at this time. With a maximum limit of twenty-five foreign honorary members and twenty incumbents, there remain five vacancies. The Secretary was instructed to ask the Committee to submit the list of names under consideration, together with data concerning qualifications, in case they have no definite recommendations to make at our next meeting.

6*e*) *Nominating* (M. A. Copeland). At 8:30 P.M. the Executive and the Nominating Committees were constituted as an Electoral College. The report of the Nominating Committee was presented by Professor Copeland. After considering and agreeing upon the nominee for the presidency, the Secretary called and got acceptance from the candidate over long distance telephone. The panel of candidates for nomination to office for vice-presidents and members of the Executive Committee was considered and appointments were made of our representatives to ACLS and SSRC. It was ordered that the whole slate be published in the June number of the *American Economic Review*.

7. *Reports of Council Representatives.*

7*a*) *ACLS* (F. H. Knight). Professor Knight was selected for another four-year term as our representative. Time did not permit full discussion of the recent meetings of the Council and the Conference of Secretaries (held at Rye, New York, January 20-22). No significant changes in policy or new events need be added to the report submitted and published in the "Proceedings."

7*b*) *SSRC* (J. P. Miller). In addition to the report of Professor Miller, who described the routine operations of the Council and activities of its more important committees, we listened to Dr. Pendleton Herring describe plans and prospects of the Council, especially the Ford Foundation subsidized summer session training institutes and Rockefeller Foundation faculty research program. R. A. Gordon was selected to succeed G. W. Stocking upon the expiration of the latter's term at year-end 1954.

7*c*) *NBER* (J. H. Williams). No report.

8. *Annual Meetings.*

8*a*) Invitations for our 1956 and 1957 meetings were received from practically all large cities in the central and eastern states. It was VOTED to meet in Cleveland, December 27-30, 1956, and in Philadelphia, December 27-30, 1957.

8*b*) The problems of local arrangements for the Detroit meeting were briefly discussed. Among these problems are the degree of jointness or independence of our meetings, program, registration, exhibits, advertising, and the use of the US Employment Service again this year.

9. *Miscellaneous and Unfinished Business.*

9*a*) After listening to H. R. Bowen and J. P. Miller and the Secretary report on the Association's activities in relation to economic education, it was agreed that we should appoint an exploratory group (H. R. Bowen to suggest names) to study and report on continuation projects suggested by the reports on graduate training in economics, undergraduate teaching of economics, and our activities in co-operation with the Commission on Economics in Teacher Education.

Arthur Smithies, responding to President Kuznets' request in December, reported on the petitions recommending certification of economists. He read a number of letters in answer to his questionnaire and on the basis of this evidence recommended no action by our

Committee. No action was taken and the letters were filed with the Secretary.

9*b* and *c*) The Keezer *ad hoc* committee report recommending that a committee on public issues be not reconstituted was taken up along with the Wilcox report on the freedom of teaching, research, and publication in economics. President Kuznets agreed to follow up the latter committee's recommendation that a standing committee be appointed on the status of the profession, with a charge that it prepare an annual report which might involve not only questions of academic freedom and loyalty oaths but also representations on public issues and all other matters considered appropriate.

10. *New Business*. The balance of the meeting was devoted to the preparation of a preliminary draft of the program for the 1954 meeting.

The meeting was adjourned at 12:30 P.M., April 3.

2. Minutes of the Christmas meetings held in Detroit, Michigan, December 28 and 30, 1954:

The *third meeting* of the *1954 Executive Committee* was called to order at 10:00 A.M. at Hotel Statler, Detroit, December 28, 1954, Simon Kuznets presiding. Others present were: J. W. Bell, Roy Blough, Gerhard Colm, B. F. Haley, C. B. Hoover, L. G. Reynolds, and Arthur Smithies. Also present, as guests, were: J. D. Black, Ruth Mack, G. L. Bach, F. H. Knight, S. E. Leland, I. L. Sharfman, and G. W. Stocking. Absent were: K. E. Boulding, N. S. Buchanan, G. J. Stigler, and D. M. Wright.

The *first meeting* of the *1955 Executive Committee* was held on December 30, at 6:00 P.M., with President J. D. Black presiding. Others present were: J. W. Bell, Gerhard Colm, B. F. Haley, C. B. Hoover, and Simon Kuznets. Attending, as guests, were: Roy Blough, B. W. Lewis, L. G. Reynolds, and F. H. Knight. Absent were: N. S. Buchanan, Milton Friedman, E. J. Hamilton, Ruth Mack, P. A. Samuelson, G. J. Stigler, and D. M. Wright.

The account of the proceedings given below does not follow the chronological order of business but treats the sequence of items as they were listed on the agenda.

1. *President's Remarks* (Simon Kuznets). After the meeting was called to order, the items on the agenda were outlined and rearranged so as to take up first those matters representing business that should appropriately be acted upon by year-end 1954, leaving others on the table for consideration by the 1955 Executive Committee.

2. *Minutes*. The minutes of the April 2-3, 1954, meeting, held at the Westchester Country Club in Rye, New York, were approved, with minor corrections, in the form in which they had been distributed.

3 and 4. *Reports of the Secretary-Treasurer, the Finance Committee, and the Auditor* (J. W. Bell). Excerpts from the Secretary's Report were presented, summarizing the chief activities of the year and outlining the status of operations in progress; i.e., the annual meeting, publications, special projects, reports of committees and of council representatives. The Secretary was asked to report at the spring meeting his plans for a revision of the next *Directory*.

The financial condition of the Association was described. Several items on the summary statements of income-expense and balance-sheet accounts which were distributed called for explanation and discussion. Our investment holdings were reviewed, changes made during the year were explained, and our investment policy was discussed. Copies of the Auditor's Report were distributed for examination. Details of financial and investment operations may be found in the reports published below.

Present incumbents of the Finance Committee (Messrs. Osgood and Farnham) were re-elected for another year, as were the auditors, David Himmelblau & Co. The vote instructed the Secretary to extend to these members an expression of confidence and warm appreciation of their helpful services.

A resolution was VOTED to renew authorization of power to buy and sell securities on the part of the Secretary-Treasurer as nominee of the Association. The purpose of this resolution was to bring up to date an action taken March 21, 1936.

5. *Report of the Managing Editor* (B. F. Haley). A report was presented (see below) from which certain items were submitted for discussion; i.e., the flow of manuscripts and the size and content of the *Review*, reviews of foreign books and the use made of foreign correspondents, aid sought from a panel of members to relieve those on the Editorial Board. Without formal vote, there was uniform expression by members of the Executive Committee favoring more latitude in the use of the Editor's discretion in enlarging the size of the *Review* some twenty to thirty pages. Such expansion would permit more pages for articles and reviews of selected foreign books.

The appointment of Carl Kaysen and R. B. Goode for three-year terms was APPROVED. They succeed J. S. Bain and H. M. Somers, whose terms expire.

6. *Reports of Standing and Special Committees.*

6*a*) *Committee on Research and Publications* (S. E. Leland, acting for J. P. Miller). The Committee met in New York City March 28, 1954, and again during the annual meetings at Detroit. S. E. Leland summarized the status of our publication projects, i.e., the "Survey," "Readings," and "Translation" series, and outlined projects under consideration by the Committee (see report below). He reported that a bibliographical article for the *AER*, authorized at the April meeting, has been commissioned and that the Committee is studying B. F. Haley's helpful estimates on publication costs of a cumulative index of articles and papers of the AEA, 1911 to date. New proposals for a *Survey* Volume III on methodology or economics in action is under consideration, as are subsequent volumes in the "Translation Series." William Jaffé's project on the life and works of Walras was discussed.

At the present time, there is a balance of $644.74 available for the work of this Committee, plus the $300.00 appropriated for the economic bibliography article.

6*b*) *Ad Hoc Committee on Economics in Teacher Education* (B. W. Lewis). A brief report (see below) was read, describing the activities of this Committee and recommending that this Committee be continued as an *ad hoc* Committee on Economic Education and be instructed to prepare for the spring meeting recommendations on the need for and character of the appropriate scope of action by the Association on the different levels of education. The report was accepted with the deletion of "the teaching of economics at the college level and" in the last paragraph.

6*c*) *Committee on International Co-operation* (Gottfried Haberler); *International Economic Association* (H. S. Ellis). Due to exigencies beyond control, no reports were received from either Gottfried Haberler or H. S. Ellis. Our relations to the IEA were discussed and those present who had attended conferences of the IEA gave their appraisal of the value of these sessions and of the work of the IEA. Although no action was taken, it was agreed that our relations to IEA should continue as in the past but that our organization set-up should be revised, at least so far as our representatives are concerned. These members are appointed for six-year terms but in practice *ad hoc* representatives are selected from members who happen to be in Europe at the time. It would seem more logical to appoint delegates to special meetings. Action was delayed, pending receipt of report from H. S. Ellis, and it was suggested that copies might be prepared for circulation before the spring meeting if organization matters were considered in the report. The report, subsequently received and published below, indicates that the size of the IEA Council has been reduced and that the AEA is to select but two instead of four representatives and recommends that two of its present four representatives be appointed alternates rather than members and that thereafter only two members be appointed.

6*d*) *Committee on Honors and Awards* (N. S. Buchanan). G. L. Bach, reporting in N. S. Buchanan's absence, explained that the difficulties encountered by last year's Committee in submitting recommendations for honors and awards could not be resolved without reviewing the whole question of the desirability of making such grants at all. The Committee was asked to continue its work, perhaps reviewing the Slichter report published in the 1945 "Proceedings." When the awards were originally instituted, such misgivings as the Committee now holds were exhaustively discussed. The chairman will be expected to attend the spring meeting and report recommendations of the Committee on Honors and Awards.

6*e*) *Committee on Foreign Honorary Members* (W. W. Leontief). In the absence of W. W. Leontief, the one-page letter was read, recommending consideration of some representative of one of the underdeveloped countries for inclusion in our roster of foreign honorary members, and an additional representative from Germany. As possible alternates, the names of two prominent economists from other lands were added. Vitae of the principal candidates were attached. No action was taken. It was suggested that the field of candidates be broadened and that additional names be submitted for consideration at the spring meeting.

6*f*) *Ad Hoc Committee on the Status of the Profession* (I. L. Sharfman). The carefully prepared report was read (published below), reviewing the work of last year's Committee on Freedom of Teaching, Research, and Publication in Economics (see *Papers and Proceedings*, May, 1954, pages 733-737). This committee had recommended further exploration of the "need and scope of activity" for a standing committee on the status of the profession. In the light of the general situation concerning academic freedom and of the developments of the past year, the present Committee recommended such a standing

committee, with its scope of activity limited, for the time being at least, to the preparation and publication of reports. The Committee also suggested that members of the Association be afforded an opportunity to discuss crucial problems of academic freedom and civil liberties at annual meetings.

After a protracted discussion of the report, it was VOTED to discharge the Committee, with thanks, and the matter was left on the table for further consideration by the Executive Committee at its spring meeting.

6g) *Ad Hoc Committee on Implementation of the Bowen Report* (G. L. Bach). The Committee was not prepared to submit a final report, but G. L. Bach was invited to give an account of the present thinking of its members concerning the principal issues involved. It appeared that little agreement had been reached during preliminary discussions, although all members acknowledged our professional responsibilities with regard to the training of economists and all favored continued study of the recommendations contained in the report. The chairman promised to have a report in the hands of the members of the Executive Committee well in advance of the spring meeting.

6h) *Nominating Committee* (J. D. Black). As a preliminary step in the election process for next year's officers (see bylaws), J. D. Black discussed names of appropriate candidates for the new Nominating Committee. E. G. Nourse was appointed chairman of the 1955 Nominating Committee.

7. *Reports from Council Representatives.*

7a) *ACLS* (F. H. Knight). In view of the rather critical status of this Council with respect to financial support by the foundations, the general question was raised: What are the functions of the national organization devoted to the advancement of human studies at this time and what stake does the AEA have in its survival? F. H. Knight described briefly the origin and history of the Council, its accomplishments, our relations to it, and the circumstances leading up to the present situation. This was the subject of discussion at last year's Council meeting and promises to be the main item on this year's agenda. So far as financial aid was concerned, Frank Knight suggested that direct individual contributing membership would probably prove futile, as well as would solicitations for voluntary contributions from our membership. The matter was left in its present status; namely, to offer to pay our representatives' expenses to ACLS meetings.

Statements of the directors of the ACLS and of the SSRC and other councils, prompted by the Congressional committee investigation of tax-exempt foundations, were alluded to in this connection.

7b) *SSRC* (G. W. Stocking). No oral report was presented at the meeting but one is printed in the "Proceedings."

7c) *NBER* (J. H. Willams). A report is published in these "Proceedings."

8. *Annual Meetings.* The time and place of our commitments for subsequent annual meetings were reviewed and confirmed: 1955, New York, New York, Hotel Commodore (joint); 1956, Cleveland, Hotel Cleveland (independent); 1957, Philadelphia, Bellevue-Stratford Hotel (independent); and 1958, Chicago, Palmer House (joint). All meetings are scheduled for December 28-30, except for Chicago, 1958, where dates have been shifted to December 27-29 in order to avoid a conflict with another large organization meeting at the Palmer House December 30-January 1.

The Secretary reported that liaison is maintained with others of the allied social science group at an annually scheduled breakfast conference. He also reported that George Garvy, of the Federal Reserve Bank of New York, had accepted our invitation to serve as chairman of the Committee on Local Arrangements for New York, 1955. A questionnaire slip is being sent to our members along with dues bills in order to poll the sentiment with respect to preferred time, place, and character of meetings after present commitments have run out. The last survey of this kind was conducted in 1945. This mail poll will be processed and results reported at the spring meeting.

9. *Miscellaneous Unfinished Business.* A letter from K. E. Boulding again raised the question of our affiliation with the AAAS. Time did not permit a review of this perennial matter.

A discussion of subjects for new AEA activities was postponed.

The balance of the meeting was devoted to a discussion of program plans for 1955. During J. D. Black's absence (mission to Pakistan), certain members of the Executive Committee agreed to take initial steps in organizing segments of the program.

The meeting was adjourned at 8:00 P.M.

Activities and Operations

Although no spectacular events marked this year's developments, the year has been one of steady growth and much has been accomplished. Our membership, though increasing at a regressive rate, includes the great bulk of formally trained economists and we maintain our position as the largest and strongest learned or professional society in our field. This position we enjoy by virtue of services rendered to our members in the form of meetings, publications (our own and cosponsored ones), committee activities, and collaboration with other groups at home and abroad—groups having common interests and purposes.

The annual meetings in Detroit were well attended, registration of our own membership totaling 1,175 and a gross paid registration of all associations totaling 1,636. (For statistics of past meetings, see 1953 *Handbook*, page 173.) Accommodations were adequate and the excellent management by a capable local arrangements committee insured smooth operations in all departments. The quality of papers presented maintained the high standards of the past—a judgment which will, we hope, be confirmed by a favorable reception of the May *Papers and Proceedings*. Already plans for the 1955 meeting are beginning to take shape.

The Managing Editor of the *Review* reports a continuous flow of high-quality manuscript and asks authority to enlarge the size of the four big issues of our quarterly publication to permit more space for main articles and for reviews of important foreign books. Except for a twelve-page supplement to the 1953 *Handbook*, no supplementary reports were mailed to our members this year. However, our "consumers" are well treated and get ample money's worth for dues and subscriptions paid. No increase in the present $6.00 charges is contemplated.

Standing Committees on Research and Publications, International Co-operation, Honors and Awards, and Foreign Honorary Members continue to function actively and *Ad Hoc* Committees on Economics in Teacher Education, the Status of the Profession, and the Implementation of the Bowen Report have been investigating the feasibility of our promoting or developing new lines of activity. Only brief references will be made in this report, since the reports of the committees themselves appear below and the minutes of the Executive Committee are reported more fully than usual because of the unavoidable absence of many of its members.

Annual Meetings. Commitments for time and place of future meetings have been made through 1958 (see minutes of December, 1954).

Membership. Exhibit II following this report shows total membership of 7,500 and subscribers of nearly 3,000. The net gain of 200, following a net gain of about the same number as last year, would seem to indicate that we have reached a plateau of growth. We are not satisfied with this record and believe that greater effort could be made to enroll more new members. Many of our younger colleagues as well as older members of the staff have for reasons of inertia or other not performed what should be considered a logical professional function. College and university catalogues, publishers' mailing lists, the National Roster of Scientific and Specialized Personnel, the recent survey of highly

trained personnel, and other sources (see Secretary's Report, May, 1951, page 769, and 1953, pages 566-567, and the "Report on Graduate Education in Economics," pages 22-25) provide evidence to show that there are hundreds, perhaps many hundreds, of logical prospects, many of whom need only to be solicited to induce them to become members. A membership drive might do the business but much can be accomplished, as was indicated in this report last year, by co-operative effort on the part of members. We suggest that you ask for copies of our information booklet and that you distribute to those interested.

Geographical Distribution. The distribution of members and subscribers remains much the same. (See May, 1952, *Papers and Proceedings*, page 720, for brief analysis and reference to previous counts and the 1953 *Handbook*, pages 180-181, for latest published figures.)

Publications. The Association publishes the quarterly *American Economic Review*, the annual *Papers and Proceedings* volume, and occasional supplements, directories and handbooks and an annually revised information booklet.

American Economic Review. The four issues of this year's volume totaled 1,232 pages. Total number of copies printed was 11,000 and the cost of publication $25,436.52. The Managing Editor's Report contains a detailed description of contents (articles, communications, reviews, notes, and the like), costs and the budget for 1955.

We continue a liberal policy in granting permission to quote and reprint materials both from the *Review* and the *Papers and Proceedings.*

The series of photographs and biographical sketches of past presidents has been completed; that is, we have caught up with current incumbents. It is proposed to continue the series until we have included all past editors, secretaries, and treasurers.

The section on "Vacancies and Applications" still serves a useful purpose, after fifteen years of experience.

Papers and Proceedings. The May, 1954, volume contains 765 pages, published at a cost of $13,932.96. We look forward to a slight reduction in the size of the current volume being prepared.

Handbook or Directory. An eleven-page supplement to the 1953 *Handbook* was mailed to our members in July of 1954. It contained names and addresses of new members added to our rolls between April 15, 1953, and June 1, 1954. The next edition will be a volume in the form of a "who's who" *Directory*, such as we published last in 1948, and plans are to issue this in 1955 or 1956.

The sixteen-page information booklet, revised annually, will soon be available for distribution to officers and to prospective members and others interested in learning about the purposes and activities of the Association.

Committee Activities. A roster of members of all standing and *ad hoc* committees is found at the end of this report.

Committee on Research and Publications (S. E. Leland, Acting Chairman). The report published below calls attention to the appearance of William Jaffé's translation of Walras, *Elements d'Economie Politique Pure.* This long-awaited volume is receiving very favorable attention. Volume VII of the "Readings Series," on *Fiscal Policy* (Arthur Smithies and J. K. Butters, editors), is now

completed and should be off the press shortly. The IEA-UNESCO volume on the *University Teaching of Social Sciences: Economics,* which the American Economic Association and the Royal Economic Society helped to underwrite, is now available at a preferred price to members (see note and advertisements in the *American Economic Review*). The volume contains articles on the content of economics curricula and methods of teaching in several foreign countries and should be of interest to all teachers of economics.

In order to clear out excess inventory of Volumes I-V of the "Readings Series," Richard D. Irwin, Inc., last fall announced a sale at reduced prices to members. The results have been gratifying. This is a special opportunity to fill gaps or to start a complete set of this series.

Ad Hoc Committee on Economics in Teacher Education (B. W. Lewis, Chairman). The report of this Committee (see below) raises a number of questions which are of interest to a large number of our members. For instance, should we, as a professional association, co-operate more directly with teachers associations in their efforts to improve the teaching of economics in secondary schools; should we encourage or recognize in some way or offer our facilities to aid in the important work of economic education being done in summer workshops; should we offer our facilities to help organize or give publicity to the numerous educational conferences conducted by business corporations and trade-unions. A 150-page booklet on *Economics in General Education,* which represents proceedings of a conference held at Riverdale, New York, August 22-September 3, 1954, has just been issued by the Joint Council on Economic Education. Some effort may be made to learn who amongst our members are interested in these questions. If enough interest is expressed to warrant the effort and expense involved, we may include an item on this subject in the next *Directory* questionnaire, or we may even prepare a special mailing.

Committee on International Co-operation (Gottfried Haberler, Chairman); *International Economic Association* (H. S. Ellis). H. S. Ellis, in a report published in the "Proceedings," gives an account of the IEA activities and certain changes in the organization that are taking place. Bulletin No. 2, 1954, of the Association Francaise de Science Économique, is almost entirely devoted to the description of the IEA summer conferences held since 1951, to its membership, organization, and publications. We have made previous note of the first three volumes of the *International Economic Papers,* published by Macmillan, London, but available in New York at a special price to members of the AEA. Volume IV is announced for publication in October, 1954, and Volume V is scheduled to appear in 1955.

Ad Hoc Committee on the Status of the Profession (I. L. Sharfman, Chairman). Attention should be called to the carefully prepared report of this Committee. (See minutes and report published below.)

Ad Hoc Committee on Implementation of the Bowen Report (G. L. Bach, Chairman). See minutes.

Reports of Council Representatives. Reports of our representatives to the ACLS, SSRC, and to the NBER (published below) should be of interest to our members.

Committees Appointed During the Year

COMMITTEE ON LOCAL ARRANGEMENTS
Edward L. Cushman, *Chairman*

COMMITTEE ON ELECTIONS
Frank G. Dickinson, *Chairman*
S. Morris Livingston
James Washington Bell, Ex Officio

Ad Hoc COMMITTEE ON IMPLEMENTATION OF BOWEN REPORT
George L. Bach, *Chairman*
Robert D. Calkins
Robert A. Gordon
John K. Galbraith
Albert G. Hart
O. H. Brownlee

Ad Hoc COMMITTEE ON THE STATUS OF THE PROFESSION
I. L. Sharfman, *Chairman*
Ben W. Lewis

NOMINATING COMMITTEE
Morris A. Copeland, *Chairman*
William J. Fellner
Ewan Clague
Martin R. Gainsbrugh
Howard R. Bowen
Mary Jean Bowman

Ad Hoc COMMITTEE ON ECONOMICS IN TEACHER EDUCATION (continued from 1953)
Ben W. Lewis, *Chairman*
Archibald McIsaac
Paul J. Strayer

Standing Committees and Representatives

COMMITTEE ON HONORS AND AWARDS
Norman S. Buchanan, *Chairman* (1956)
George L. Bach (1954)
Edward S. Shaw (1954)
Fritz Machlup (1956)
Edward S. Mason (1958)
Joseph J. Spengler (1958)

COMMITTEE ON FOREIGN HONORARY MEMBERS
Wassily W. Leontief, *Chairman*
P. T. Ellsworth
Paul A. Samuelson

COMMITTEE ON RESEARCH AND PUBLICATIONS
John P. Miller, *Chairman* (1955)
Simeon E. Leland, *Acting Chairman* (1954)
George W. Stocking (1954)
John H. Williams (1955)
Joseph J. Spengler (1954)
Jacob Marschak (1955)
D. Gale Johnson (1956)
Max F. Millikan (1956)
Alexander Gerschenkron (1956)
James Washington Bell

COMMITTEE ON INTERNATIONAL COOPERATION
Gottfried Haberler, *Chairman*
Howard S. Ellis
Theodore W. Schultz
James Washington Bell

INTERNATIONAL ECONOMIC ASSOCIATION REPRESENTATIVES
Gottfried Haberler (1955)
Howard S. Ellis (1955)
Milton Friedman (1958)
Max F. Millikan (1958)

SOCIAL SCIENCE RESEARCH COUNCIL REPRESENTATIVES
George W. Stocking (1954)
John P. Miller (1955)
D. Gale Johnson (1956)

AMERICAN COUNCIL OF LEARNED SOCIETIES REPRESENTATIVE
Frank H. Knight (1954)

NATIONAL BUREAU OF ECONOMIC RESEARCH REPRESENTATIVE
John H. Williams (1955)

Representatives of the Association on Various Occasions

WESTERN COLLEGE FOR WOMEN, CENTENNIAL COMMENCEMENT CONVOCATION
Paul J. Garfield

LINCOLN UNIVERSITY (PENNSYLVANIA) CENTENNIAL
Raymond T. Bye

DEDICATION OF AMERICAN BAR CENTER, CHICAGO
Earl J. Hamilton

VEREIN FÜR SOZIALPOLITIK MEETINGS, BAD NAUHEIM
Gottfried Haberler

COLUMBIA UNIVERSITY BICENTENNIAL COMMENCEMENT
Simon Kuznets

INAUGURATION OF UNIVERSITY AND COLLEGE PRESIDENTS:
Edwin Harold Rian, Jamestown College
Lyle J. Tyner
Owen Meredith Wilson, University of Oregon
C. Ward Macy
Carl Cluster Bracy, Mount Union College
Thomas O. Weir
David Hitchens Morgan, Texas A. and M. College
T. R. Hamilton
Herrick Black Young, Western College for Women
Joseph C. Seibert
William James Lord Wallace, West Virginia State College
Andrew J. Dadisman
Clifford Cook Furnas, University of Buffalo
Ralph C. Epstein

Use of Mailing List

The following were granted permission to use our mailing list to send the material indicated:

UNIVERSITY OF CHICAGO PRESS: To send promotional material on six books.

COMMITTEE FOR ECONOMIC DEVELOPMENT: To send prepublication notice of papers on problems in antirecession policy, and policy statements on "Managing the Federal Debt" and "United States Tariff Policy."

UNIVERSITY OF MICHIGAN, BUREAU OF BUSINESS RESEARCH: To advertise Business Studies, Business Reports, and Business Papers series.

NATIONAL ASSOCIATION OF MANUFACTURERS: To send *Educational Aids for Colleges, Instructor's Manual for Productivity*, and research study, *Productivity—Gauge of Economic Performance.*

The Business Outlook: To send trial subscription offer.

S. Millman: To send out circular on new books in economics and some specially priced books.

THE ANTIOCH PRESS: To send material regarding the book, *Wildcat Strike*, by Alvin W. Gouldner.

INSTITUTE OF LIFE INSURANCE: To send 1953 *Life Insurance Fact Book.*

COMMERCIAL CREDIT CORPORATION: To send Clyde W. Phelps's monograph, *Financing the Instalment Purchases of the American Family.*

AMERICAN ACCOUNTING ASSOCIATION: To send invitations to members to subscribe to *The Accounting Review.*

RUTGERS UNIVERSITY PRESS: To send circular on Professor Kurihara's *Post-Keynesian Economics.*

BOYCE MORGAN & ASSOCIATES: To offer 1965 business outlook study and introductory subscription to the *Report on the Business Outlook.*

KELLEY & MILLMAN: To send out notice of a book sale.

AMERICAN INSTITUTE FOR ECONOMIC RESEARCH: To send two technical bulletins and fellowship award announcements and application blanks and announcement of *Reconstruction of Economics,* by E. C. Harwood.

Respectfully submitted,

JAMES WASHINGTON BELL, *Secretary*

EXHIBIT I

PUBLICATION COSTS

PAPERS AND PROCEEDINGS				HANDBOOKS		
Year*	Number of Pages	Number of Copies	Cost	Number of Pages	Number of Copies	Cost
1930	222	4,300	$ 1,353.91			
1931	308	4,300	1,919.18	88	4,200	$ 589.54
1932	316	4,200	1,819.75			
1933	216	4,000	1,284.85	88	3,900	522.71
1934	232	3,700	1,192.91			
1935	248	4,000	1,347.88			
1936	360	4,200	2,037.90	58	4,100	454.36
1937	344	4,300	1,922.03			
1938	200	4,500	1,234.10	112	4,500	1,118.84†
1939	288	4,600	1,785.91			
1940	444	4,900	2,658.12	108	5,000	822.58
1941	479	5,200	3,294.45			
1942	548	5,400	3,909.79	208	5,500	1,775.72†
1943	535	5,500	3,652.56			
1944	470	5,800	3,350.40			
	144	5,900	1,215.22‡			
1945	536	6,400	4,502.84			
1946	960	6,700	8,149.90	143	6,900	2,035.71
1947	781	7,700	8,140.79			
1948	591	8,500	8,701.41	345	7,700	6,948.06†
1949	537	9,500	7,844.50			
1950	650	10,100	9,864.76	41	9,200	1,163.84†
1951	816	10,400	11,965.40	18	8,300	692.63†
1952	768	10,700	13,190.83	11	8,188	620.09†
1953	612	10,900	10,935.98	187	8,400	4,416.69
1954	765	11,000	13,933.00	11	7,900	660.06

* This is the year of publication and pertains to the meeting of the preceding year. The figures are published in the subsequent year.

† "Who's who" volumes; 1950—"Who's who" supplement; 1951, 1952, and 1954—names and addresses supplement.

‡ Part of papers presented at annual meeting published as supplement to June number.

EXHIBIT II

MEMBERS AND SUBSCRIBERS

Class of Membership	Totals 11/30/53	Added	Removed	Gain or *Loss*	Totals 11/30/54
Annual	6,682	620*	436§	184	6,866
Junior	409	296†	342*	*46*	363
Family	121	14	12	2	123
Complimentary	40	10	4	6	46‡
Life	62	7	—	7	69
Honorary	21	1	3	*2*	19
	7,335	948	797	151	7,486
Subscribers	2,844	512	438	74	2,918
Complimentary	30	5	28	*23*	7
Totals	10,209	1,465	1,263	202	10,411

* Includes 137 junior members changed to annual.

† Includes 20 annual members changed to junior.

‡ Includes 10 who do not receive publications.

§ Resigned, 82; nonpayments, 260; died, 22; lack of address, 52; changed to junior members, 20.

REPORT OF THE TREASURER FOR THE YEAR ENDING NOVEMBER 30, 1954

The financial results for the past year are shown in the following comparative statements. The first shows financial operations and the second balance-sheet conditions for the current year compared with last year and five years ago. These tables supplement the detailed report of the Auditor, which immediately follows the Report of the Finance Committee.

Financial Operations. In the table below, showing income and expenses, we group all sources of income together as well as all items of expenditure. In the Auditor's Report, operation and publication results are separated.

COMPARATIVE RESULTS OF OPERATIONS FOR 1949, 1953, AND 1954

	11/30/49	11/30/53	11/30/54
Income			
Membership dues	$31,121	$ 41,970	$ 42,991
Subscriptions	12,254	16,491	16,838
Sales	1,863	2,524	2,440
Advertising	7,775	8,019	7,543
Directory income	1,410	—	—
Republications income	1,000	—	—
Sundry income	—	63	845
Dues and publications income	$55,423	$ 69,067	$ 70,657
Interest	$ 1,117	$ 1,435	$ 1,621
Dividends	3,170	3,588	2,962
Less custodian fees	*126*	*179*	*176*
Sales of securities (net)	48	11,636	10,727
Investments (less fees)	$ 4,209	$ 16,480	$ 15,134
Total income	$59,632	$ 85,547	$ 85,791
Expenses			
Office salaries	$11,637	$ 15,251	$ 15,491
Other administrative expenses	3,275	3,630	4,479
Annual meeting	*470*	130	*1,766*
Executive Committee	1,700	1,939	1,268
Other committee expenses	864	1,039	342
Administative and operating expenses	$17,006	$ 21,989	$ 19,814
Review printing	$20,165	$ 24,292	$ 25,031
Papers and Proceedings printing	7,845	10,936	13,933
Handbook printing	—	4,018	1,059
Editorial office (*Review*):			
Contributors	1,667	1,562	3,015
Editorial and clerical salaries	7,243	9,843	9,953
Other expenses (net)	756	78	78
Publications	$37,676	$ 50,729	$ 53,069
Total expenses	$54,682	$ 72,718	$ 72,883
Net operating income or *loss*	$ 4,950	$ 12,829	$ 12,908
Appropriations	—	—	985
Net income or deficit	$ 4,950	$ 12,829	$ 11,923

Total income from all sources amounted to $85,791 for the fiscal year 1954, which was approximately the same as for the previous year. No significant changes are noted in the two-year comparisons. By coincidence, even profits on security sales were approximately the same. Growth factors account for present magnitude when compared with five years ago, but show some flattening out, at least so far as income is concerned. However, expenses show no marked shifts except for the unusual profit figure for the 1953 annual meeting and the increases in office and printing costs. Our net income this year was again especially large. Like last year, some non-recurring income accounted for this, as well as the lack of any extraordinary expenditures.

Financial Condition. The balance-sheet figures below show comparative status of the principal assets, liabilities, appropriated and other funds, and surplus account for the current year, last year, and five years ago. The especially liquid condition is accounted for by the investment shifts referred to in the Finance Committee report. Under liabilities, "Accounts payable" represents a more normal figure than the one for the previous year, which included estimates not only for the December number of the *Review* but also the *Handbook*. Appropriations exist at this time only for the Committee on Research and Publications.

COMPARATIVE FINANCIAL CONDITION FOR 1949, 1953, AND 1954

	11/30/49	11/30/53	11/30/54
Assets			
Cash on deposit and on hand	$ 7,456	$ 4,568	$ 12,041
Receivables (net)	3,422	3,671	21,479
Prepaid expenses and inventories	779	969	644
Furniture and fixtures (net)	675	1,817	1,639
Investments at cost:			
Bonds	33,109	68,308	61,519
Stocks	48,427	46,459	38,082
Total assets	$93,868	$125,792	$135,404
Liabilities and Funds			
Accounts payable	$ 6,080	$ 10,590	$ 7,467
Allied Social Science Associations	829	—	—
Deferred income	16,366	8,062	8,597
Membership extension fund	1,088	609	491
Fund for proposed secretariat	35	—	—
Sundry	—	—	10
Fund for Committee on Research and Publications	—	315	944
Fund for Committee on Public Issues	—	945	—
Committee appropriations (not expended)	1,877	—	—
Life memberships	4,325	7,025	7,725
Total liabilities and funds	$30,600	$ 27,546	$ 25,234
Surplus			
Balance at beginning of period	$58,219	$ 85,417	$ 98,246
Transfers from life memberships	100	—	—
Net income or *loss* for period	4,950	12,829	11,924
Unappropriated surplus	$63,269	$ 98,246	$110,170
Total footings	$93,869	$125,792	$135,404

Net worth or surplus after deducting liabilities and appropriations from total resources amounts to $98,246, which with the net income for the year of $11,924 gives us an unappropriated surplus of $110,170. This is, in a sense, the measure of our rainy-day fund, our insurance against adverse times, our resources for new undertakings and for our continued growth. With this backlog we can continue with assurance with our present modest $6.00 per annum dues and subscriptions while our contemporaries for the most part find it necessary to raise their dues.

The accompanying table shows changes in our investment portfolio for selected dates as well as the rate of return on these investments.

Respectfully submitted,

JAMES WASHINGTON BELL, *Treasurer*

INVESTMENT PORTFOLIO

Year	At Par	Cost			Market
	Bonds	Bonds	Stocks	Total	Stocks and Bonds
1925	$25,000	$24,661.75		$ 24,661.75	
1930	31,000	32,439.48		32,439.48	
1933	33,500	32,962.48	3,954.23	36,916.71	$ 31,522.50
1935	16,000	15,280.48	28,114.50	43,394.98	50,338.72
1940	25,000	22,519.80	41,155.95	63,675.75	60,553.88
1942	27,000	24,651.12	41,556.06	66,207.18	58,211.88
1945	40,000	36,705.95	44,955.81	81,661.76	103,574.76
1948	35,000	33,108.63	48,624.14	81,732.77	84,841.91
1950	35,000	33,108.63	51,978.53	85,087.16	104,177.27
1951	43,000	43,340.16	49,764.51	93,104.67	117,316.75
1952	42,000	42,312.67	58,934.00	101,246.67	130,836.02
1953	68,000	68,308.05	46,458.90	114,766.95	134,562.38
1954	61,000	61,518.63	38,082.20	99,600.83	132,280.63

RETURN ON INVESTMENTS

Year	Bonds	Stocks	Total	Rate of Return on Cost
1925	$1,350.00		$1,350.00*	
1930	1,695.21		1,695.21	5.22%
1933	1,679.49	$ 108.57	1,788.06	4.84
1935	1,022.96	680.70	1,703.66	3.92
1940	1,037.56	2,182.46	3,220.02	5.06
1942	1,306.49	2,186.17	3,492.66	5.28
1945	1,479.99	2,488.85	3,968.84	4.71
1948	1,194.85	2,944.31	4,139.16	5.06
1950	1,117.50	3,860.39	4,977.89	5.85
1951	1,026.30	4,607.67	5,633.97	6.05
1952	1,117.84	3,681.53	4,799.37	4.75
1953	1,435.12	3,587.45	5,022.59	4.36
1954	1,621.06	2,961.75	4,582.81	4.58

* Estimated income for year.

REPORT OF THE FINANCE COMMITTEE

December 23, 1954

Executive Committee,
American Economic Association,
Evanston, Illinois.

GENTLEMEN:

The following report of the investment holdings of the Association is for the fiscal year ending November 30, 1954. The exhibit shows the list of securities held by the Association at cost and at approximate market value at fiscal year-end. Changes made during the year are indicated and profit or loss on sales is shown.

During the past year, the following changes were made:

SUMMARY OF SECURITIES SOLD OR MATURED
Year Ended November 30, 1954

Shares or Par Value	Issue	Cost	Selling Price	Gain or *Loss*
	Sold			
50	Aluminum Corp. of America	$ 2,133.07	$ 3,591.73	$ 1,458.66
105	Olin Mathieson Chemical Corp.	4,713.73	5,838.70	1,124.97
50	Monsanto Chemical Co.	3,120.74	4,598.68	1,477.94
100	Pillsbury Mills, Inc.	3,653.78	4,755.40	1,101.62
75	Procter & Gamble Co.	2,459.72	7,030.05	4,570.33
16/100	Gulf Oil Corp.	—	7.40	7.40
$ 3,000	U.S. Savings Series G, 2½%, 2/1/54	3,000.00	3,000.00	—
4,000	Illinois Central R.R. Co., 4¼%, 8/1/66	4,026.17	4,100.00	73.83
15,000	U.S. Treasury Notes, 2⅛%, 12/1/53	15,000.00	15,000.00	—
5,000	U.S. Treasury C. of I., 2¼%, 2/15/54	5,000.00	5,000.00	—
14,000	U.S. Treasury Bonds, 3¼%, 6/15/83	13,991.25	14,918.75	927.50
12,000	U.S. Treasury C. of I., 2⅜%, 6/1/54	12,015.00	12,000.00	*15.00*
	Total	$69,113.46	$79,840.71	$10,727.25
	Bought			
50	Carrier Corp.	$ 2,990.61		
100	Olin Mathieson Chemical Corp.	4,713.73		
$ 6,000	Gulf Interstate Gas Co., 6%, 12/1/54	6,240.00		
20,000	U.S. Treasury Notes, 1¼%, 2/15/59	20,003.00		
5,000	U.S. Treasury Bonds, 2½%, 11/15/61	5,000.00		
15,000	U.S. Treasury Bonds, 2½%, 12/15/58	15,000.00		
	Total	$53,947.34		

Although but two formal meetings of the Finance Committee were held during the year, we had several informal conferences and were always within reach of each other by telephone. In May and November, we met to consider formal appraisals of our holdings and discussed investment policy at length. In general, our object was to maintain a fairly defensive position during a period of "doubtful recession" but in which stock prices were rising continuously. We started the year with the bond-to-stock ratio roughly 60:40 on cost and 50:50 on market value. Both in the spring and in the fall, we took profits on stock sales, investing the proceeds of these sales and those of maturing bonds in U.S. securities. By year-end, the 60:40 ratio of bonds to stocks was re-established on a cost basis, though the approximate market value on November 30, 1954, showed stocks worth $70,000-odd to bonds $60,000-odd.

LIST OF SECURITIES HELD BY THE ASSOCIATION

Stocks

Number of Shares of Common Stock	Issue	Cost	Approximate Market Value 11/30/54
50	Aluminum Co. of America	$ 2,133.08	$ 4,100.00
50	Carrier Corp.	2,990.61	2,825.00
200	Central and South West Corp.	2,801.69	6,075.00
100	Continental Can Co.	3,714.55	7,775.00
100	Eastern Air Lines	2,774.10	3,362.50
134	Gulf Oil Corp.	3,716.99	8,207.50
220	Household Finance Corp.	4,222.32	6,930.00
300	Houston Lighting & Power Co.	3,088.53	12,150.00
100	Kroger Co.	1,851.74	4,775.00
60	Peoples Gas Light & Coke Co.	7,466.15	9,300.00
110	Socony Vacuum Oil Co.	3,322.44	5,266.25
		$38,082.20	$ 70,766.25

Bonds

Par Amount	Issue	Cost	Approximate Market Value 11/30/54
$ 6,000	Gulf Interstate Gas Co., 6%, Interim Notes, due 12/1/54	$ 6,240.00	$ 6,300.00
20,000	U.S. Treasury Notes, 1¼%, Series "A-1959," due 2/15/59	20,003.00	19,931.25
5,000	U.S. Treasury Bonds, 2½%, due 11/15/61	5,000.00	5,042.19
8,000	U.S. Treasury Bonds, 2¾%, Series "B," due 1975/80	8,000.00	7,985.00
7,000	U.S. Treasury Bonds, 2½%, due 12/15/72–67	7,275.63	6,927.81
15,000	U.S. Treasury Bonds, 2½%, due 12/15/58	15,000.00	15,328.13
	Bonds	$61,518.63	$ 61,514.38
	Stocks	38,082.20	70,766.25
	Total	$99,600.83	$132,280.63

The above table shows that securities costing $69,113 were sold at $79,840, making a profit of $10,727. From the proceeds of about $80,000, we invested approximately $54,000, representing some shifts in stocks but mostly in government securities. The $20,000 shown as receivables on November 30 were invested a few days later in government securities, which means in final analysis that some $6,000 were taken out of the fund.

The exhibit accompanying the Treasurer's Report shows our investment holdings and the rate of return on cost over a period of years. The rate of return for the past year averaged nearly 5 per cent.

Respectfully submitted,

ROY C. OSGOOD, *Chairman*
C. WELLS FARNHAM
JAMES WASHINGTON BELL

REPORT OF THE AUDITOR

December 20, 1954

Executive Committee
American Economic Association
Evanston, Illinois

DEAR SIRS:

In accordance with instructions we have examined the accounts and related records of the American Economic Association for the year ended November 30, 1954, and now submit our report thereon together with the following exhibits:

Statement of Financial Position—November 30, 1954	Exhibit 1
Statement of Income and Expense for Year Ended November 30, 1954	Exhibit 2

Results from Operations

Net income for the year ended November 30, 1954, was $11,924 compared with net income of $12,829 for the year ended November 30, 1953, as shown in the following summary:

Particulars	Year Ended November 30 1953	1954	Increase *Decrease*
Income:			
Dues	$41,970	$42,992	$1,022
Interest and dividends (net)	4,844	4,406	*438*
Profit on sales of securities (net)	11,636	10,727	*909*
Miscellaneous income	63	845	782
Total income	$58,513	$58,970	$ 457
Expense:			
Publication expense	$50,729	$53,068	$2,339
Less—Publication income	27,034	26,821	*213*
Net publication expense	$23,695	$26,247	$2,552
Administrative and other operating expenses	21,989	19,814	*2,175*
Total expense	$45,684	$46,061	$ 377
	$12,829	$12,909	$ 80
Less—Appropriation	—	985	985
Net Income	$12,829	$11,924	$ *905*

The increase in dues reflects the increase in membership during the year under review, as reported by the Secretary:

Classification	Number of Members at November 30 1953	1954
Regular	6,682	6,866
Junior	409	363
Family	121	123
Life	62	69
Honorary	21	19
Complimentary	40	46
Total	7,335	7,486

Interest on bonds owned was accounted for in accordance with stated rates; dividends received on stocks were compared with amounts reported in published records of dividends paid.

Net publication expense, as shown in the following summary, amounted to $26,247 for the current year compared with $23,695 for the preceding year:

Particulars	Year Ended November 30 1953	1954	Budgetary Estimates for Year 1954
Expenses:			
Printing of—			
Review	$24,292	$25,031	$24,700
Handbook	4,018	1,059	
Proceedings	10,936	13,933	
Editor's honorarium	3,500	3,500	3,500
Payments to contributors	1,562	3,015	2,500
Editorial clerical salaries	6,343	6,452	6,550
Editorial supplies and expenses	493	713	600
Sundry publication expenses	*415*	*635*	
Total expenses	$50,729	$53,068	
Less—Income:			
Subscriptions, other than members	$16,491	$16,838	
Sales of copies	2,524	2,440	
Advertising	8,019	7,543	
Total income	$27,034	$26,821	
Net publication expense	$23,695	$26,247	

There was an increase of $2,552 in net publication expense (from $23,695 to $26,247) consisting of the following:

Increase in subscriptions	$ *347*
Decrease in copy sales	84
Decrease in advertising	476
Increase in expenses (net)	2,339
Net increase	$2,552

Billings for the December, 1954, issue of the *Review* and reprints had not been made by the publishers at the time of our examination. The publishers estimated the *Review* printing to cost $6,550 and the reprints $105 which amounts are included in the foregoing tabulation.

Financial Position

Condensed statements of financial position of the Association at November 30, 1953, and 1954 are compared below:

Assets	November 30 1953	1954	Increase *Decrease*
Cash on deposit and on hand	$ 4,568	$ 12,041	$ 7,473
Receivables (net)	3,671	21,479	17,808
Prepaid expenses	969	644	*325*
Furniture and fixtures (net)	1,817	1,640	*177*
Investments at cost—			
Bonds	68,308	61,519	*6,789*
Stocks	46,459	38,082	*8,377*
	$125,792	$135,405	$ 9,613
Liabilities, Funds and Surplus			
Accounts payable	$ 10,590	$ 7,467	$ *3,123*
Deferred income	8,062	8,597	535
Sundry fund		10	10
Membership extension fund	609	491	*118*
Fund for Committee on Research and Publications	315	945	630
Committee on Public Issues	945		*945*
Life memberships	7,025	7,725	700
Surplus—			
Balance at beginning of year	85,417	98,246	12,829
Net income for year	12,829	11,924	*905*
	$125,792	$135,405	$ 9,613

Cash on deposit was satisfactorily reconciled with balances confirmed directly to us by the depositories.

The receivables of the Association were not confirmed by correspondence with debtors. Based upon the Association's past experience the reserve for doubtful accounts appears to be adequate to cover normal losses.

Changes in the investment account were verified by the examination of brokers invoices and other supporting data. Securities held at November 30, 1954, were confirmed directly to us by the State Bank and Trust Company of Evanston, Illinois, custodian for the Association.

Insofar as we were able to ascertain, all liabilities of the Association at November 30, 1954, are reflected in the accompanying statement of financial position and the Secretary has represented to us that to the best of his knowledge all liabilities are disclosed.

A summary of the transactions in the various funds is presented hereunder:

Particulars	Membership Extension Fund	Sundry Fund	Public Issues	Committee on Research and Publications
Balance November 30, 1953	$608.96	$	$944.91	$314.56
Changes during year—				
Appropriations				985.44
Received		37.76		
Expended	*118.20*	*28.06*	*160.25*	*355.26*
Transferred to miscellaneous income			*784.66*	
Balance November 30, 1954	$490.76	$ 9.70	$	$944.74

We were advised that the Committee on Public Issues was discontinued. The unexpended balance was transferred to miscellaneous income.

We express our appreciation of the courtesies and cooperation extended to our representatives during the course of the examination.

Very truly yours,

DAVID HIMMELBLAU & Co.
Certified Public Accountants

EXHIBIT 1

AMERICAN ECONOMIC ASSOCIATION
STATEMENT OF FINANCIAL POSITION—NOVEMBER 30, 1954

Assets		
CURRENT ASSETS:		
Cash on deposit and on hand—		
State Bank and Trust Company, Evanston	$ 3,909.78	
National Bank of Commerce of Chicago	8,106.07	
Petty cash	25.00	$ 12,040.85
Receivables—		
Review advertising	$ 2,237.85	
Accrued interest and dividends	743.08	
Publication sales	330.98	
Membership dues	653.50	
Due on securities sold	17,624.15	
Sundry	247.05	
Total receivables	$21,836.61	
Less—Reserve for doubtful accounts	357.55	21,479.06
Inventory of stamps and envelopes		574.28
Unexpired insurance		69.60
Total current assets		$ 34,163.79
INVESTMENTS (at cost):		
Bonds	$61,518.63	
Stocks	38,082.20	99,600.83
FURNITURE AND FIXTURES (less accumulated depreciation)		1,639.54
Total assets		$135,404.16

Liabilities, Funds and Surplus			
CURRENT LIABILITIES:			
Accounts payable			$ 7,466.98
DEFERRED INCOME:			
Prepaid subscriptions	$ 7,317.38		
Prepaid dues	1,279.24		8,596.62
FUNDS:			
Membership Extension	$ 490.76		
Committee on Research and Publications	944.74		
Sundry	9.70		1,445.20
LIFE MEMBERSHIPS AND SURPLUS:			
Life memberships	$ 7,725.00		
Unappropriated surplus—			
Balance November 30, 1953	$98,246.53		
Net income for year ended November 30, 1954 (Exhibit 2)	11,923.83	110,170.36	117,895.36
Total liabilities, funds and surplus			$135,404.16

AUDITOR'S OPINION

Executive Committee
American Economic Association:

In our opinion, the accompanying financial statements present fairly the financial position of American Economic Association at November 30, 1954 and the results of its operations for the year ended that date, in conformity with generally accepted accounting principles applied on a basis consistent with that of the preceding year.

Our examination was made in accordance with generally accepted auditing standards and included such tests of the accounting records and other auditing procedures as we considered necessary in the circumstances.

Chicago, Illinois
December 20, 1954

DAVID HIMMELBLAU & Co.
Certified Public Accountants

EXHIBIT 2

AMERICAN ECONOMIC ASSOCIATION
STATEMENT OF INCOME AND EXPENSE
FOR THE YEAR ENDED NOVEMBER 30, 1954

Particulars		Amount	
INCOME:			
Dues—			
Regular, junior and family members		$41,812.36	
Subscribing and contributing members		1,179.00	$42,991.36
Investments—			
Interest on bonds	$ 1,621.06		
Dividends	2,961.75		
	$ 4,582.81		
Less—Custodian fees	176.34	$ 4,406.47	
Gain on sale of securities (net)		10,727.25	15,133.72
Miscellaneous income (net)			845.26
Total income			$58,970.34
EXPENSE:			
Administrative and other operating expenses—			
Secretary's salary	$ 3,500.10		
Office salaries	11,990.87		
Annual meeting (net)	*1,766.24*		
Executive committee expenses	1,267.87		
Other committee expenses	342.02		
Postage expense	889.26		
Stationery and supplies	1,532.33		
Insurance	183.11		
Provision for depreciation	187.25		
Telephone and telegraph	106.57		
Dues and expenses—			
American Council of Learned Societies	100.00		
International Economic Association	400.00		
Exchange on checks	52.05		
Social security taxes	288.22		
Miscellaneous expense (net)	739.88	$19,813.29	
Publication expenses—			
Printing of:			
Review	$25,031.03		
Handbook	1,058.75		
Proceedings	13,932.96		
Editors honorarium	3,500.00		
Payments to contributors	3,015.00		
Editorial clerical salaries	6,452.55		
Editorial supplies and expense	712.93		
Sundry publishing expenses	*634.64*		
Total publishing expense	$53,068.58		
Less—Publication income:			
Subscriptions, other than members	$16,838.32		
Sales of copies	2,439.61		
Advertising	7,542.87	26,820.80	26,247.78
Total expense			46,061.07
BALANCE			$12,909.27
LESS—APPROPRIATION FOR RESEARCH AND PUBLICATIONS			984.44
NET INCOME FOR YEAR ENDED NOVEMBER 30, 1954 (Exhibit 1)			$11,923.83

REPORT OF THE MANAGING EDITOR FOR THE YEAR ENDING DECEMBER, 1954

The editor of a professional journal is seldom content. Either the flow of good manuscripts is not as heavy as he would like or it is more than adequate, with the result that he is unable to find room for all of the papers worthy of publication. It is the latter situation that has confronted the Editorial Board of the *Review* this past year. Although the total number of manuscripts submitted has not increased as compared with 1953, the number of articles as distinct from communications has risen from 122 to 160. Furthermore, it is the general impression of members of the Editorial Board that the quality of the papers offered for publication has been noticeably better than in recent years. At the same time, the percentage of articles accepted has of necessity dropped to a very low 12.5 per cent, even though the space allocated to them was increased substantially. Many of the articles that could not be accepted have, however, found other publication outlets. Table 1 gives comparative figures with regard to manuscripts submitted for the past six years.

TABLE 1

MANUSCRIPTS SUBMITTED, 1949–54

	1954	1953	1952	1951	1950	1949
Manuscripts received	231	234	190	222	197	200
Articles	160	122	133	157	156	144
Communications	71	112	57	65	41	56
Percentage of articles accepted	12.5	15	21	19	19	20

As the table also reveals, the number of communications submitted this past year was much lower than in 1953. This, however, is not surprising, as the 1953 year was abnormally high, owing to the fact that there were included in the 1952 volume seven articles that turned out to be highly controversial. The flow of communications returned to something like a normal level this past year.

Table 2 provides a summary of the break-down of the volume as between articles, communications, book reviews, etc. Since the proportion of the volume devoted to articles was increased in response to the increase in the number of acceptable manuscripts submitted, space devoted to communications correspondingly contracted; and there was some reduction in the space devoted to book reviews (without, however, a corresponding reduction in the number of books reviewed). The other departments of the *Review* changed comparatively little.

We have continued the policy of slightly favoring articles that have some empirical basis. Of the thirty leading articles in the 1954 volume, I believe it

TABLE 2

SUMMARY OF CONTENTS, 1952–54

	1954		1953		1952	
	No.	Pages	No.	Pages	No.	Pages
Leading articles	30	481	18	389	28	428
Communications:						
Original	6	37	5	20	11	52
Comments and replies	11	42	28	112	8	25
Book reviews	159	301	161	339	173	349
Memorials	1	2	1	2	—	—
Classified list of new books		57		59		59
Classified list of periodical articles		53		54		44
Classified list of dissertations		27		32		46
Notes		38		34		34
		1042*		1044*		1037

* Includes blank pages.

would be fair to say that fourteen were based on empirical, or at least factual, research, while thirteen were essentially theoretical, and three review articles. With regard to subject-matter fields, there was the usual concentration in general economic theory—seven articles and three communications. This past year, however, there was also an unusually heavy concentration in the international field—eight articles and two communications. The remaining papers were rather widely distributed. Table 3 summarizes the subject-matter distribution for the thirty articles and seventeen communications.

TABLE 3

SUBJECT-MATTER DISTRIBUTION: ARTICLES AND COMMUNICATIONS, 1954

	Articles	Original Communications	Comments and Replies
General economic theory	7	1	2
Planning	1	—	—
Social accounting	2	—	—
Business fluctuations	1	1	3
Money and banking	1	—	—
Public finance	3	—	2
International economics	8	2	—
Industrial organization	1	1	—
Land economics	2	—	—
Labor	2	1	2
Population; social welfare	2	—	2
	30	6	11

The problem of book reviews continues to be troublesome. As in the past few years, many books that should have been given at least a short review notice have had to be passed over. I am told time and again by members of the Association that they find the book review section particularly useful, and that they would like to see more nearly complete coverage of the new books. Yet I do not believe that this should be done at the expense of the space devoted to

articles and communications; and the other departments have been reduced to about their minimum length if they are still to be useful. I have tried out this past year on a very limited scale the device of short summary notes prepared by graduate students in the case of less important books; but I am not satisfied that the solution lies in this direction. I have also reduced the average length of the book reviews accepted, by insisting on quite short reviews in some cases.

Another difficulty with the book review section has been the rather limited extent to which reviews of foreign books have been included. This is partly due to lack of space, but to a greater extent to the difficulty of learning about the books published abroad that are worthy of review. In order to begin to meet this problem, I have been authorized by the Executive Committee of the Association to appoint a number of foreign correspondents who will advise me from time to time about important books published in their respective countries, and who will, I hope, serve the *Review* in other ways as well. Thus far the following correspondents have been appointed, and have been assisting me this past year:

Alexandre Kafka (Brazil) — Victor L. Urquidi (Mexico)
Jean-Marcel Jeanneney (France) — P. J. Verdoorn (Netherlands)
Erich Schneider (Germany) — Erik Lindahl (Sweden)
Giovanni Demaria (Italy)

The increase in the number of reviews of foreign books that may be expected to result from this program will of course put further pressure on the available space for reviews; but some reallocation of space in favor of the work of the economists of other lands is surely desirable. It may, however, also be desirable to consider whether the Association can afford to increase the size of the *Review* about thirty pages per volume—the additional space to be devoted to reviews of foreign books.

Table 4 presents the actual expenditures in 1954 in comparison with the estimated budget and with actual expenditures in 1953. Partly because of the increase from 10,900 to 11,000 in the number of copies printed, our costs for printing and mailing are about $700 higher than the budgeted amount, and about $1,000 higher than last year. The other items are quite close to their budgeted amounts for the year. Because of the larger number of articles published, the payments to contributors were substantially higher in 1954 than in

TABLE 4
ACTUAL AND BUDGETED EXPENDITURES

	Budget 1954	Actual 1954	Actual 1953
Printing and mailing	$24,700	$25,436.52*	$24,365.57†
Editorial	3,500	3,500.00	3,500.00
Editorial assistance	6,550	6,602.76	6,366.88
Supplies	600	611.37	515.13
Contributors	2,500	2,488.50	2,059.00
	$37,850	$38,639.15	$36,806.58

* Printing estimate December number: $6,550 plus reprints $65.00.
† Corrected from 1953 Annual Report.

TABLE 5

COPIES PRINTED, SIZE AND COST OF PRINTING

	Copies Printed	Pages Net	Pages Gross	Cost Including Reprints
March	11,000	250	296	$ 6,243.10
June	11,000	260	296	6,008.93
September	11,000	280	320	6,569.49
December	11,000	252	320	6,615.00 (estimated)
		1,042	1,232	$25,436.52

1953, although not in excess of the budgeted amount. Table 5 shows the number of copies, size and cost of printing by quarters.

The estimated costs for the coming year are presented in Table 6, based on a volume of 1,000 pages and a printing of 11,000 copies.

TABLE 6

RECOMMENDED BUDGET FOR 1955

Printing (paper, postage, reprints, etc.)	$25,500
Editor's salary	3,500
Editorial assistance	6,600
Supplies	600
Contributors	2,500
	$38,700

During the year I have frequently sought the aid of members of the profession in addition to the hard-working members of the Editorial Board—partly to relieve the latter of some of the excessively heavy burden they have been carrying and partly to obtain advice of specialists in particular areas not represented on the Editorial Board. The following have assisted us in this way:

M. A. Adelman
A. A. Alchian
K. Arrow
G. L. Bach
A. Bergson
K. E. Boulding
K. Brunner
N. W. Chamberlain
L. V. Chandler
J. B. Condliffe
J. S. Davis
E. Domar
R. Dorfman
J. F. Due
R. Eisner
W. Fellner
M. Friedman
A. Gerschenkron
J. K. Galbraith
A. G. Gruchy
G. N. Halm
R. W. Harbeson
A. G. Hart
E. P. Hohman
E. M. Hoover
H. S. Houthakker
D. G. Johnson
W. O. Jones
N. Kaplan
F. Machlup
S. J. Maisel
R. Musgrave
W. H. Nicholls
R. V. Roosa
P. A. Samuelson
T. W. Schultz
T. Scitovsky
L. H. Seltzer
E. S. Shaw
J. J. Spengler
W. F. Stolper
B. Swerling
W. B. Taylor
G. Tintner
J. Tobin
H. F. Williamson

Respectfully submitted,

BERNARD F. HALEY, *Managing Editor*

REPORT OF THE COMMITTEE ON RESEARCH AND PUBLICATIONS

The Committee on Research and Publications presents herewith a report of progress and achievements for the current year:

1. The translation of Walras, *Elements d'Economie Politique Pure*, by William Jaffé, sponsored by the American Economic Association and the Royal Economic Society, has appeared in published form. The volume has been well received. Sales are going well. A number of reviews also appeared; more are scheduled for publication.

2. The preliminary draft of *Readings on Fiscal Policy*, under the editorship of Arthur Smithies and J. K. Butters, has been prepared and the final draft of the manuscript is being completed. The volume should appear in 1955.

3. An article on "Bibliographies in Economics" for publication in the *Review* is in preparation by Donald Irwin. This project was approved by the Executive Committee, April 2, 1954. It will be useful not only to economists but also to reference librarians.

4. The preparation of a volume of readings on industrial organization was put on the agenda for the next meeting of the Committee on Research and Publications. George Stocking agreed to present a preliminary report on the articles available and to suggest a possible list of readings for publication. The feasibility of this project can then be determined.

5. Consideration was also given to the possibility of publishing a book of readings on economic growth. Max Millikan agreed to prepare a list of possible items for such a volume for the next meeting of the Committee. On the basis of this report the scope and emphasis of the volume could be determined. It was suggested that some of the readings be devoted to the problems of underdeveloped countries. Historical materials were also to be given weight.

The Committee on Research and Publications also considered various proposals for the translation of foreign works, but no action was taken. The Committee continued to believe that translations of the works of Pareto should have highest priority at this time. Gale Johnson agreed to present a report on the status of Pareto translations at the next meeting of the Committee.

The major business before the Committee was the proposal to prepare a cumulative index of the *American Economic Review* and the *Papers and Proceedings* of the American Economic Association. The details of a basic plan for the index and cost estimates therefore were presented by Bernard Haley, who had constructed a sample index based upon four regular numbers of the *Review* (December, 1953, to September, 1954) and the May, 1954, *Papers and Proceedings*. After considerable discussion it was decided to recommend to the Executive Committee that it authorize the compilation and publication of a cumulative classified index of the *American Economic Review* and the *Papers and Proceedings* of the American Economic Association for the period since 1911, the indexing plan to be roughly as follows: (1) index to cover Volume I to XLIV (or XLV depending upon date of completion of the index) of the

Review and *Papers and Proceedings;* (2) the index to follow a subject-and-author classification of articles, communications, and papers in the *Review* and *Papers and Proceedings* from 1911 to date; index of book reviews, excluding index of reviewers, to be included from 1930 to date. The classification of subject matter should be the groups or fields (eighteen classes) currently used in the *Review.*

It was recommended that 5,000 copies of the index be published and offered for sale to members of the Association. The Committee was of the opinion that the index could be sold for $1.00 per copy. The volume would probably exceed 400 pages in length. Preliminary estimates of cost ran from $7,500 to $8,000.

The Committee on Research and Publications also considered an alternative proposal to sponsor the construction of an index of the seven or eight leading English and American periodicals in economics for the last ten or fifteen years. A number of those consulted felt that such an index would be of greater value to the profession than an index of the publications of the American Economic Association alone. The cost of this project would be $10,000 to $12,000, depending on the coverage and the details of the indexing plan. The Committee felt that such a project would require foundation assistance.

Respectfully submitted,

SIMEON E. LELAND, *Acting Chairman*

REPORT OF THE EXPLORATORY COMMITTEE ON THE STATUS OF THE PROFESSION

A year ago Professor Clair Wilcox reported, as chairman, for the *Ad Hoc* Committee on Freedom of Teaching, Research, and Publication in Economics. (See *Papers and Proceedings*, May, 1954, pages 733-737.) This report analyzed the situation carefully and at some length. It first surveyed the Executive Committee's active concern for the preservation of freedom in these spheres—through its resolution of 1947 affirming "the free and untrammeled right" of college and university teachers to select textbooks and related materials in their teaching and research; through the establishment in 1948 of the past-presidents' Committee on Academic Freedom of which Professor Frederick C. Mills was chairman; and through the constitution in April, 1952, upon the discharge at its own request of the Mills committee, of the *ad hoc* committee for which Professor Wilcox reported in December, 1953. It also focused attention upon the major issues involved in this continuing concern of recent years.

The questions raised in the two committees thus established—and particularly in the discussions of the Executive Committee throughout the period—were stated and answered by the Wilcox committee as follows:

1. Should the Association undertake to prepare and publicize a declaration of principles of academic freedom for economists? *We think not.* [No italics in original.]
2. Should the Association undertake to investigate and report upon specific cases involving infringement of the academic freedom of economists? *Again, we think not.* [No italics in original.]
3. What other action, if any, should the Association take with respect to freedom of teaching, research, and publication?

. . . . . .

We believe a report should be prepared each year, and published in the annual volume of Papers and Proceedings, *on the status of the profession, including information on current attacks on freedom and apparent threats to freedom, on the conditions that promote freedom and on those that discourage it. We therefore recommend that the Executive Committee provide for the creation of a standing committee on the status of the profession to prepare such a report.* [No italics in original.]

In light of this recommendation, President Kuznets set up the present Committee, to explore "the need and scope of activity" for a standing committee on the status of the profession. "Before a standing committee is established," he wrote, "we must be sure that there is a useful field of activity to be pursued; and the possibility of pursuing it within the usual limitations of what any committee can reasonably be expected to do."

Some time after this exploratory committee was created, President Kuznets submitted to it a communication which he had received from Dr. Horace B. Davis, of the University of Kansas City, who had been discharged from his position as associate professor, of economics because he refused to answer questions concerning his political views or affiliations. Shortly thereafter, President Kuznets also had submitted to the Committee a letter from a group of French economists and one from a group of Austrian economists, protesting

the prosecution of Paul M. Sweezy for contempt of court, as a result of his refusal to answer certain questions in a New Hampshire investigation of subversive activities, as well as copies of correspondence between Dr. Sweezy and the Managing Editor of the *American Economic Review* with respect to the publication of a notice of this incident. Dr. Davis asserted "that the American Economic Association cannot afford to disregard this affront to one of its members and should take some action." Dr. Sweezy merely requested that the following item be published in the "Notes" section of the *Review:* "Paul M. Sweezy, formerly of the Harvard Economics Department, has been sentenced to jail in New Hampshire for refusing to answer questions, in a state investigation of subversion, regarding a lecture on the subject of socialism which he delivered in a course at the University of New Hampshire." In each instance President Kuznets requested the exploratory committee to "take a look" at the situation and advise him as to "what, if anything, should be done about it."

We will first deal with these specific cases.

With respect to the Davis case, the Committee advised President Kuznets that "there is nothing the American Economic Association can do or should do in the circumstances of this situation." Even if there were an appropriate sphere of activity for the Association in connection with individual discharges involving alleged infringement upon academic freedom and tenure, it seems probable that this would not be such a case, either for investigation or for action. From the material Dr. Davis has himself submitted it appears that he was discharged because of his unwillingness to co-operate with the university authorities and not because of his political views or his reliance upon the Fifth Amendment. In his own words, he was fired "for refusing to answer, not the Congressional Committee, but the Board of Trustees." In the judgment of this Committee, it is altogether reasonable and proper for a university, through its appropriate officers or agencies, to insist that those who constitute its faculty shall deal with it frankly and make available to it all facts and circumstances that may bear upon their status in the academic community. It must be recognized that reciprocal responsibilities govern the situation, and not merely a one-sided obligation unquestioningly to protect the staff member under attack. Only on such a basis can a university operate intelligently and effectively in safeguarding the intellectual freedom we all cherish. It appears that the American Association of University Professors sought to prevent the discharge of Dr. Davis, though without success; that the investigation of the case is being continued; and that Dr. Davis is "preparing a legal test of the University's action." While the final outcome will of course be of interest to economists, there appears to be no adequate ground for special concern on the part of the Association.

The Sweezy case is of a different character. As yet we have made no recommendation to President Kuznets and nothing has been requested of the Association by Dr. Sweezy other than to serve, through the *Review,* as a channel of communication with his colleagues in the profession concerning this experience.

When the incident was brought to the attention of the exploratory commit-

tee, we asked Dr. Sweezy for "the full story," particularly insofar as it might be available in published form. He sent us the August, 1954, issue of *Monthly Review,* which contains an article by Leo Huberman, coeditor of the journal with Sweezy, entitled, "The Daggett-Sweezy Case."

It appears from this account that at a closed hearing, on January 8, 1954, Sweezy answered questions, in an investigation by the State Attorney General of "subversive activities" and "subversive persons" within the state of New Hampshire, to the effect that he "was a Marxist and a socialist," that he "had never been a member of the Communist Party," that he "had never attended any meetings of the Communist Party anywhere," that he "did not, of his own personal knowledge, know any members of the Communist Party in the state of New Hampshire," and that he "did not now advocate, nor had he ever advocated, the overthrow of constitutional government by force and violence"; but that he refused to answer questions "concerning the Progressive Party or people alleged to be connected with it." A second hearing, also closed, was held on June 3, 1954. At this hearing Sweezy again refused to answer questions "concerning the Progressive Party and alleged members thereof"; and he also refused to answer questions "concerning a lecture on the subject of 'Socialism' which he had given at the University of New Hampshire on March 22, 1954."[1] He stated that neither in that lecture nor in any other lecture anywhere did he advocate the overthrow of constitutional government by force and violence; but he declined to answer any further questions concerning the lecture, on the grounds that such questions were beyond the scope of the inquiry authorized by the legislative resolution of 1953 "directing the Attorney General to inquire into violations of the Subversive Activities Act of 1951," and that they constituted "an invasion of constitutional rights of free speech guaranteed by the First Amendment." On June 28, on petition of the Attorney General, a court hearing was held, to compel answers to the disputed questions or to subject the recalcitrant witnesses to citation for contempt of court—"an offense for which the penalty in New Hampshire is normally jail until such time as the defendant choses to purge himself of contempt, which in this case would mean answering the questions." On June 30 the court upheld the position of the Attorney General. Sweezy was found to be in contempt and sentenced to jail; but over the objections of the Attorney General he was admitted to $1,000 bail, pending an appeal of the case to the Supreme Court of New Hampshire.[2]

While the article upon which the above account is based was "intended as a recital of facts and nothing more," and while it may doubtless be deemed

[1] "This was the third year," it was reported, "in which Sweezy had given the lecture on socialism," at the invitation in each instance of Dr. Gwynne Harris Daggett, associate professor of English and a member of the staff of the Humanities Course offered at the University. Professor Daggett had also been questioned at two closed hearings, and he likewise refused to provide information concerning the Progressive Party or the lecture on socialism.

[2] In the Daggett case, there was not only the threat of punishment for contempt of court, but the likelihood of discharge from his teaching post at the University. Accordingly, upon advice of counsel and numerous friends, and after receiving assurance from Sweezy that "the fight would be continued" and that "the case would be carried to the highest courts so that the important constitutional questions involved could be decided," Professor Daggett answered the disputed questions and the case against him was dismissed.

to be accurate and reliable, it is none the less an ex parte statement. Hence a full and properly-balanced knowledge of the facts must await further developments. Sweezy has promised to send to this Committee "printed copies of the record on which the appeal is being taken, and these will include not only the court proceedings but also the transcript of the hearings which went before." This material, together with such decisions of the appellate courts as may follow, will not only disclose all the relevant facts, but should throw revealing light upon the merits of the controversy.

As already noted, however, all that Sweezy requested was the publication of a brief notice of the matter in the *Review*. "I have many friends and acquaintances in the American Economic Association who are perhaps not likely to hear of this incident otherwise," he wrote to Professor Haley, "and I would appreciate your affording me this opportunity of informing them." Professor Haley felt that the problem ought to be attacked, not by broadening the "Notes" section of the *Review*, but through handling by the standing committee on the status of the profession, the creation of which was recommended in the report of the Wilcox committee. Sweezy replied that he did not mean to suggest that the inclusion of such items in the "Notes" "would be an adequate method of dealing with current threats to academic freedom so far as they affect the economics profession"; but he insisted that there remained, in any event, "a plain problem of communication" concerning attacks on economists' civil liberties for which the *Review* obviously afforded the best medium. In these circumstances he took the liberty of suggesting that the question of the publication of such notices be taken up with the Board of Editors as a whole or with such other body as is ultimately responsible for the policy of the *Review*. While Professor Haley readily expressed approval of this suggestion and promised also to bring the matter to the attention of the Executive Committee as an important problem in Association policy, he stated explicitly and incisively some of the difficulties involved in the Sweezy proposal, particularly as they bear upon implied expressions of judgment as to the merits of such controversies.[3] Thereupon Sweezy suggested a process of "continuous cooperation" between the *Review* and the recommended standing committee which would be calculated to eliminate the difficulties envisioned by the Man-

[3] Asserting that he believed it to be "the position of the Association officers that they are not ready or equipped to take a position on *individual* cases involving academic freedom or civil liberties," he continued: "I am sure that you believe that your proposal does not involve the Association in this way, since you regard it as simply designed to meet a 'plain problem of communication' through 'dissemination of factual information.' But is it as simple as this? Do I take the responsibility for publishing every item of this sort involving an economist, in the form written by the complainant, describing from his point of view threats or actions that have the effect in his judgment of impairing his academic freedom or civil liberties? If so, do I attach to these items as published an editorial note disclaiming all responsibility for their factual accuracy? Do I stand ready to provide space also for replies and comments of others who cite different facts or place a different interpretation upon the facts from that given by the complainant? Or do I, or does some other agency of the Association, first conduct some sort of investigation of the accuracy of the facts cited in the complaint? If so, what facilities does the Association have for conducting such factual investigations? And is it not pertinent to point out that the Association would be assuming the responsibilities of the American Association of University Professors?"

aging Editor. He specified that civil liberties cases in which a member of the profession is concerned would be reported to the committee; and the committee would be charged with verifying the facts "and making a brief factual report in the next issue of the *Review* if it concluded that substantial issues were involved." The purpose of such a procedure, wrote Sweezy, "would not be to dispose of civil liberties cases nor to displace the AAUP, but simply to inform the profession"; and with this suggestion Professor Haley found himself in complete agreement.[4]

Since we have been constituted as an exploratory body rather than as an actions committee, the primary significance of these cases lies in their bearing upon the issues presented to us by the report of the Wilcox committee. In the judgment of this exploratory committee, they tend to confirm the soundness and wisdom of the Wilcox recommendations; and the same conclusions were reached by this Committee independently of these specific cases.

Negatively, the Wilcox committee found it inadvisable for the Association either to formulate any general declaration of principles concerning academic freedom of economists or to investigate specific cases of alleged infringement upon the academic freedom of economists. In reaching this conclusion, convincingly supported in its report, the Committee was confirming a like judgment by the earlier Mills committee on academic freedom. In its report to the Executive Committee dated March 22, 1952, it declared "that a policy calling for the active investigation and enforcement of principles of academic freedom is inappropriate and undesirable." In developing the reasons for this opinion, the Mills committee not only called attention to the "exhaustive on-the-ground investigations" of all cases and the "very considerable costs, in money and time," that would be involved, but, like the Wilcox committee, placed emphasis upon the fact that the handling of these matters is the special function of the AAUP. In its own words: "There exists a body created for the central purpose of defending academic freedom. Capable men have worked for many years through the AAUP on the complicated problems of academic freedom. A body of experience has been built up and a national reputation won by the AAUP in this field. Confusion, overlapping of effort, and waste could be expected if each professional society were to follow an independent program in

[4] On the same day on which Sweezy made his final suggestion to Professor Haley, he wrote as follows to the chairman of this exploratory committee: "It occurs to me that your committee may have some trouble connecting this case with the economics profession in a definite enough way to warrant your taking account of it. That would not surprise me, and I am not quite sure what my own answer would be. I sent the small news item, which you saw, to the editor of the *American Economic Review* because I wanted to find some method of communicating with the many people in the profession whom I know and many of whom I think would be interested in the case. I know from experience that this is not easy, some of my closest friends at Harvard when I was there having only just heard of the whole matter and that rather by chance. But whether the fact that a member of the profession—and I trust I qualify as such even though I am not now teaching at a University—is a victim in a civil liberties case which certainly involves academic freedom but is not immediately related to his status as an economist, whether this constitutes ground for concern on the part of the profession as such, I really do not know. I should hope so, if only because I think it would be better for everyone to overextend his interests in civil liberties rather than the reverse, but I am quite aware that my views in matters of this sort are likely to be different from those of others in the profession."

enforcing and protecting academic freedom." The present Committee is unequivocally of the same mind. As its chairman wrote President Kuznets on September 27 in advising him concerning the Davis case: "While I may be anticipating my committee's report of next December, I am constrained to say unhesitatingly that . . . the type of problem involved in the present-day drive against so-called subversives in our educational institutions concerns the academic community as a whole rather than particular disciplines, and that there is no distinctive sphere of activity for our Association as such in these controversies. The American Association of University Professors appears . . . to be the appropriate agency for safeguarding intellectual freedom in our colleges and universities; and if the AAUP is not proving to be an effective instrument for this purpose, it behooves university personnel as individuals, as well as the various professional organizations or learned societies with which they are affiliated, to seek to strengthen this over-all group or to establish a new and adequate over-all group in its place. It would be most unfortunate if the highly controversial concept of academic freedom as it emerges in the contemporary scene were to be defined and applied separately and independently by each of the many academic disciplines which might become involved. It is of the utmost importance that such a situation be avoided."[5]

But the Wilcox committee, while recognizing that the AAUP, with its more than 40,000 members and sizable annual budget, "still retains a substantial measure of prestige," declared it to be "lacking in vigor and effectiveness," and concluded that "the most important step that could be taken to preserve freedom of teaching, research, and publication would be the reinvigoration of the AAUP." The independent confirmation or denial of these judgments would require a more extended process of investigation than this exploratory committee could feasibly undertake. Its chairman did visit the Washington office and conferred with the general secretary. The impression carried away is that the AAUP's inadequate performance is due in no small measure, aside from temporary emergency considerations, to insufficient resources and an absence of sufficient activity on the part of the membership. This means too small a professional staff of full-time workers and too few part-time lay volunteers, in view of the constantly increasing burden of tasks that must be handled. It should be kept in mind, furthermore, that in the present climate of opinion and the prevailing investigatory techniques with respect to so-called "security risks," college and university personnel tend to become involved, not so much in the traditional distinctively-academic disputes, as in what amount in essence to general civil-liberty controversies; and that much of the AAUP's performance assumes the unspectacular form of more or less successful mediatory efforts. In any event, whatever the present shortcomings of the AAUP—and its Committee on Organization and Policy is working assiduously to find ways

[5] With reference to the specific grievance, the chairman wrote: "In the case of Dr. Davis of the University of Kansas City it appears to be entirely clear from his own account that the discharge was not grounded in the fact that he was an economist. There is every reason to believe that the same result would have followed if he had been an astronomer or an archaeologist. The fact that he is an economist, and a member of the AEA, is purely coincidental." In due course this communication to President Kuznets was approved by the other member of the exploratory committee.

and means of improving the situation—such shortcomings would be likely to be much more pronounced in the case of separate professional organizations or learned societies acting independently for the various academic disciplines. For investigations on the merits and for the exertion of pressures designed to safeguard freedom in teaching, research, and publication, we must continue to avail ourselves of the forty-year experience of AAUP, which came into being with the central objective of preserving and promoting academic freedom and whose principles and procedures in this sphere have received widespread approval.[6]

The affirmative recommendation of the Wilcox committee likewise merits full approval. While the tasks of formulating principles and procedures and applying them in individual cases should appropriately continue to be reserved to the AAUP, a broad and important interest none the less remains for the American Economic Association, as for other organizations of academic disciplines, in being kept informed concerning the aggregate experience of the membership of the profession and concerning the developments which tend to hamper or advance its contributions to knowledge, and insight, and fruitful scholarship. In the extraordinary circumstances of the contemporary scene, the information to be provided will tend to center upon "current attacks on freedom and apparent threats to freedom," as well as upon "the conditions that promote freedom and those that discourage it"; but the more general position of the profession is also a matter of legitimate concern for the Association. Hence the Wilcox group recommended that a standing committee on the status of the profession be established. The function of the committee, as a minimum, would be to prepare an annual report dealing with the aforementioned matters, to be published in the regular volume of *Papers and Proceedings* issued each year. Such a report should properly embrace not only developments with respect to college and university personnel but those concerning all members of the profession, whether they hold academic or governmental or business appointments or function independently as individuals. There can be no question, of course, that the Sweezy incident merits inclusion in such a report. But the usefulness of this provision of information would be greatly enhanced if accurate factual knowledge of relevant developments of substantial significance were made available on a more current basis. Hence the plan evolved by Dr. Sweezy and Professor Haley for quarterly publication in the *Review* of statements of facts verified by the standing committee concerning alleged infringements upon the academic freedom or civil liberties of economists fully approves

[6] See AAUP *Bulletin*, Spring, 1954 (Vol. 40, No. 1), which contains not only the 1940 and 1925 Statements of Principles, but the original 1915 Declaration of Principles of Academic Freedom and Tenure. It is interesting to note that at the organizational meeting of AAUP on January 1 and 2, 1915, "it was *voted* that the Association form a Committee on Academic Freedom and Academic Tenure, which should include members of a joint Committee on Academic Freedom and Tenure of the American Economic Association, the American Political Science Association, and the American Sociological Society, which had been constituted in 1913 to study and report on problems of academic freedom and tenure in teaching and research in economics, political science, and sociology." Three past presidents of the American Economic Association—Edwin R. A. Seligman, Richard T. Ely, and Frank A. Fetter—were members of this Committee which drew up the original Declaration of Principles and Professor Seligman was Chairman of the Committee.

itself to this exploratory committee. These quarterly reports would provide the principal materials for the annual surveys; and since the AAUP would constitute the primary source of the information utilized in these reports, the activities of the standing committee would provide a potential base for more general co-operation between the AAUP and the American Economic Association. The standing committee might well serve in an advisory capacity to AAUP investigating committees in such matters as involve the special competence of economists, and it might also exercise leadership in obtaining, when likely to prove helpful, the formal support of the Executive Committee for crucial AAUP actions. Finally, on the basis of its accumulated and continuing experience, it might submit recommendations to the Executive Committee from time to time for altering or expanding its own functions and responsibilities, as well as those of the Association as a whole, in relation to the AAUP and to the rights, duties, and opportunities of economists.

In summary, then, and in terms of the frame of reference established for us by President Kuznets, we find a clear need for the recommended standing committee; and we conclude that its scope of activity should be limited, at least for the time being, primarily to the provision of factual quarterly, if necessary, and annual reports, to be published respectively in the *Review* and in the *Paper and Proceedings*, concerning alleged infringements upon the academic freedom or civil liberties of economists, but to be supplemented by such general observations and recommendations concerning the status of the profession and the Association's relation thereto as might promise to be fruitful.

The thinking of the exploratory committee has been developed into a rather extensive report, in the hope of continuing the stimulus of the general membership of the Association provided in this sphere by the excellent report of the Wilcox committee. Toward the same end, we suggest that for the next annual meeting of the Association, the standing committee on the status of the profession, if established, might very usefully arrange a session on freedom of teaching, research, and publication in economics, so that the membership may be afforded an opportunity to participate actively in a discussion of these crucial problems of academic freedom and civil liberties.

I. L. SHARFMAN, *Chairman*
BEN W. LEWIS

REPORT OF THE *AD HOC* COMMITTEE ON ECONOMICS IN TEACHER EDUCATION

This Committee has had as its special concern the activities (and the problems that have prompted the activities) of the Commission on Economics in Teacher Education. Of necessity, however, the interest of the Committee has extended to other sectors in the broad field of economic education. Members of the Committee, as individuals, have participated in the work of the Joint Council on Economic Education (the parent body with which the Commission on Economics in Teacher Education is affiliated), with the Business Education Committee of the Committee for Economic Development, and in several other activities within this general area.

One significant fact has emerged clearly from our several experiences: economics is "on the march" in the schools, in business, and in adult and general education. In this connection it is getting a measure of attention from professional economists, but we believe that most members of the profession are only slightly aware of the force and character of the movement. There is great strength in the movement and, at least so far as sheer volume is concerned, it will make substantial strides in the years immediately ahead, either with or without the support of our profession. On the score of direction or quality, however, there is reason for uncertainty and even misgiving. Herein, it seems to us, as well as to many who have indicated their concern to us, lies an opportunity for and the responsibility of professional economists. If the level of economic understanding among the American people that results from this movement is lower than we believe to be good and if in the meantime we sit by, indifferent or uncertain and inactive, we have only ourselves to blame.

Members of this Committee are aware of the continuing interest of a sizable segment of the profession in the problems of teaching economics at the college level. We are aware of sporadic professional interest in economics in adult and general education and in the schools. And we are aware, also, of a growing "feeling" that in some way and in some degree the American Economic Association should play a more active and positive role in stimulating and helping to focus the interest and activities of its members in this entire area of economic education. There is diversity of opinion as to the precise part which the Association should undertake, but the belief is substantial and growing that the Association is not now carrying its full responsibility in this area. This matter needs exploration looking toward action.

In light of the foregoing, this Committee recommends that:

1. This Committee be continued as an *ad hoc* committee under the name of the Committee on Economic Education.

2. The Committee be instructed to explore and to bring findings and recommendations to the Executive Committee, if possible at its spring meeting, 1955, on the need for and the character of action properly to be taken by the As-

sociation to stimulate and to help to focus the interest of its members in the area of economic education; that is, the teaching of economics at the college level and in the schools (including teacher training), economics in business education (including "business" in economic education), and economics in adult and general education.

BEN W. LEWIS, *Chairman*
ARCHIBALD M. MCISAAC
PAUL J. STRAYER

REPORT OF OUR REPRESENTATIVE TO THE INTERNATIONAL ECONOMIC ASSOCIATION FOR 1954

During the calendar year 1954, the IEA held, aside from numerous sessions of its various working committees, two general meetings. The Executive Committee held a meeting in Paris, June 10-13, attended by all members, to make short- and long-range plans in view of the Ford Foundation grant extending over five years, from July 1 of this past year. Among other things, plans for an expanded program of round table conferences, with emphasis upon the participation of economists from the less developed countries, resulted from this meeting. The Executive Committee, in view of the growth of the number of member associations and the consequent increase in the size of the Council, decided to reduce the number of each association's delegates to the Council from four to two or from two to one, depending upon the category of the country.

A round table on wage determination was held September 4-13, at Seelisberg, Switzerland, with thirty-five participants from fourteen countries. The program was planned by John T. Dunlop (United States), Jean Marchal (France), and G. Demaria (Italy). A volume containing twenty background papers and a record of the Seelisberg discussions is being prepared and will probably be published, as earlier volumes have been, by Macmillan, London.

During the past year, the International Economic Association has published Volume 4 of the series of articles translated into English and bearing the title, *International Economic Papers*. A fifth volume is being prepared and it is hoped that, as in the case of Volume 4, UNESCO will contribute a subsidy of $1,000. A volume entitled, *Monopoly and Competition and Their Regulation*, edited by E. H. Chamberlin, contains the background papers and a record of the discussion of the round table held on this theme at Talloires (France) in September, 1951. The book was published by Macmillan, London, and is being sold in the US by the St. Martin's Press, New York. Sales of the volume have been good despite the price of $7.50. A condensed version of the papers and discussion of economic progress in the round table on this subject at Santa Margherita during the first week of September, 1953, has been published in UNESCO's *International Social Science Bulletin*, Volume 6, 2 (Summer, 1954). Complete texts of the papers and a fuller account of the sessions are being published in December, 1954, in mimeographed form by the Institut de Recherches Economiques et Sociales, at Louvain, Belgium.

For the more immediate and more distant future, the International Economic Association has plans in various stages of advancement for several round tables and other conferences. For three years, a committee under the chairmanship of Brinley Thomas (Cardiff, Wales; other members, Sauvy and D. Glass) has been perfecting a program on international migration, which is now scheduled for September, 1955, in Western Austria. The meeting will be partly supported financially by the Intergovernmental Committee on European Migration (Geneva). The Association is also planning a round table or seminar on

the economic development of Latin America, to be held under joint auspices with the Economic Commission for Latin America in December of 1956 at Rio de Janeiro. The Association committee—Howard S. Ellis for the Executive Committee, Eugenio Gudin (Brazil), Javier Marquez (Mexico), and Henry Wallich (United States)—has drawn up tentative plans. UNESCO will probably contribute $10,000 to this conference. During the coming summer, a refresher course in economics, planned by the IEA, represented by Austin Robinson, and the Indian Economic Association, represented by C. N. Vakil, will be held at a hill station in northern India. The purpose is to help Indian economists, particularly teachers of advanced students in the universities, to become or remain *au courant* in the recent developments of economics. The Indian government has made a substantial grant to finance the attendance of Indian economists, and the IEA will cover the expenses of four or five economists from Europe and the US who will offer courses. A similar refresher course for economists in the Middle East is being studied by two members of the IEA Executive Committee: Emile James (France) and Xenophon Zolotas (Greece).

Preparations are nearly complete for the First Congress of the International Economic Association, to be held September 4-9, 1956, upon the invitation of the Rector of the University, Professor U. G. Papi, at the University of Rome. Five principal speakers have undertaken to present main papers and these will be discussed by ten economists of international distinction. Announcements of this Congress are now being printed and will be inserted in the issues of national economic association journals appearing in the early months of 1955. The meetings are open to all members of all member associations. The text of main papers will be available at each session in translation into three or four languages, and provision will be made for stimultaneous or sequential translation of discussions. The theme of the Congress will be, "Stability and Progress in the World Economy."

For the more remote future, round tables are contemplated on the philosophical foundations of economics, being prepared by Alfred Stonier and Douglas Hague (University College, London); on the economic implications of the size of nations, preparatory explorations in December, 1954, in Paris by Austin Robinson, Lorie Tarshis, I. Svennilson, and Duquesne de la Vinelle; and on the development of southern Italy.

As far as concerns the relations of the AEA to the IEA, there are at present only two items of business—one rather routine and the other of considerable long-range importance. As for the first, consonant with the reduction of the size of the Council, the AEA should, prior to the Council meeting of 1956, select two of its present four representatives to be alternates rather than members and thereafter appoint only two members. As for the second, the Executive Committee of the IEA requests its constituent member societies to give early consideration to the problem of the finances of the IEA upon the expiration of the Ford Foundation grant. Perhaps a "hump" in the activities of the IEA is inevitable, but too great a decline after four years would be fraught with substantial disadvantages.

Respectfully submitted,

HOWARD S. ELLIS

REPORT OF OUR REPRESENTATIVE TO THE SOCIAL SCIENCE RESEARCH COUNCIL

Two years ago in reporting on the Council's activities I noted that a Rockefeller Foundation capital grant of $1,500,000 had placed the Council's finances on a more stable basis. At the Council's spring meeting this year President Herring reported that since September of 1953 the Council had received additional grants of over a million dollars. These grants are designed primarily to develop individual research and researchers rather than to promote group research projects. In a society in which bigness and organization are almost becoming fetishes, it has perhaps been inevitable that social science research should suffer from what has been aptly called "projectitis." The Council's renewed concern with the development of the research talents of and opportunities for the lone researcher seems particularly commendable. These grants make possible a continuation of the Council's research training fellowships, faculty research fellowships, first year graduate study fellowships, and the grants-in-aid program on a substantially larger scale.

Council activities of the past year of particular interest to economists include the work of the following committees: agricultural economics, business enterprise, census monographs, economic growth, labor market research, mathematical training of social scientists, and Slavic studies.

The Committee on Agricultural Economics reported at the Council's fall meeting that it hoped to complete soon its study of research on areas of persistently low incomes and low productivity, but it reported difficulties in financing its projected handbook on research techniques in agricultural economics. Meanwhile this project is inactive.

The Committee on Business Enterprise research met in February and July. It plans a conference in the fall of 1955 on "Expectations, Uncertainty, and Business Behavior" and a second conference in 1956 on some other aspect of research on the business enterprise. The committee's chairman, Howard Bowen, has completed his monograph on *The Business Enterprise as a Subject for Research* and the Council will publish it sometime in 1955.

Of the twenty monographs which it has projected, the Committee on Census Monographs has sent two to the printer: *American Agriculture, Its Structure and Place in the Economy* and *Income of the American People.* It reports that five others are nearing completion: *Older Population of the United States; Residential Financing; Social Characteristics of Farm, Village, Town and Urban Populations; Foreign Born and Ethnic Population Groups; Child and Youth Population.* Eleven monographs dealing with various aspects of the housing problem, occupational trends, structure of the labor force, internal migration, differential reproduction, and related topics are in a less advanced stage of preparation, while two have not yet been assigned.

The Committee on Economic Growth, under Simon Kuznets' chairmanship, is bringing to completion the program it projected some two years ago. The

Duke University Press is publishing the papers presented at the 1952 conference on economic growth in Brazil, India, and Japan. The Committee conducted four conferences during the past year, dealing respectively with strategic factors in periods of rapid economic growth, cities and their role in economic growth, entrepreneurship and economic growth, and economic criteria in investment plans for development. The Committee has arranged with economists in England, Germany, Italy, and Sweden to appraise estimates on national income and wealth extending back over a century or more and to report on their findings within a year after beginning the project. The Committee reports: that the International Association for Research on Income and Wealth, with headquarters in Italy, will publish the several papers on economic growth presented at the Association's 1953 meeting; that the Committee's chairman presented a paper entitled, "Toward a Theory of Economic Growth," at Columbia University's third bicentennial conference in May; and that at the invitation of the population division of the United Nations the chairman participated in a conference on plans for further research on the economic and social implications of population trends.

The work of the Committee on Labor Market Research is nearing completion. In June the Council published the Committee's final report on its study of labor mobility under the title, *Labor Mobility in Six Cities; A Report on the Survey of Patterns and Factors in Labor Mobility, 1940-50.* The Committee has published a second volume entitled *Labor Mobility and Economic Opportunity,* under the imprint of the Massachusetts Institute of Technology and John Wiley & Sons and a third volume on *Research on Labor Mobility; An Appraisal of Research and Findings in the United States,* authored by Herbert Parnes and appearing as Council Bulletin 65. After tentatively canvassing other directions in which research might be desirable, the Committee proposed that it be continued into the coming year to formulate its recommendations to the Council.

After obtaining reports from three-fourths of the participants in the 1953 Hanover institute on training in mathematics evaluating their experience, the Committee on Mathematical Training of Social Scientists has arranged for two institutes to be held simultaneously in the summer of 1955—one at the University of Michigan and one at Stanford. The Committee has also inaugurated three projects for preparing teaching materials for certain social science fields. Two are of particular interest to economists. Harold W. Kuhn is preparing problems and exercises in the theory of games and linear programing and Robert R. Bush is preparing similar materials in other areas of economics.

The Committee on Slavic Studies reports that the Rockefeller Foundation will meet one-half the cost of financing for the next five years the Committee-sponsored *Current Digest of the Soviet Press* and the Committee is taking steps to obtain the additional money needed to finance the work. In May, 1954, the Committee sponsored a conference at Arden House on "Continuity and Change in Russian and Soviet Thought," and the twenty-six papers presented at the conference are being edited for publication.

In 1947, at the request of the American Economic Association the Council helped in financing the translation of August Lösch's *Die räumliche Ordnung*

der Wirtschaft. After some delay occasioned by problems raised by the Alien Property Custodian, the work has been completed and the Yale University Press has published the translation.

In terminating my six years membership on the Council's Board of Directors, the last year as chairman, I wish to express appreciation to the Association and the Council for a stimulating and interesting experience.

Respectfully submitted,

GEORGE W. STOCKING

REPORT OF OUR REPRESENTATIVE TO THE AMERICAN COUNCIL OF LEARNED SOCIETIES

The main duty of the delegate is to attend the annual meeting, which was held in Rye, New York, in January, 1954. He also reads the minutes of all meetings of the Board of Directors. The main work of the Council must be learned from its published *Proceedings*. The shortened Council meetings of recent years are largely taken up with the routine of reports from the Executive Director (now Dr. Mortimer Graves) and other officers and staff of the Washington headquarters, of some of the committees through which the Council chiefly works, the election of officers, etc. Available time is devoted to discussion of general or timely topics of concern to the interest of scholarship in the country, hence to those who are chosen to speak for this interest, as organized in professional societies in the humanities and the social sciences.

For reasons that will be obvious in a general way, the organization has of late been increasingly concerned with relations between scholarly work and the government. There are matters of freedom of international movement of both materials and individuals; also the interruption of promising careers by draft for military service. Here the authorities have shown a disinclination to consider scholars in the fields represented as of special importance, particularly in comparison with workers in natural science. Further, the federal government is responsible for important scholarly activities and institutions. In January, 1954, the appointment of a Librarian of Congress was impending and reports indicated that considerations other than the best possible conduct of this vital agency were being pushed to influence the selection. The question of how far and how the Council or its twenty-four constituent societies should try to affect governmental action in such matters gave rise to some lively discussion, ending with the action that seemed most likely to receive consideration. An excellent statement in answer to the charges made against tax-exempt foundations by the Reece committee was prepared and submitted by Messrs. Graves and DeKiewiet.

At the present juncture, the matter that is naturally of most acute concern in ACLS circles is that the Council's existence is in jeopardy for lack of financial support. The occasion is that the foundations, which have long supplied the bulk of the "overhead" financing, the support of the central office and staff, have announced early discontinuance of grants for this purpose. At the meeting there was considerable discussion of the functions of the Council and methods of making the need for it appreciated by the foundations or other possible sources of support. From the AEA point of view, it is to be noted that the social science society constituents of the ACLS have been somewhat out of the main focus of its activities since the organization of the SSRC. This was not in prospect when the ACLS was set up, immediately after World War I, to represent this country in international relations of scholarship and re-

search in fields other than natural science. Your delegate would, however, express the conviction that the work of the ACLS and representation in it of the social sciences, alongside the more distinctly humanistic groups, is both of national importance and of substantial value to the former associations and their membership. If so, the AEA should co-operate in any suitable way in securing provision for the Council to continue with adequate support for effective work on at least the scale of recent years. I have no specific recommendation to offer in that connection at this time. The matter will undoubtedly receive the earnest consideration of our Executive Committee whenever there is any occasion for action.

Respectfully submitted,

FRANK H. KNIGHT

WORK OF THE NATIONAL BUREAU OF ECONOMIC RESEARCH IN 1954

Thirteen reports resulting from the National Bureau's work were published in 1954. On December 31 eleven were in press. Three were approved and about ready to go to press, and seven were being reviewed, or were soon to be reviewed, by the Board of Directors. A number of others were in an advanced stage of preparation and work on several new projects was under way and advancing effectively.

New Studies Started and Planned

New research in the field of international economic relations developed still further in 1954. A study of the structure of world trade and payments, the possibilities of which Herbert B. Woolley explored in 1952-53, was developed in 1954 with the assistance of a grant from the Ford Foundation. Earl Rolph undertook a brief exploratory study of change in the public debt of various countries since 1914, bringing into consideration related changes in price levels and currency valuation.

The review of what is known of the economic growth of Soviet Russia, started in the autumn of 1953 with the aid of a grant from the Rockefeller Foundation, was developed rapidly in 1954. It is being directed by Raymond W. Goldsmith and concerned primarily with the question: What has been the growth of Soviet Russia's output over the past three decades?

With the assistance of a grant from the Alfred P. Sloan Foundation, work was started late in 1953 and continued actively in 1954 on a project that will render an account of the course of money and real wages in the United States from the Civil War to the present day and of the changes at least since the turn of the century in the relations between the volume of output on the one hand and the quantities of labor and capital employed in production on the other. Leo Wolman is in charge of the wage study and John W. Kendrick is directing the work on productivity.

With a grant from the National Science Foundation, a study dealing with methods of determining the demand for and supply of scientific and technical personnel was started in 1954 by George J. Stigler.

Upon the suggestion of the Council of Economic Advisers—and in view of the widespread interest in the applicability of the National Bureau's business cycle investigations to the problem of forecasting—new studies in this area have been started under the direction of Geoffrey H. Moore. These studies are reviewing the possibilities and limitations of statistical indicators of cyclical swings, the measurement and interpretation of indexes of scope or diffusion of business fluctuations throughout the economy, and the usefulness for forecasting purposes of data on businessmen's expectations.

Ilse Mintz has begun an investigation of cycles in foreign trade of the United States; and Millard Hastay has begun a study dealing with "long cy-

cles" in economic activity for the purpose of throwing further light on the character and extent of long swings in our economy.

A study of postwar capital markets was planned in 1954 and a grant for its conduct was authorized by the Life Insurance Association of America. It is expected to be organized and operations begun in 1955.

Research associates appointed for 1954-55 and the fields in which they have started and currently have projects under way, are:

1. George Borts, Business Cycles—Regional Fluctuations
2. Michael Gort, Capital Formation and Financing
3. George S. Brinegar, Agricultural Finance

Universities-National Bureau Committee for Economic Research

The Universities-National Bureau Committee for Economic Research, which had been enlarged in 1950 to include twenty-eight universities offering graduate work in economics and emphasizing research, completed its first four years on June 30, 1954, and has now been continued for the period July 1, 1954-June 30, 1958.

Two Special Conferences, the fourth and fifth under the sponsorship of the Committee, were held in 1953. One of the two was on "Policies to Combat Depression," held at Princeton University in October. The second was on "Capital Formation and Economic Growth," held in November at the Carnegie Endowment International Center in New York.

The sixth Special Conference was held at Princeton University in May, 1954, and was devoted to further discussion of the same subject as the fourth; namely, "Policies to Combat Depression."

The seventh Special Conference was held in September, 1954, and was devoted to "Measurement and Behavior of Unemployment." Plans have been completed for the eighth conference, to be devoted to consumption economics, which will be held in 1955. A topic in international economics is proposed as the subject for the ninth Special Conference, to be held in 1956, and steps are now being taken to prepare plans for it.

The 1954 Annual Meeting of the Conference on Research in Income and Wealth was held in October of that year and was devoted to discussion of the comparability of national accounts. The 1955 Annual Meeting will be devoted to a review of the existing national income statistics of the United States.

Papers presented at the two Special Conferences on Policies to Combat Depression, held in October, 1953, and May, 1954, at the Conference in November, 1953, on "Capital Formation and Economic Growth," and those on capital formation presented at the 1953 Annual Meeting of the Conference on Research in Income and Wealth, are now being edited and prepared for publication.

Relocation of Offices

On May 1, 1954, the National Bureau moved its offices from 1819 Broadway and from "Hillside" to 261 Madison Avenue, New York City. The decision to move became necessary when the decision of the City of New York to build

the projected Coliseum at Columbus Circle and to demolish the building in which the National Bureau's offices were located became final late in 1953.

Directors, Officers, and Staff

C. Canby Balderston, elected Chairman of the Board of Directors at the 1954 Annual Meeting, resigned in August as Director by Appointment of the University of Pennsylvania and as Chairman of the Board upon his appointment as a member of the Board of Governors of the Federal Reserve System. Percival F. Brundage, elected President at the 1954 Annual Meeting, resigned in May as President upon his appointment as Deputy Director of the Bureau of the Budget. Harry Scherman was elected President to fill the unexpired term by resignation of Mr. Brundage.

John H. Williams was elected Director by Appointment of the American Economic Association to fill the unexpired term of Donald H. Wallace who died September 19, 1953. At the 1954 meeting of the Board, Melvin G. de Chazeau was elected Director by Appointment of Cornell University to succeed Paul M. O'Leary whose term of office had expired, and Solomon Fabricant was elected a Director at Large. C. Reinold Noyes, Director at Large, resigned as a member of the Board on June 30, 1954.

Solomon Fabricant, who had served as Acting Director of Research in 1953, was appointed Director of Research on March 1, 1954, to succeed Arthur F. Burns, who went on leave of absence on March 19, 1953, to serve as Chairman of President Eisenhower's Council of Economic Advisers.

George H. Borts, of Brown University, George Brinegar, of the University of Connecticut, and Michael Gort, of the University of California, were appointed research associates for 1954 and 1955.

Members of the Association may make suggestions relative to the work of the National Bureau either directly or through the member of the Board of the National Bureau by appointment of the Association.

PUBLICATIONS

OF THE

AMERICAN ECONOMIC ASSOCIATION

1955

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Volume XXV, 1935

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Volume XXIX, 1939

The American Economic Review, **March, **June, September, and December; each, $1.25

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Papers and Proceedings. Economic Theory of Imperfect Competition, Oligopoly, and Monopoly; Role of Monopoly in the Colonial Trade and Expansion of Europe; Progress of Concentration in Industry; Does Large-Scale Enterprise Result in Lower Costs; Sherman Act and the Enforcement of Competition; Patent Policy; A Consideration of the Economic and Monetary Theories of J. M. Keynes; Keynesian Economics: The Propensity to Consume and the Multiplier, and Savings, Investment, and Wage Rates; Economics Collides with Ethics; An Appraisal of the Taft-Hartley Act; Fiscal Policy in Prosperity and Depression; Problems of Timing and Administering Fiscal Policy in Prosperity and Depression; Transportation and Public Utilities; Futility of Trust-Busting; National Productivity. Pp. 591. 1.50

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